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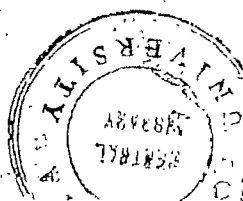
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IN THIS ISSUE

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Davis, K. 1963*a*. "Social Demography." Pp. 124-37 in *The Behavioral Sciences Today*, edited by Bernard Berelson. New York: Basic.

———. 1963*b*. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29: 345-66.

Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32: 5-19.

Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt, ed. 1962. *The American College*. New York: Wiley.

Weber, Max. (1921) 1968. "Society's Problems." Pp. 12-16 in *Economy and Society*, edited by Guenther Roth and Claus Wittich. New York: Bedminster.

Talcott Parsons and the Theory of Action. II. The Continuity of the Development¹

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In *The Structure of Social Action*, Talcott Parsons developed a theory of action which corresponds in structure and method to Kant's critical philosophy. The core of this theory is the assertion that every action is to be understood as a product of the interaction of dynamizing and controlling forces. This basic proposition is elaborated into a theory through the construction of a four-dimensional "action space," which can be further differentiated into subsystems possessing varying degrees of orderedness. The differential effect of dynamizing and ordering forces on any given action is determined by its location in the action space and by the kind of relation obtaining between the subsystems of the action space. The relation between these subsystems which enables them simultaneously to expand their areas of effectiveness is *interpenetration*. The entire development of Talcott Parsons's theory of action is a progressive refinement of the theoretical devices available for the analysis of such processes of interpenetration. This is demonstrated in this essay for all the various stages of Parsons's theoretical development, from the laying down of the theoretical core in *The Structure of Social Action* (1937) through *The Social System* (1951) to *Action Theory and the Human Condition* (1978).

Thus conceived, a social system is only one of three aspects of the structuring of a completely concrete system of social action. The other two are the personality systems of the individual actors and the cultural system which is built into their action. Each of the three must be considered to be an independent focus of the organization of the elements of the action system in the sense that no one of them is theoretically reducible to terms of one or a combination of the other two. Each is indispensable to the other two in the sense that without personalities and culture there would be no social system and so

¹ German version of this essay appeared in *Soziale Welt* (Winter 1980). I would like to thank Paul J. Gudel of the Division of Comparative Studies in the Humanities, Ohio State University, for his competent and careful translation. The English version contains a few small changes and additions for the sake of clarification of the argument. This essay is the second part of a two-part study of Parsons's work (pt. 1 is Münch 1981). Requests for reprints should be sent to Richard Münch, Sozialwissenschaftliches Institut, Universität Düsseldorf, Gebäude 23.31, Universitätsstrasse 1, 4000 Düsseldorf 1, West Germany.

on around the roster of logical possibilities. But this interdependence and interpenetration is a very different matter from reducibility, which would mean that the important properties and processes of one class of system could be theoretically *derived* from our theoretical knowledge of one or both of the other two. [PARSONS 1951, p. 6]

This view casts serious doubt on the allegation that functional analysis of social systems implies an imperative of conformity. We must keep in mind that we are here speaking of the general action level, and that the term "moral" refers to any culturally grounded justifications of selection among alternatives for action, whether they be for sustaining a given social system, or for its revolutionary overturn, or for any of the wide variety of withdrawing or otherwise deviant modes of association. [PARSONS 1977*i*, p. 253]

1. INTRODUCTION

In *The Structure of Social Action* ([1937] 1968*b*), Talcott Parsons articulated a theory of action the fundamental strategy of which corresponds to that of Kant's critical philosophy and the development of which must be understood as a systematic elaboration of this basic Kantian perspective. The hallmark of this perspective is the assumption that every action is to be understood as the product of a certain relation between analytically differentiable spheres or subsystems. These subsystems of human action are of two basic kinds: those with a regulative function and those with a dynamizing function. The paradigm of this distinction is found in the distinctions between the categories of the understanding and sense perception, between the categorical imperative and practical judgments, and between aesthetic categories and aesthetic sensation which we find in the critical philosophy of Kant ([1781] 1956, [1797] 1967, [1799] 1968). It is only through the interpenetration of these spheres that cognitive, moral, or aesthetic experience can constitute itself. However, interpenetration is only one of many possible relations which may obtain between analytically differentiable subsystems of action. Between these subsystems there exist fundamental tensions. Max Weber's typology of the relations between religious ethics and the world provides us with a ready set of categories for thinking about the various ways in which these can be eased: the accommodation of the potentially regulative subsystem to the dynamic subsystem, their reconciliation, their mutual isolation, their interpenetration, the one-sided constriction of the potentially dynamizing subsystem by the regulative subsystem (Weber [1922] 1976, pp. 314-81; Parsons 1967*e*; Münch 1980*b*, 1981). Interpenetration, however, is that form of relation through which opposed spheres or subsystems can both expand without thereby creating mutual interference. Interpenetration is the mechanism by which the poten-

tial of every system is converted into actuality; it is the mechanism of self-realization and evolution.

Closely connected with this theoretical core of Parsons's action theory is his voluntaristic solution to the problem of order, particularly as this problem manifests itself in social order. As soon as we recognize that human action is intentional action, we can no longer understand it as completely causally determined by dynamic factors. On this plane of intentional action, order is possible only if the actors share common values, which in principle they acknowledge of their own free will. This recognition of common values is itself possible only if the individuals are not acting within a framework of purely instrumental rationality, but are able to take an increasingly universal standpoint of collective solidarity which transcends the solidarity of particular groups. Shared values must be anchored in this collective solidarity. Furthermore, the recognition of more specialized norms pertaining to human action is dependent on their being rationally justifiable through their subsumption under the common values. Finally, this normative framework is necessary in order to give form to the dynamic spheres. On the other hand, when we grant to these dynamic spheres an autonomous efficacy or power, we can no longer understand the order of action as depending solely on a one-sided constriction of the dynamic spheres through the imposition of a preexistent normative framework. We must understand it as dependent on the interpenetration of the normative framework and the dynamic spheres, which relate to one another as form and matter. A one-sided restraining relationship would not allow us to account for the autonomy of individual action. Such a relationship would correspond to Durkheim's conception of mechanical solidarity, within which the concept of the individual has no place. The combination of the autonomy of the actions of individuals with social order is possible only as the interpenetration of a normative frame of reference and the dynamic spheres of action, which include the wants and needs of individuals.² "Interpenetration" is in this sense a generalization from the normative idea of the coexistence of the actions of autonomous individuals and social order which is, in the framework of action theory, the central idea of modernity. The values on which the theory of action as constitutive of a field of fundamental inquiry is based are therefore rooted in the normative idea of the modern.

The core of the theory of action is a set of premises which have the character of postulates and not simply (as is so often asserted) a taxonomy (Schütte 1977). In contrast to the positivistic and idealistic theories of

² Jeffrey Alexander (1978) calls the thesis that intentional action is not purely causally determined "formal voluntarism," which he distinguishes from "substantive voluntarism," his term for the content of Parsons's theory regarding the conditions under which the human potential for autonomy can be realized in action. Alexander sees this substantive voluntarism as embodied primarily in a theory of increasing differentiation.

action, the voluntaristic theory allows for the cumulative progress of inquiry in the manner of the sciences, as postulated in Karl Popper's model (Popper 1963, 1972; cf. Lakatos and Musgrave 1970; Münch 1973, pp. 178-212). What is true in positivistic and idealistic theories is preserved in voluntaristic action theory, while their errors are eliminated and new areas for the application of explanatory principles are opened up.

If we look at Parsons's integration of Durkheim, Weber, and Freud into the theory of action from this perspective, we see how much material of potentially constructive value for systematic sociology Parsons has made accessible in the work of these classic authors (Parsons 1953, 1964*a*, 1964*c*, 1967*b*, 1967*e*, [1937] 1968*b*, 1978*b*). And it is only against this background that we can understand the latent structure of Parsons's development of his own theory. This development should be understood as a progressive amplification of the Kantian core which is first set out in *The Structure of Social Action*. This is true of the analysis of institutionalization and internalization as interpenetrations of cultural system, social system, and personality, and of the introduction of the pattern variables in *The Social System* (Parsons 1951), *Toward a General Theory of Action* (Parsons and Shils 1951*a*), and *Family, Socialization, and Interaction Process* (Parsons and Bales 1956*b*); it is true of the differentiation of the sub-systems of action and the analyses using multiple "system references" with the help of the microscopic-macroscopic applications of the four-function paradigm in *Working Papers in the Theory of Action* (Parsons, Bales, and Shils 1953*b*); it is true of the introduction of the cybernetic hierarchy of controls; and it is true, finally, of the theory of the generalized media of interchange, which Parsons first developed for the social system in *Economy and Society* (Parsons and Smelser 1956) and then extended to the system of action generally in *The American University* (Parsons and Platt 1973) and to the system of the human condition in "A Paradigm of the Human Condition" (Parsons 1978*d*). This unitary development represents a systematic expansion of the scope of the theory, from the social system to the general system of action to the system of the human condition. Characteristic of this development is its continuity in relation to the core which is present in *The Structure of Social Action*, that is, the Kantian theory of interpenetration. It is this continuity which will be demonstrated here in detail.⁸

The thesis of the "Kantian core" and the "continuity of the develop-

⁸ In his review of the volume of critical essays on Parsons edited by Max Black (1961), Philip Selznick wrote "Does Parsons have a distinctive perspective, a 'philosophy of sociology'? I think not. His writing has become so diffuse and eclectic in recent years that it is really difficult to identify him with a special point of view" (Selznick 1961, p. 934). On the other hand, Schwanenberg (1971), Gerstein (1975), and Alexander (1978) accentuate the continuity in Parsons's work (cf. Parsons 1961*b*, 1977*a*, 1977*e*, 1977*g*, 1977*i*).

ment" relates to the latent structure (the deep structure) of the theory of action and is independent of both Parsons's own conscious attempts to describe the latent structure and the inconsistencies and discrepancies of the theory's surface structure. If we accept for the moment Karl Popper's distinction among a "first world" of material objects, a "second world" of subjective consciousness, and a "third world" of objective symbol structures (1972, chap. 4), we can say that the theory put forth here concerns the third world of objective symbol structures rather than the second world of subjective awareness.⁴ My purpose here has been to reconstruct the core of the theory in such a way as to permit the greatest possible integration of the various stages of Parsons's theory into a coherent system, so that Parsons might seem to manifest progress instead of simply change. Even this task has been in the service of a broader goal, which is to make the strongest case possible for Parsons's theory in the face of its critics and to show as clearly as possible the great variety of the theory's possible applications.

2. THE INTERPENETRATION OF CULTURAL SYSTEM, SOCIAL SYSTEM, AND PERSONALITY: *THE SOCIAL SYSTEM, TOWARD A GENERAL THEORY OF ACTION*

In *The Structure of Social Action* ([1937] 1968b), Parsons differentiated the system of action into only two spheres: the sphere of instrumental ("goal-rational") action, in which there exist only hypothetical imperatives, and the sphere of categorical-normative obligation. In *The Social System* (Parsons 1951), this two-term system of action was replaced by a three-term schema which begins with the differentiation of the cultural system, the social system, and the personality system. The analysis now explicitly revolves around the interpenetration of these three systems of action. Every concrete action unit—an actor in a situation—is to be explained as the product of a specific, multifaceted relationship among these three subsystems and a specific degree of interpenetration among them. In order to characterize the particular nature of the relation among these subsystems, Parsons turned away from the concept of interdependence and toward the concept of interpenetration, as may be seen in the quotation used as the first epigraph to this essay.⁵ And in Parsons and Shils's contemporaneous

⁴ Parsons himself, in his late phase, saw himself not as creating the Kantian core in his own theory, but as discovering it as something already there (Parsons 1978d). The same is true of his epistemology, e.g., his concept of empirical fact: "I had adapted this from L. J. Henderson, but it was basically Kantian" (Parsons 1978a, p. 1354). Cf. my similar interpretation (Münch 1981).

⁵ The root of the concept of interpenetration in the concept of the interdependence of normative and conditional action in *The Structure of Social Action*, and its systematic employment since *The Social System*, are not acknowledged anywhere in the discussion between Luhmann (1977, 1978) and Jensen (1978).

"Values, Motives, and Systems of Action" (1951*b*), their contribution to *Toward a General Theory of Action* (1951*a*), interpenetration is explained as a specific kind of relationship between analytically differentiated subsystems:

A third basis of their intimate relation to each other is the fact that they *interpenetrate* in the sense that no personality system can exist without *participation* in a social system, by which we mean the integration of *part* of the actor's system of action as *part* of the social system. Conversely, there is no social system which is not from one point of view a mode of the integration of parts of the systems of action which constitute the personalities of the members. When we use the term *homology* to refer to certain formal identities between personalities and social systems which are to be understood in terms of the above considerations, it should be clear that we in no way intend to convey the impression that a personality is a microcosm of a social system, or that a social system is a kind of macrocosmic personality. [Parsons and Shils 1951*b*, p. 109]

The interpenetrating subsystems possess different qualities, but they are "homologues" of one another. This means that elements of one system can become elements of the other. In the zone of interpenetration thus created, a new quality appears, which forms a bridge between the previously separate subsystems of action. Each individual action is now seen as a product of the interpenetration of these subsystems. The reciprocal penetration of instrumental action and normatively obligated action requires the *institutionalization* of normative culture in the social system and the *internalization* of normative culture in the personality system (Parsons 1951, pp. 36-45, 201-48; Parsons and Shils 1951*b*, pp. 146-58, 176-83, 202-4).

A normative culture as a general system of values—for example, the fusion of individualism, universalism, rationalism, and activism which characterizes modern society—can be understood as a symbolic code, which undergoes continuous modification through the course of its history by means of its interpenetration with the social and personality systems. Its function is to provide for the possibility of variation while preserving the specific cluster of values which characterizes the action system (pattern consistency). This cluster or pattern of values must therefore be sufficiently generalized to permit a given type of action system to adjust to new conditions without having to alter its fundamental structure. The scope for variation which this code grants to action is therefore too broad to guarantee the orderedness of social interaction in any given situation. As a result, social order cannot be conceived of as simply the consequence of the existence of a normative culture. The culture must be institutionalized in a social system, and this means that it must be specified as a set of binding obligations.

In a social system, normative culture is fleshed out, given a concrete

content of meanings. Correspondingly, other possible contents are excluded. The hallmark of a social system is the obligation imposed on the members of the society to accept a certain variant of the basic cultural pattern. This variant, and the obligation to accept it, must attain to a certain degree of self-evidentness, even though there may be other possible variants of the cultural pattern. This self-evidentness can come about only through an isolation of the members of the social system from the outside and an intensification of their solidarity with one another, so that deviations from the cultural pattern become violations of the obligations of group solidarity. The interpenetration between social systems and an overarching cultural system makes possible an increase of social solidarity within each individual social system and an increase in the solidarity between the systems. These two increases do not exclude one another in this case, but instead depend on one another. Whenever the specific normative ideals of a social system are projected into and embedded in the cultural system, they must be sufficiently generalized so that they do not come into conflict with the normative demands of other social systems. Conversely, through this interpenetration cultural values are sufficiently specified in the form of practical rules so as to make possible the identification of the members of diverse social groups with the same cultural pattern. In this way, the cultural pattern is interpreted so that it meets not only the rarefied "contemplative" needs of an intellectual elite but also the practical, more mundane needs of various other social groups for ethical regulation. The result is necessarily a greater overlap, on the level of the social organization of this interpenetration, of the subsystem of practical problem solving with the subsystem in which the interpretation of the cluster of cultural values is paramount, such as the systems of higher education and scholarship, religious congregations, and other cultural institutions.

Social order, however, requires not only the capacity of the cultural pattern to maintain its basic nature through variation and the institutionalization of a concretization of the cultural pattern through obligations imposed on the actors by their mutual solidarity. Order must also be compatible with the capacity of the actors for autonomous action. Here, too, "interpenetration" is Parsons's answer. Its specific form is the internalization of normative culture in the personality through the process of socialization. After reading Freud, Parsons approached this range of problems from the standpoint of Freudian psychoanalytic theory, but he simultaneously preserved the contributions of Durkheim and Weber to the theory of socialization (Parsons [1937] 1968b):

In my reading of Freud, I gradually realized the importance of what I and others came to call the phenomenon of "internalization" (Freud's own term was "introjection")—both of sociocultural norms and of the personalities of the others with whom an individual has been interacting, above

all a "socializing agent" (the latter case sometimes being called "identification").

This idea first emerged clearly in Freud's thought in the conception of the superego, though one may say it was present from an early stage, especially in the conception of transference (for example, the treatment of the analyst as if he were the analysand's father). Freud came to consider moral standards, particularly as implemented by the father, integral parts of the child's personality, through some phases of the learning process. Gradually the scope of this aspect of Freud's theory of "object relations" widened in the course of his later work to include not only the superego but also the ego and even the id. At about the same time, it became clear to me that a very similar conception, developed from a quite different point of view, was essential to Durkheim, especially in his conception of social control through moral authority. The idea was also at least implicit in Weber's treatment of the role of religious values in determining behavior, and appeared with great clarity in the work of a group of American social psychologists, notably G. H. Mead and W. I. Thomas. The conception of the internalization, in successive series, of sets of cultural norms and concrete social objects has become a major axis of the whole theory of socialization, figuring in new forms even in the most recent treatments of the problems of higher education. [Parsons 1977a, pp. 37-38]

This way of looking at the process of socialization made its first appearance in the analysis of the interpenetration of the cultural system, the social system, and the personality system in *The Social System*. It was further expanded in *Family, Socialization, and Interaction Process* (along with the inclusion of the behavioral system [behavioral organism] as a fourth system of action), where it is explicitly connected with Freud (Freud 1972; Parsons 1951, chaps. 6 and 7, 1953, 1956a, 1956b, 1964c; Parsons and Olds 1956).

The internalization of normative culture is a process in which the process of institutionalization is reversed. In the process of socialization the individual is bound first of all to a given specification of the normative culture—and, of course, also to the members of a concrete family. A child is bound to his family by very strong ties of love and respect, and it is through these ties that he is bound—through increasingly specific reattachments of regard or disregard for desirable or undesirable ways of acting—to an acceptable variant of the normative culture. In the extreme case, in which a child's total capacity for love and regard was occupied by the parents and in which the family was isolated from the larger community, we might see a complete identification of the actor with his family role. The individual would in that case be incapable of concretizing the normative culture independently, in situations outside the family. The less the internalization of the normative culture transcends the limits of the family and kinship relations, the less will it generally be possible for the actor to generalize the normative culture sufficiently to permit him to respecify it in different social

systems. The process of socialization therefore must lead its recipient beyond the family to continually more comprehensive specifications of the normative culture, and it must continually move him beyond each set of solidarity relations that he establishes. The interpenetration of the various solidarity relations and the specifications of the normative culture which are connected with them is the most important factor in the establishment of a common basis for the identification of different normative systems with one and the same fundamental pattern.

One mechanism of the interpenetration of normative systems and solidarity relations is the integration of emotional and instrumental guidance in the family. Sometimes these functions are divided between the mother and the father. In such cases, the division of labor must be accompanied by social solidarity between the parents. The child's capacity for identification with the normative orientation of the parents is created only through his emotional ties to them, while it is through their demand that the child behave in accordance with norms which are external to the family that the child is able to transfer this identification to the normative system beyond the family (Parsons 1956a, pp. 77-94). This same two-sidedness, of an emotional attachment of the individual to a group which then subjects that individual to normative demands which originate outside the group, must be exhibited in the actor's relation to all the groups which succeed the family as participants in the process of socialization: peer groups, school classes, professional groups, occupational groups, religious congregations, neighborhood associations, etc. (Parsons 1964b, 1964c, 1964d, 1964e; Parsons and Platt 1972, 1973, pp. 163-224). In the course of this socialization the individual has continually opened up for him new worlds within the world of a normative culture, and his attachment to the normative culture is generalized and freed from the tyranny of concrete specifications (Parsons and Bales 1956a; Parsons and Platt 1973, pp. 163-224). The whole process of socialization, therefore, is a process in which the individual, *through* the internalization of the normative culture, attains to increasing autonomy of action, but it is also a process in which the bond between the individual and the normative culture is strengthened, because this bond comes to depend less and less on any particular relations of the individual.⁶ The interpenetration of different socializing entities in this way makes possible a form of interpenetration of cultural system, social system, and per-

⁶ In this fundamental postulate of the theory of socialization we see already a convergence of the theoretical positions of Simmel ([1908] 1968), Durkheim ([1924] 1953), Mead (1934), and Piaget ([1932] 1965). The positions of these authors are preserved in Parsons's theory of socialization. It is astonishing how widespread is the opinion that Parsons's theory of socialization integrates individual and society by subjecting the individual completely to social norms (see, e.g., Wrong 1961; Wilson 1970; Reichwein 1970; Brandenburg 1971; Krappmann 1971; Morrione 1975). For commentary on this interpretation of Parsons, see Parsons (1962, 1974), Turner and Beeghley (1974), Geissler (1979).

sonality which permits an increase in the inherent qualities of each. The normative culture is made more consistent through the process of generalization, while its institutionalization in various social systems simultaneously becomes more specific; the bonds between the individual and these specifications become tighter, while the autonomy of the personality simultaneously is increased. Conversely, the less pronounced the interpenetration is, the less consistent the normative culture appears, the less is social interaction governed by connections between individuals and the normative culture beyond those which were formed in the context of very particular social groups, and the fewer possibilities there are for personal individuality.

Personalities and social systems are, according to the implications of this view, not directly homologous; they are differently organized about different foci of integration and have different relations to the sources of motivational energy. But they are more than merely "analogous," they are literally "made of the same stuff"; as we have so often put it, they are not merely interdependent, they *interpenetrate*. Above all, it is important that the *focus of organization* of both types of system lies in certain aspects of the relevant culture patterns, namely the value-systems which have been institutionalized in the social system, internalized in the personality system. Moreover, these are not merely "the same kind" of cultural values, they are literally *the same* values, looked at and analyzed in terms of different system-references. Neither of these system-references is the "right" or the "real" system reference, both are equally real and stand on the same ontological level. [Parsons and Bales 1956a, pp. 357-58]

As we have seen, Parsons achieved, in *The Social System* and the essay written with Edward Shils for *Toward a General Theory of Action*, a definite extension and refinement of his theoretical instrument, with the help of which complex relations of interpenetration could be understood in all their significant detail. The notion of interpenetration itself, which in *The Structure of Social Action* Parsons had had to handle by means of the distinction between the instrumental and categorical-normative orientations of action, was made more precise and easy to apply to the world. Therefore it is completely inaccurate to say, as has often been said, that at this stage of his career Parsons turned away from the theory of action toward the theory of systems and was from this point on misled into substituting the system for the individual. The object of inquiry in *The Structure of Social Action* is not isolated actors or actions but the systems formed by social action; here Parsons is already primarily concerned with the problem of how the existence of social order within such a system can be explained. The voluntaristic theory of action is from its first formulation a theory of the social system, while a general theory of action is attained only in *The Social System* and "Values, Motives, and Systems of Action" (Parsons and Shils 1951b). "The conception of a theoretical system as my major focus of interest first crystallized in the series of studies leading up to *The Struc-*

ture of Social Action. At this stage, it was confined to the social system case; only in the course of the work in 1949–1950 which eventuated in *Toward a General Theory of Action* did the idea of a *general* theory embracing not only social systems, but also psychological and cultural systems fully crystallize” (Parsons 1959, p. 708).

While in *The Social System* Parsons placed the interpenetration of the cultural system, the social system, and the personality system in the foreground, he at the same time laid the foundations for a new type of analysis, which could work with a multiplicity of system references simultaneously and which would open an entirely new way of thinking about the old problem of the relation of individual and society. *The Social System* represents a clear theoretical advance over *The Structure of Social Action* in that the theoretical apparatus of the former permits the integrated analysis of cultural, social, and personality systems while transcending the limited perspective of a simple system functionalism.⁷

Parsons went on to apply this technique of analysis to the problems of institutionalization and internalization which we have sketched here in a series of works which meet the high standard set by *The Social System*. Perhaps the most significant of these works is *Family, Socialization, and Interaction Process* (Parsons and Bales 1956b). Parsons wrote his contributions to this volume in the wake of his reading of Freud. He makes use of Freudian theory to analyze internalization as the result of the interpenetration of ego-ideal, super-ego, ego, and id, and to mark out the phases of the socialization process. A further refinement resulted in the theory of the generalized media of interchange on the level of the general system of action. Institutionalization and internalization are regarded here as the products of the interpenetration of the cultural system, the social system, the personality system, and the behavioral system. This interpenetration is produced by the generalized media of interchange—definition of the situation, affect, performance capacity, intelligence—each of which is anchored in a particular subsystem but also extends into each of the other subsystems. At this stage of the development of the theory, the decisive step is the inclusion of a multiplicity of levels and a multiplicity of interpenetrating systems on the level of the general theory of action. The main theoretical

⁷ Parsons himself protested against this labeling of his work (1977a, pp. 48–50; 1977f, pp. 100–117; 1977g, p. 127). Cf. Parsons's own attempt to distinguish himself from Merton: “I have differed from him mainly in my strong stress on the importance of the concept ‘system,’ on the inevitability of abstraction, and on the necessity for the self-conscious use of plural or multiple system-references” (Parsons 1977f, p. 108; cf. Merton [1949] 1968). I disagree, therefore, with the attempts of Mullins (1973) and Elsenstadt and Curelaru (1976) to place Parsons in the camp of structural functionalism. Nor is there any reason to postulate a change from structural functionalism to functional structuralism, as Luhmann (1970) has done. Rocher (1974) emphasizes the difference between Parsons's theory and structural functional theories, but he lets some of the criticisms of structural functionalist theories stand as valid criticisms of Parsons also.

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effort is directed toward the discovery of the conditions necessary for the development of a higher degree of freedom of action within the frame of reference of an increasingly generalized set of values. The attainment of higher levels of socialization signifies for Parsons, in this context, not commitment to the normative beliefs of any given group or social class, or even society as a whole, but a commitment to a universal framework of values which transcends particularities. That Parsons never abandoned such a commitment is evident in the second epigraph to this essay.

3. THE PATTERN VARIABLES: *THE SOCIAL SYSTEM, TOWARD A GENERAL THEORY OF ACTION*

The introduction of the pattern variables represents the next stage in the development of the theory's ability to differentiate subsystems of action and analyze their interpenetration. The pattern variables are: universalism-particularism, achievement-ascription (i.e., performance-quality), diffuseness-specificity, neutrality-affectivity, and self-orientation-collectivity-orientation. This last pair was eliminated after the introduction of the four-function paradigm. The pattern variables can be used to indicate with precision the specific relations between opposed modes of orientations of action which have been joined together by the interpenetration of different subsystems of action (Parsons 1951, chaps. 3-5; 1956a, [1960] 1967f; Parsons and Bales 1953; Parsons, Bales, and Shils 1953a). One can examine the interpenetration of cultural system, social system, and personality system as a specific combination of these orientations of action. They are not, however, mutually exclusive orientations. The terms of each pair can interpenetrate as well, thus increasing the scope of both terms together.

The constant generalizing of solidarity relations beyond the limits of the groups in which those relations are originally formed, a process which constitutes the socialization of an individual, is the generalization of that individual's collectivity orientation, but it also and simultaneously provides more scope for self-oriented action. The originally diffuse loyalty to the group is extended by its mediation through loyalties outside the group—for example, through the instrumental role of the father, or through peer groups or school classes. These provide a basis for the transformation of diffuse attachment into specific attachments embodied in specific roles.

In similar fashion, the progressive extension of affectively forged relations between the object of socialization and the agents of socialization through neutral relations makes possible the carrying over of the "primordial trust" which is acquired in affective relations to the neutral relations. It is not necessary, then, that a new affective basis for each of these latter relations be fashioned, as would otherwise be the case. These successive interpenetrations of social systems produce both particular connections to the norms of

various groups and a similar connection to a universal norm, which exists independent of any relation to concrete groups. Finally, the gradual transition from the recognition of individuals solely on the basis of their membership in one group (the family) to a system of allocating praise and blame on the basis of the performance of desirable or undesirable actions provides the basis for an ability to control behavior by a differential allocation of praise and blame without the danger that the individuals who are censured will react by withdrawing altogether from the network of social relations. In each of these cases, we see in the first term of each pair a mechanism which limits the possibilities of action. The gradual transition to the second term expands the scope of action. In this way, dispositions toward action which originally can be realized only in limited spheres become available to many systems.

The pattern variables are designed as a means of characterizing specific dimensions of interpenetration in subsystems of action and social subsystems. Parsons's need for something like the pattern variables arose out of his desire to investigate the role of interpenetration in specific problem areas. The pattern variables first appear in his work on the socialization process, particularly in the writings on socialization in the family, in adolescent peer groups, and in schools. A related group of writings from this general period is the studies of the modern professions, particularly those of the relation between doctor and patient. The first of these studies was the essay "The Professions and Social Structure" (1939); numerous works on the professional complex followed (Parsons 1951, chap. 10; 1968*a*, 1978*e*, 1978*f*; Parsons and Platt 1973). The core of these works is the idea that the professional-client relationship is a zone of interpenetration of the intellectual disciplines, the social systems which determine the criteria governing the fulfillment of the various roles of the client, the personality and behavioral systems of expert and client. Here again, the crucial problems are to locate a form of interpenetration which makes possible an increase in the demands of all these subsystems and to locate a specific combination of action orientations which limit the sphere of action and action orientations which expand it. The relationship between professional and client requires clear external delimitation combined with an inner solidarity which can provide the foundation for mutual trust. The need for trusting cooperation in the relationship makes it natural that the client emphasize the diffuse, affective, particularistic, and qualitative aspects of the relationship. On the other hand, the professional's ties to the canons of his discipline and to the social systems which are dependent on the client's role performance force him constantly to lead the client toward the specific, neutral, universalistic, and performance orientations, all of which are effective only on the basis of the "limited" orientations.

4. THE FOUR-FUNCTION PARADIGM: *WORKING PAPERS IN THE THEORY OF ACTION*

The next refinement of Parsons's theory was the introduction of the four-function paradigm (or the A-G-I-L schema) in *Working Papers in the Theory of Action*, which Parsons wrote in collaboration with Robert Bales and Edward Shils (1953*b*). The four-function paradigm was the result of combining Bales's categorization of the problems which systems face, which he developed during his work on task accomplishment in small groups, with Parsons's pattern variables. The first use of the paradigm was as a model for the types of deviance from norms, and thus also as a model for the related types of social control, but even in the *Working Papers* it was already being generalized beyond the boundaries of this specific problem and being given multiple applications. The paradigm defines the problems which all systems face and relates each problem to a certain combination of action orientations which provides a solution to the problem. The action orientations themselves are divided into object categorizations and attitudes. Object categorizations express the actor's cognitive perception of the objects which constitute his environment, in relation to each other and in relation to his goals. He can perceive them as universal or particular, performative or qualitative. Attitudes signify a cathectic relation to objects, which can be specific or diffuse, neutral or affective. The *Working Papers* goes on to make the following specific connections between systems problems and the orientations of action (Parsons, Bales, and Shils 1953*a*, pp. 172-90):

A. *Adaptation* as a systems problem requires the mobilizing of resources as a basis for the realization of goals. The solution of the problem of adaptation requires a combination of universalistic and performative object categorizations, along with specific and neutral attitudes.

G. *Goal attainment* (or goal selection) requires a mechanism which allows ends or goals to be arranged hierarchically in all specific situations in which choices of goals must be made. The proper combination of action orientations here is that of performative and particularistic object categorizations and specific and affective attitudes toward the objects.

I. *Integration* requires the cohesion of the units in the various systems (actors in the social system, need dispositions in the personality system, symbols in the cultural system) with one another, their solidarity. The appropriate action orientations here are particularistic and qualitative object categorizations, together with diffuse and affective attitudes.

L. *Latent pattern maintenance* requires the ability to preserve the basic structure of the system, together with the ability to adjust to changing conditions within the framework that the basic structure provides. The solution to these problems presupposes a combination of qualitative and universalistic object-categorizations, together with neutral and diffuse attitudes.

These connections between systems problems and action orientations were then further differentiated into subdimensions by Parsons; this process reached its climax in his essay "Pattern Variables Revisited: A Response to Robert Dubin" (Parsons [1960] 1967f; Dubin [1960] 1967; Loubser 1976). The development of the four-function paradigm was progressively freed from its connection with the pattern variables, and the paradigm was progressively generalized beyond its originally limited area of application, so that it could be continually respecified and applied to new ranges of objects. In this way, the paradigm assumed more and more the character of a general theoretical instrument for the analytical differentiation of reality. From the point of view of the development of the theory as a whole, the most significant events of this period are the use of the four-function paradigm in the analytic differentiation of subsystems of action (Parsons 1977i, p. 245), the repeated application of the paradigm to the problem of the internal differentiation of various individual subsystems and to the problem of the differentiation of levels of analysis having different system references, and the connection of the paradigm with the hierarchy of cybernetic controls. The application of the A-G-I-L schema to the subsystems of action, their environment, and their microscopic-macroscopic ordering yielded three basic levels of analysis, each of which in turn could be further internally differentiated into a microscopic-macroscopic hierarchy (Parsons, Bales, and Shils 1953a, pp. 172-79; Parsons 1977i):

1. The level of the "human condition," which Parsons explored at the very end of his career, consists of: L—telic system, I—action system, G—human organic system, A—physicochemical system.
2. The level of the general system of action consists of: L—cultural system, I—social system, G—personality system, A—behavioral system.
3. The level of the social system consists of: L—social-cultural (fiduciary) system, I—societal community, G—polity, A—economic system.

These analytic differentiations can be carried even further and used to focus on and isolate a specific aspect of reality out of the entire complex. For example, we can focus on the legal system, considering it as the integrative subsystem of the political system, and within the legal system we can focus on, say, the fundamental principles of modern law as the integrative subsystem of the latent pattern maintenance subsystem (see figs. 1A, 1B, and 2).⁸ What is valuable about this technique of analytic differentiation is that, through the location of an aspect of reality in the schema, we can understand in what ways and to what extent the nature of this aspect is determined by its interpenetration with other subsystems on various other levels. So we can, for example, explore how the character and significance

⁸ Unlike Parsons, I interpret administration as goal specifying (G) and legislation through political exchange as adaptive (A) in regard to the articulation of interests.

of the fundamental principles of modern law—the orientation toward the meaning of actions (L), equality before the law (I), freedom (A), and legal rationalism (G) (which is itself differentiable into abstraction [L], excluded contradiction [I], analysis [G], and formalism [A])—are the product of the interpenetration of cultural legal discourse, rational legal science, the legal profession, and the legal community. This last relation already provides a link to the next higher level of analysis. Ultimately, however, the decisive significance will belong to the subsystem we want to investigate and its function. On the next higher level of analysis, we can understand legal culture as a product of the interpenetration of the legal community, judicial decisions, the legal process, and the constitution, which here provides the connection to yet the next level of analysis. One level higher, the law finds itself in the zone of interpenetration of constitution, political exchange, executive power, and the societal community, which is the mediator to the next level of analysis. One level higher again, we would find the political system in the zone of interpenetration of fiduciary system,

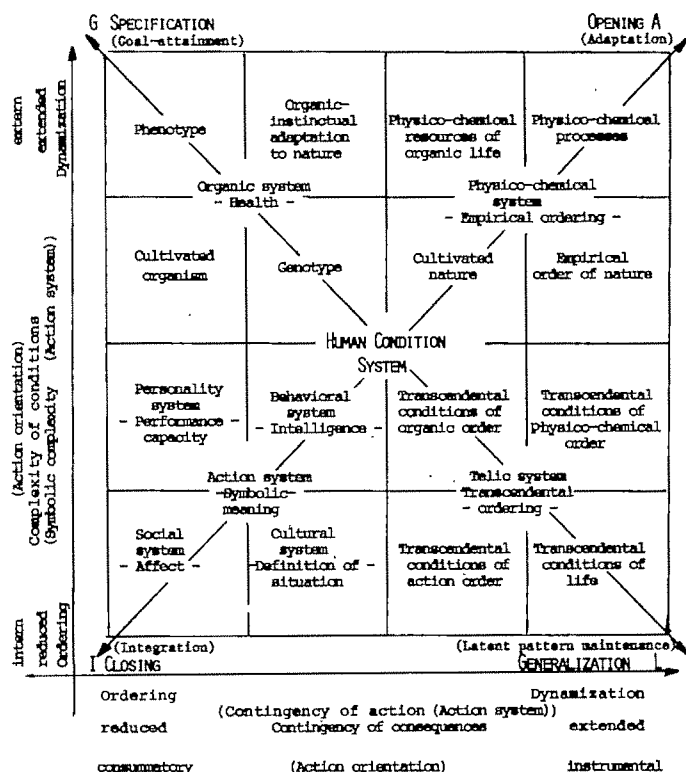


FIG. 1A.—The human condition. (For a detailed diagram of the action system, see fig. 1B.)

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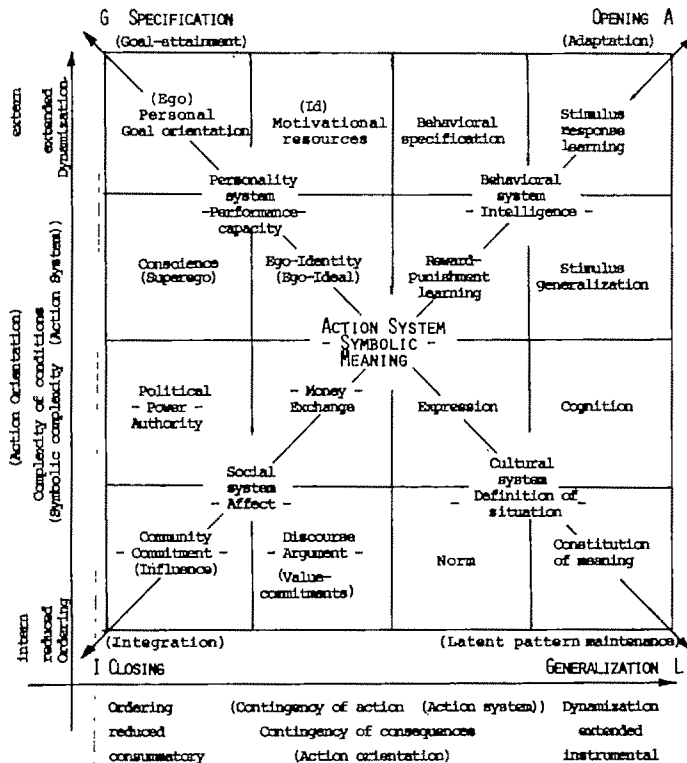


FIG. 1B.—The action system

societal community, economy, and personality system, which is the link to the level of the general theory of action.

What we have been presenting here is an analytical reconstruction of the legal system. This is fundamentally different from talking about the structure of a concrete legal system. In a concrete legal system, certain aspects of the analytical model may be missing completely, and others may be disproportionately dominant. A concrete legal system may produce judicial decisions totally lacking any basis in the cultural and social spheres and inaccessible to the opening procedures of the legal process. Such decisions would be determined solely by the political component, by the application of political power. Assertions of this sort are empirical applications of the theoretical model, which itself encompasses all of the analytic dimensions.⁹

⁹ Burger (1977) criticizes Parsons's analytical sociology as not empirically verifiable. He pleads for an empirical sociology in which the different factors which determine action will be incorporated directly into an empirical model. Burger fails to recognize that the various forces which control action must be analytically separated, in order to determine what their effect is in general, before they can be joined together in empirical applications. An adequate theory of action must include both an analytical model and

This distinction between analytic reconstruction and concrete reality, between theoretical model and empirical application, applies equally to all the models which will be presented in the course of this paper.

The introduction of the cybernetic hierarchy of conditions and controls allows us to determine more precisely the kind of interpenetration which occurs between individual subsystems.¹⁰ From the A system through the

¹⁰ Parsons introduced this element of the theory after an intensive reading of recent developments in physiology and modern cybernetics (Cannon 1932, Wiener [1948] 1961). See Parsons 1959, pp. 681-88; 1961*a*, chap. 1; 1966, p. 29; Schwanenberg 1971, pp. 577-79.

G system and the I system up to the L system, the level of information increases, that is, the ability of the system to guide action by means of symbolic articulations (e.g., rational argument) increases. The subsystems with a higher level of information have the ability to guide or control action in the subsystems which are less rich in information. They do so by defining the limits of action in the subordinate subsystems. For example, a system of generalized values defines the limits of solidarity formation, goal selection, and resource mobilization. Conversely, from the L system up through the I system and the G system to the A system, the level of motivational energy available for action increases. In this hierarchy, the higher subsystem has a dynamizing effect on the lower ones, that is, it expands their sphere of action. This would endanger the degree of order in the lower subsystems, if this dynamization were not delimited through its penetration by a more highly ordered system. The expansion of the market has a dynamizing effect on the ethics of a community (I system) because it loosens the ethical constraints on action. This could lead to the dissolution of the entire societal community and the disappearance of ethical regulation altogether if the penetration of pure market activity by the societal community did not produce an ethical tempering of this activity and thus keep the dynamizing effect of the expansion of the market within its proper limits. The precipitate of this interpenetration would be business ethics, which mediates two subsystems which would otherwise operate solely according to their own inner laws. What is significant here is that the market can be said to have a dynamizing effect on the societal community precisely to the extent that the latter is forced, for its own survival, to limit or control the impact of the former; conversely, high energy systems can only be controlled and thus can only have their energy increased to the extent that they push against limits set by high-information systems. Control and dynamization are inseparable processes, and both depend on a constant interpenetration of subsystems.

One way of understanding these inverted hierarchies of control and dynamization more precisely is to consider the fourfold classificatory scheme which is formed by the distinctions of internal-external and instrumental-consummatory orientations of action, which Parsons introduced after his first formulation of the A-G-I-L schema as the foundation for the analytic differentiation of subsystems (Parsons [1960] 1967f; cf. Baum 1976a, 1976b, 1977; Münch 1980b, pp. 31–34). This construction, which Parsons used from "Pattern Variables Revisited" onward, lacks, it seems to me, the "fine-tuning" capacities of the cybernetic hierarchy. It is more difficult to apply to the internal differentiation of the four-dimensional space of action and to the construction of zones of interpenetration. It seems to me that a construction of the analytic schema based on the idea of the cybernetic hierarchy is more fruitful. A starting point for this construction is the

distinction between symbols and individual acts as basic elements of every action system. The nature of the relation between symbols, in the form of systems of meaning, normative systems, expressive symbol systems, and cognitive systems considered as expectations of actors' symbolic articulations, on the one hand, and actions themselves, on the other hand, is of decisive significance both for the orderedness and control of systems and for their openness and capacity for dynamization. The relations between symbols and actions are already specifications of the more general relations between antecedents and consequents which obtain between all events. Symbols may vary between the poles of highest complexity and highest orderedness; actions vary between the poles of highest contingency and highest predictability.¹¹ The relations between complexity of symbols and contingency of actions can be expressed in a hierarchically ranked fourfold table. Repeated application of this table yields continually finer-grained determinations of the relations between symbols and actions and a microscopically-macroscopically ordered differentiation of system levels. One can move upward, embedding the system of action in the system of the human condition, or downward, using the schema to make further differentiations of the action system and the social system (see figs. 1A, 1B, and 2).¹²

With this system of classification, we can formulate precisely the relation of each function of a subsystem to our two basic elements—symbols and actions—and our two basic properties—complexity and contingency. Four and only four basic relationships obtain between complexity of symbol and contingency of action: generalization, specification, closedness, and openness. The construction of this sort of relation between symbol and action is a way of characterizing the function of every structure and process of action. Thus, discourse yields a generalization of symbols combined with actions of high contingency as their implementation. Authority is that structure of social action which makes possible obligatory commitments to specific actions in the face of alternatives which are presented with a high degree of symbolic complexity. Community closes the universes of symbolic presentations and actions through its insistence on the self-evidentness of the obligations of its members. Exchange opens the spectrum of symbolic articulations and actions; it is the social structure which embodies the least amount of restraint on possible actions and expectations. On the level of

¹¹ Here I refer to Luhmann's basic categories (1974): experience and action, complexity and contingency. However, Luhmann backs away from constructing a generally applicable analytical schema.

¹² I use here not the order of the subsystems which is set out in *Economy and Society*, but that which was worked out by Gould (1976) and appeared in *Working Papers* (Parsons and Smelser 1956, p. 68; Parsons, Bales, and Shils 1953a, pp. 192, 195). Only this ordering of the A-G-I-L schema brings out its full potential as a way of locating a multiplicity of subsystems between the poles of the ordering and dynamizing systems. It allows us to solve the theoretical problems raised by Luhmann (1978).

the general system of action, a symbolic code as the structure of the cultural system is the most highly generalized and as such allows a high contingency of action. The specification of concrete goals out of a multiplicity of symbolically presented alternatives necessitates the forging of a personal identity. The self-evidentness of duty requires the bonds which a social system provides. The opening up of expectations and actions necessary for adjustment to changing conditions requires the learning, through a system of rewards and punishments, of ways to manipulate the physical-biological world.

It is necessary to distinguish these controlling and dynamizing relations between subsystems, which exist on the theoretical level, from the actual relations which obtain between corresponding empirical subsystems. The relations between empirical subsystems can range from the accommodation of the potentially controlling subsystem to the dynamizing subsystem, through their reconciliation, their mutual isolation, and their interpenetration, to the one-sided domination of the potentially dynamizing subsystem by the controlling one.¹⁸

By defining the functions of systems through the classification of the relations between the elements of a system and their most abstract properties, one can justify the choice of these functions: at this level of abstraction the classification is exhaustive. The abstractness of the functions is what allows them to be specified for various subsystems, sub-subsystems, etc., without the danger of committing that error, so feared by Parsons, which Whitehead calls "the fallacy of misplaced concreteness." The microscopic-macroscopic, "sliding scale" nature of the classificatory scheme allows for the drawing of ever finer distinctions and for the continual identification of intermediary types which lie in the zones of interpenetration of the basic types. The repeated application of the classificatory scheme performs here the function that in mathematics is performed by systems of numbers, of which the simplest is the binary system. By means of the repeated application of the same set of numbers, the binary system does everything the decimal system does. The four-function paradigm is this type of system, one which through the repeated application of a small group of basic categories is able to capture all aspects of reality (Parsons 1977i, pp. 249-60). The four-function paradigm is itself a duplication of a simpler two-term schema (absence of orderedness vs. maximum of orderedness) through the

¹⁸ Parsons never made a clear distinction between these types of relations. He tended to write as if every concrete action had to be thought of as a product of the interpenetration of subsystems (Parsons 1959). "Interpenetration" here obviously has a very general meaning. But Parsons also uses the term "interpenetration" in a much more specific sense. In this narrower sense, interpenetration signifies a specific type of relation between subsystems, one possible relation among others. In the interest of precision, I will use the term only in this second, narrower sense.

application of this simpler schema to the two basic elements (symbols and actions).

As an example of the way in which repeated application of the fundamental schema can yield ever finer determinations of functions as intermediary stages between, and zones of interpenetration of, the basic schema, let us look at exchange as a form of social interaction. Exchange, as the structure of the economic system, performs in the social system the function of opening the framework of action. It has a dynamizing effect. As a subsystem of the social system, it can have this effect only within the framework of a social order which already limits the possibilities of action and establishes peaceful exchange as the only legitimate form of the private acquisition of goods. Within the social system, this limitation cannot arise out of the activity of the acquisition of goods itself. It can arise only out of the penetration of the activity of acquisition by normative rules governing exchange and founded on commitments to a societal community which proscribes violations of the rules. The "market community" is that form of community which is located in the zone of interpenetration of exchange and the societal community. The market community carries the guidance of the community into acquisitive activity and, on the other hand, transmits the dynamizing effect of exchange to the community (Weber [1922] 1976, pp. 382-85; Durkheim [1893] 1964; Parsons and Smelser 1956, pp. 101-84). This makes possible both the expansion of exchange activity and the expansion of the moral order, which now finds for itself a sphere of application to which it had heretofore been a stranger. Through this interpenetration of exchange and societal community, a society can become both economic *and* moral and can decrease the incompatibilities between these two spheres. These incompatibilities would be especially marked in societies in which the development of the acquisitive life was not accompanied by an expansion of the bonds of community but instead the formation of communities extending beyond kinship relations remained impossible, as has more or less been the case outside the Occident. In these societies, money truly does become what Marx took it always to be, "the debasement of the moral and economic order of things" (Marx 1970, p. 146; translation by Paul Gudel). Here is that unbridgeable chasm between the sphere of economic rationality and the sphere of the ethic of brotherliness which Weber diagnosed as a consequence of the failure of religious ethics and the world mutually to penetrate (Weber [1920] 1972, pp. 536-46). The compatibility or incompatibility of social spheres is a consequence of the organization of their relations with one another; the mechanism by which essentially incompatible spheres can be made compatible is not simply differentiation, but the independent social anchoring of their cores in such a way that they must interpenetrate, so that in the zone of interpenetration a new quality will be formed as a bridge between them.

Another form of dynamizing and controlling interpenetration exists between exchange and authority and between exchange and cultural discourse. The expansion of commercial exchange has a dynamizing effect above all on cultural discourse and on the system of authority. On the one hand, it strains the consistency of cultural patterns and, on the other hand, it strains the ability of the system of authority to make and enforce decisions. This dynamizing effect can once again be held within limits only if the value system can be injected into exchange through value generalization, thus determining what will count as legitimate activity in the area of exchange. But in addition the system of authority must be able to enforce the limitations on market activity beyond the self-evident rules of the market community.

The classification of controlling and dynamizing effects according to the relations of symbolic complexity and contingency of action shows that the I system possesses the highest level of orderedness. In the social system this means that it is through the societal community that the realm of symbols and the realm of action become closed as realms possessing self-evidentness. Therefore, it is from the societal community (not from the L system, the fiduciary system) that the strongest limiting effect proceeds down through the system as a whole. On the other hand, the generalization of the value system which takes place in the fiduciary system exercises a dynamizing effect with regard to the self-evident elements of a community, in that it extends the realm of action which is legitimized by general values. If we change Parsons's cybernetic hierarchy in this way, putting the I system at the highest level of control, we can grasp more precisely the dynamizing effects of the cultural system on the level of the general system of action, of the fiduciary system on the level of the social system, and in general of any L system on any level of analysis. Discourse, as the structure of the fiduciary system, acts to mark out the space of acceptable interpretations of the value pattern of a society; accordingly, it has a generalizing effect with regard to the concretizations of the value pattern which hold for specific social spheres. It promotes cohesion among the different normative orientations which are valid for specific social spheres by anchoring them in a consistent value pattern. This insures unity in diversity and continuity in change and facilitates the formation of consensus between adversary positions. On the other hand, the marking out of a space for the interpretation of the value pattern through discourse can also beget conflict. Discourse can hold itself aloof from the concretizations of values contained in social institutions and place on those institutions new demands that they justify themselves. When the claims of cultural discourse begin to make constant, exorbitant demands on social institutions, the value pattern is supplying not the kernel of the society's sense of identity, but the kernel of its ultimate fragmentation. A third possible relation between the cultural system and the other spheres of a society would be the tight binding of the

former to a determinate social group which is dominant in the societal community, as, for example, Confucianism formed the ideology of the ruling class of literati in China (Weber [1922] 1976, pp. 395-458). The tight connection between the value pattern of American society and the white Anglo-Saxon Protestant community, the WASPs, produced a similar effect. In extreme cases of this type, the social value pattern comes to take the form of the value orientation of a ruling social group, and it loses its power of generalization as well as its ability to facilitate the inclusion of different social groups in one societal community (Parsons 1967*a*, 1967*b*, 1967*d*, 1971, chap. 6; 1977*c*, 1977*d*, 1977*j*; Lipset 1963).

Which of these three relations between the cultural system and the system of the societal community actually obtains depends on the degree of interpenetration of these two systems. The formation of consensus between different social groups and the persistence of continuity through change depend on the mutual penetration of both systems. If the cultural system possesses sufficient institutional independence from the societal community and yet reaches out into it, and if the societal community can reach out into the cultural system without losing its own institutional base, then the cultural system can exercise a generalizing effect on the concrete value orientations of individual groups, which on the basis of this assimilation of their value orientations are given a common identity. The extension of the societal community into the cultural system gives the value system an adequate grounding in the societal community and a relationship to the value orientations and practical requirements of social groups. The dynamizing effect produced by intellectual interpretation of the value pattern, which because of the intellectual orientation toward rational construction inclines toward a more stringent consistency of ethical convictions, is limited through its connection to a social community. If, on the other hand, culture is the affair of intellectuals only, its inherent tendency will be greatly accentuated, and the cultural system will begin to have a divisive effect on the society. Conversely, where the cultural system is merely the appendage of one dominant social class, it cannot provide a foundation for a common identity for the different communities within the society. In both cases, the interpenetration of culture and societal community is lacking.

The educational system, especially the university, has a particularly important role in the formation of this complex relation of differentiation and interpenetration between culture and societal community: "It seems reasonable to suggest that the system of higher education is particularly important in the development of mobile 'markets' for the factors of solidarity" (Parsons 1977*i*, p. 254). Not all educational systems fulfill this function in the same way, as one can easily see from a glance at different systems. While the American system of education, for example, relies more

on interpenetration, the European system, traditionally, tends to segregate and isolate intellectual culture from the other spheres of the society, which presents a considerable potential for conflict within these societies.

In this connection, one can give more precision to the frequently cited distinction, drawn by David Lockwood (1964), between social integration and system integration. By "social integration" is meant, as Lockwood holds, the emergence of collective solidarity as a function of the societal community. "System integration" would denote, more precisely than in Lockwood's distinction, the interpenetration of analytically differentiated subsystems as *one* among many different possible relations between subsystems. The theorem of the cybernetic relations of control and dynamization belongs to the Parsonian theory of action as a way of making more precise the theorem of interpenetration, as a way of analyzing relations between subsystems. This theorem too should not be misunderstood as an assertion concerning concrete systems. It does not entail a "cultural determinism" or a functional primacy of the cultural system in a concrete sense (Schluchter 1980, pp. 115-116, 136-139). The theorem of the high position of the cultural system as an analytical system in the cybernetic hierarchy applied to social institutions postulates that institutions can lay claim to a truly normative validity, as opposed to a mere empirical validity dependent on power and interest, only if they can establish themselves as specifications of a more general system of common values. I have given a still higher place in the hierarchy to the societal community because these culturally justifying institutions can possess normatively binding validity only if the value system is anchored in at least a possibly universal community of solidarity which, through the self-evidentness of the obligations it imposes, sets a limit to the potentially endless rational questioning and examining of the value system. This hypothesis is traceable to the theorem, already present in *The Structure of Social Action* ([1937] 1968b), that the social order as normative, that is, as an order which includes the freedom of the individual rather than as a causally determined, empirical order, cannot be reduced to the interplay of power and interest.¹⁴ The Parsonian theory of action never denies the empirical effectiveness of power and interest, or of conflict, as is so often asserted.¹⁵ Conflict is a dynamic

¹⁴ That there are value conflicts in concrete cases, and that the capacity of the social-cultural system to guide and control the other systems is in reality frequently limited, are not facts which in themselves falsify the Parsonian theory of action. Nothing in the theory denies these facts. Therefore there is no need to replace the Parsonian theory with a "realistic" theory of action, as has been proposed by Schluchter (1980, pp. 116-40).

¹⁵ Dahrendorf's characterization of the "theory of integration" as opposed to a "theory of conflict" is a caricature of Parsons's theory of action. The central thesis of the theory of integration is that every society rests on a de facto consensus of its members. In this concrete formulation, such a proposition is to be found nowhere in Parsons;

factor which tends toward the dissolution or reorganization of existing institutions. The reorganization of old institutions and the establishment of wholly new ones, however, are subject to the same conditions as every other attempt to establish social order. And here, if it is a question of normatively valid institutions, constellations of power and interests in themselves are as much insufficient conditions as cultural legitimation and a communal foundation are indispensable conditions. Moreover, power and interest cannot even be the only moving factors in change. The cultural legitimation of institutions through rational argument, and the expansive movement of the socialization process, through which the individual is continually growing into more extensive social circles, exercise a pressure on concrete institutions to change in the direction of a more general justification of norms.

6. THE THEORY OF THE GENERALIZED MEDIA OF INTERCHANGE

A. The Interpenetration of the Social Subsystems: *Economy and Society*

Parsons reached a new stage in the analysis of analytically differentiated subsystems with the introduction of the theory of the generalized media of interchange, which he formulated successively for the subsystems of the social system (money, political power, influence, value-commitments)¹⁶ (Parsons and Smelser 1956; Parsons 1969*a*, 1969*b*, 1969*c*; Baum 1976*a*, 1976*b*; Münch 1976), the action system (definition of the situation, affect, performance capacity, intelligence) (Parsons 1977*h*, 1977*i*; Parsons and Platt 1973), and finally the system of the human condition (transcendental ordering, meaning, health, empirical ordering) (Parsons 1978*d*). The starting point for this theoretical development was a renewed encounter with economic theory, occasioned by an invitation from the De-

the reconstruction of the theory of action given here should show that such a proposition is incompatible with the theory (Dahrendorf 1958*a*, 1958*b*; Mills 1959; Collins 1968; Gouldner 1971).

¹⁶ For the media "value-commitments" (L) and "influence" (I), I would substitute "argument" as the medium of social-cultural discursive action (L) and "commitment" as the medium of communal action (I). We can imagine specifications of these media within the general framework of action. E.g., we could propose "interpretation" (L), "consensus formation" (I), "expertise" (G), and "truth" (A) as submedia of "argument." Similarly, I understand "value-commitments" as a normative-cultural specification of "commitments" which is found in the zone of interpenetration of communal action and cultural discursive action. "Influence" is a conjunction of expertise and commitment to the societal community (Münch 1980*b*, pp. 36-41). For the concept of the "fiduciary system," I would substitute that of the "social-cultural system." The concept of the fiduciary system already contains the professional aspect which is actually only the G component of the social-cultural system. The notion of the social-cultural system is more general in its significance and refers to that aspect of social action which deals with symbolic understanding.

partment of Economics at Cambridge University to deliver a guest lecture in honor of the memory of Alfred Marshall. This invitation led to an intensive period of work with Neil J. Smelser, out of which emerged the coauthored *Economy and Society* (Parsons and Smelser 1956). In this book, Parsons and Smelser developed a theory of the economic system and a theory of money within the framework of the general theory of action. The theories differed from pure economic theory in that they viewed economic action as mediated through the interpenetration of analytically differentiated social subsystems—the economic system, the political system, the integrative system, and the cultural system (Weber [1922] 1976, pp. 181–233). In the foreground of the analysis stood the relations between the economic system and the other three subsystems. These relations appeared here, for the first time, as relations of interchange, in which money as a generalized medium of interchange played a central role. And next to money, the medium of interchange anchored in the economic system, there began to emerge indications of the concepts of political power, influence, and value-commitments as media anchored in the political system, the integrative system, and the cultural system, respectively.

All of the media of interchange possess certain common characteristics which make them particularly suitable for the establishment of a certain type of relationship between systems, one which combines differentiation and interpenetration. Each medium possesses the following qualities (Parsons 1977*h*, pp. 204–14):

1. Media of the social system and the action system are symbolic in character, that is, they are signs of other objects which one can recall through the medium under certain circumstances. Money symbolizes a certain quantity of goods or services which can be obtained with it.
2. Media are generalized, that is, one can with their help re-present a variety of objects, independent of concrete circumstances.
3. Every medium is founded on the institutionalization of a "medium code," that is, on binding normative rules which set the conditions of acquisition and use. Through the institutionalization of a code, a medium becomes a valid means of interchange, but only within a clearly defined context in which it can be used for achieving determinate goals. Money becomes a medium of peaceful exchange through the existence of the institution of property. Political power is the legitimate medium of the collectively binding implementation of decisions only through the existence of a valid order of authority. Influence is the medium for the motivation of action only through the existence of an order of prestige. Value-commitment is a medium which engenders loyalty to social institutions only because of the existence of an order of common values under which institutions can be subsumed.
4. Through its institutionalized code, each medium is given a specific significance, on the basis of which each can be distinguished from the others and which gives each medium a delimited area of valid application.

5. Media circulate, in two respects: first, they can be passed around among actors; second, each medium is able to pass over the boundaries of the system in which it originates and thus make possible a specific form of interpenetration of subsystems.
6. A particularly significant characteristic of media is that they are not subject to zero-sum conditions. This means that when one actor increases the amount of a medium that he has at his disposal, he does not necessarily reduce the availability of the medium to other actors. This necessitates constant material production, so that the total quantity of the medium can be increased without depreciation. It is only under this condition that, for example, the creation of money results in a real increase in the sum of money in a system. Similarly, the increased production of compulsorily binding decisions is a presupposition of the real increase of power, the raising of the level of social solidarity is a presupposition of the increase in influence, and the increased production of knowledge is a presupposition of the real increase of intelligence. The same transcendence of zero-sum conditions holds for the relation of the media to each other: an increase in one does not require a decrease elsewhere.

At the level of the social system, the following zones of interpenetration are created through the reciprocal guidance of action by the appropriate generalized media (see fig. 3):

- L ↔ I. Through the use of arguments (value-commitments), the normative orientation is placed within a general framework of communal activity. The binding of social-cultural discussion to the

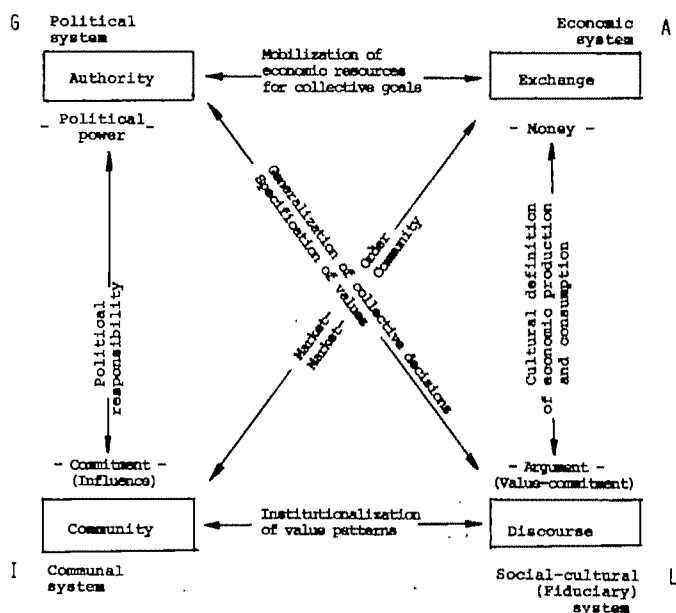


FIG. 3.—The zones of interpenetration of the social system

commitment to a community (to the gaining of influence in the community) sets the normative limit to this discussion. Out of these two processes arises the institutionalization of the value pattern.

- L \longleftrightarrow G. Through argument (value-commitments), collective decisions are embedded in a general framework. Conversely, general values are specified in the process of collective decision making through the application of legitimate political power.
- L \longleftrightarrow A. Argument (value-commitments) defines the cultural framework of economic action. The use of money extends the scope for cultural action because it liberates the orientation to cultural standards from ascriptive ties.
- G \longleftrightarrow A. The use of money facilitates the mobilization of economic resources for the achieving of collective and individual goals; the long-standing security of economic resources necessitates a collective responsibility which goes beyond the interests of individuals, is independent of particular power groups, and is supported by legitimate and generalized political power.
- G \longleftrightarrow I. The binding validity of collective decisions requires the anchoring of articulations of interest demands in a supportive community through commitment to this community (the acquisition of influence in it). The conversion of these legitimately defined interests into collective decisions in the process of decision making must be supported by the exercise of legitimate political power. It is in this context that political responsibility develops.
- I \longleftrightarrow A. The submission of economic action to the economic order—the order of the market—arises out of the commitment to a community (the acquisition of influence in it). The use of money broadens the scope for action within the limits of the community.

We cannot go into all of these relations of interpenetration in detail here. As an example, we shall look at the relation between the social-cultural (fiduciary) system and the economic system with the help of the theory of media.

The characteristics of media which are of decisive significance for the theory of interpenetration are the institutional anchoring of the media and their ability to circulate beyond the bounds of the subsystems of their origin. Both qualities help meet the conditions of interpenetration: the differentiation of subsystems and their simultaneous mutual opening to one another. Parsons and Smelser make this characteristic of the media quite clear in their discussion of the relations of interchange between the economic system and the other three systems, and in this way they distinguish their analysis of the medium of money from all purely economic theories of money (1956, chaps. 1–3). Money is the mediating term which makes possible the interpenetration of the economic system and the cultural, integrative, and political systems, because it can circulate among these systems and thus not only convey the demands of the process of the rational acquisition of goods into these other subsystems but also bring

back from these subsystems their claims on the economic subsystem. Parsons and Smelser demonstrate this dual function of money primarily in their analysis of the interchange between the business firm, the basic unit of the economic system, and the household, the basic unit of the social-cultural (fiduciary) system:¹⁷

Household members want to "live" according to a given pattern; the firm's goal is to "produce," secure rewards, and accumulate facilities to continue producing. Some mechanism must mediate between these two distinct orientations.

On the one hand, money represents the *generalization* of purchasing power to *control decisions* to exchange goods; on the other hand it symbolizes attitudes. The former is the "wealth" aspect of consumers' income, the latter the "prestige" aspect. If it cannot command goods and services money is not acceptable as wages; if it cannot symbolize prestige and mediate between detailed symbols and a broader symbolization it is not acceptable on other grounds. Only with this dual significance can money perform its *social* functions. [Parsons and Smelser 1956, p. 71]

In particular, between the business firm and the household there is both an interchange of factors (labor capacity and wage income) and an interchange of products (commodity demand and commitment to production of goods). The firm is anchored primarily in the economic system and the household primarily in the social-cultural (fiduciary) system. That does not mean that the economic system comes to a stop at the door of the concrete household. It means that the household is an empirical unit in which analytically differentiated subsystems with their own internal orders can affect one another and mutually penetrate. The household is on the one hand an economic unit which participates in economic activity as a source for labor capacity, as a producer or a consumer, and thereby is subject to the economic order. On the other hand, the household is a significant place for socialization and as such is a bearer of the cultural order: "The relationship between the economy and the latent pattern-maintenance and tension-management subsystem is essentially analytical, i.e., it illustrates the crucial point of interaction between two differentiated functional sub-systems. But the economy does not 'end' at the market for consumer goods. Indeed a good deal of the working capital of the economy is, even in our society, located concretely in households in the form of consumer durables in the process of depreciation" (Parsons and Smelser 1956, p. 54). One can consider the labor market and the consumer market as zones of interpenetration of the economic system and the social-cultural (fiduciary) system, in which firm and household constitute the units of mediation between the interpenetrating systems (Parsons

¹⁷ This core of the theory of media is not present in Blain's (1971) alternative formulation.

1969*b*, p. 398; Parsons and Smelser 1956, pp. 51–85, 101–84). In the interchange of factors, labor capacity and wage income, the interpenetration of these two systems means that the qualifications of the workers—for example, their value orientations, their cognitive-technical abilities, their sense of discipline—are determined by their socialization in the family and other loci of socialization that bear primary cultural responsibility. The process of socialization is the source of all indirect influence on economic life; this becomes more true the more the process of socialization is expanded. The qualifications of workers derive chiefly from cultural standards, which the firm cannot evade simply by laying down its own qualifications requirements. In the firm, the qualifications of the workers confront the demands of economic rationality, which expresses itself through the distribution of wage income differentially, on the basis of economic criteria of scarcity (Parsons and Smelser 1956, pp. 175–84). Wage income is thus determined primarily by criteria of economic rationality and only secondarily by culturally imparted qualifications, while labor capacity is determined primarily by cultural standards and only secondarily by economic standards. Wage income is a primary economically determined input factor in the household, where it forms the basis for the formation of a specific life-style, which in turn yields a certain orientation to cultural standards. Here wage income sets an economic limit to the realization of cultural standards and provides a way for the economic system to penetrate the social-cultural (fiduciary) system. Concrete consumer demand, however, is a product of orientation toward cultural standards and to that extent provides a way for the social-cultural (fiduciary) system to penetrate the economic system. The corresponding commitment of the business to produce goods is not, however, simply derivable from cultural standards of consumption. Instead, those standards will be broken down by the business's orientation toward economic rationality, to which it is forced by market competition. The commitment to produce goods is therefore determined primarily by the economic system, and secondarily by cultural standards, while commodity demand is determined primarily culturally, and secondarily economically.

In accordance with this conception, Parsons included "value-commitment" as the medium specific to the social-cultural (fiduciary) system. In the interchange of factors, value-commitments oblige the individual to orient himself toward standards of discipline and cognitive rationality—for example, in the shape of the puritan ethic—relating to his capacity as a worker, while the use of money as the payment for performance pushes both the business which pays it and the worker who receives it toward economic rationality, thereby setting a limit to the reach of cultural standards. In the interchange of products, the use of money pushes both the producer and the consumer toward economic rationality as a

sumption. Culturally formed desires for consumption provide the material which is given *form* through submission to the laws of the economically rational employment of money. If there were no generalized medium, there would be no basis for rational economic calculation, and economic activity would be overwhelmed by concrete standards of consumption that would be inaccessible to rational calculation. We can see this in all forms of economy which lack that coherence which can be provided only by the use of money (Weber [1922] 1976, pp. 45–62, 199–211).

As a generalized medium, value-commitments can control action in all situations of labor and production. The anchoring of this medium in the cultural value pattern gives to value-commitments that constant, invariable meaning through which action in these situations does not occur according to mere whim, but remains always tied to the cultural value pattern. The development of generalized value-commitments allows the shaping of economic activity by the cultural value pattern. If the commitments to values are too concrete, it will be impossible to transfer them to any other areas of life. In the other areas, purely internal laws, such as the rules of self-interest, would then be free to hold complete sway. The generalization of value-commitments, though, permits them to be specified in concrete situations in such a way as to be harmonizable with economic demands. Economic demands provide in this case the material of action which through generalized value-commitments is given ethical form but is not stifled by its demands.

The control exercised on the relation between the economic and social-cultural (fiduciary) systems by money and value-commitments as media of interchange allows for an expansion of the range of both systems, without their interfering with one another or one overwhelming the other. By the creation of credit (through banks in the economic system and through educational institutions in the cultural system), zero-sum conditions are overcome. Within both systems, an increase leads to increased capacity for production. Money can be invested and applied to a wider range of interests. Value-commitments can be invested, and the sphere in which they can be validly implemented can be expanded, for example, if the socialization performed by educational institutions can be opened up to wider areas of the specification of value-commitments.

Analyzing relations of interchange between subsystems by means of the theory of media permits us to grasp such dynamic processes as deflation or inflation of media (Parsons 1969*b*, pp. 383–95; 1969*c*, pp. 467–69; Parsons and Platt 1973, pp. 310–45; Münch 1976, pp. 135–74). Deflation and inflation are crises which indicate a decrease in the capability of the system in question to fulfill its function. Deflation is a decrease in the demand for goods, as this is expressed in money, which ultimately leads to a corresponding decrease in the capacity for production. In like manner, a deflation of value-commitments is a decrease in the potential breadth of implementation

of general values; it impairs the ability of the general value pattern to shape action in widely disparate areas of life. Monetary inflation is caused by a rise in demand which is not followed by an increase in production. In the same way, inflation of value-commitments occurs when the claims of value-commitments on various areas of life are not accompanied by a corresponding ability of these value-commitments ethically to shape action in these spheres.

The development of generalized value-commitments naturally has specific presuppositions in the organization of the socialization process. Only when successive stages of socialization into interpenetrating subsystems trigger that interpenetration between the cultural, social, and personality systems of which Parsons had already had intimations in *The Social System* (1951) can value-commitments be sufficiently generalized. The formation of value-commitments as a general medium of interchange already presupposes, therefore, an interpenetration, which must occur before the medium can raise the level of combined differentiation and interpenetration of the social subsystems.

Through the introduction of the generalized media of interchange, Parsons achieved a further refinement of the differentiation and interpenetration of social subsystems, as we have seen from even the limited examples we have looked at. The theory of the generalized media of interchange is thus a logical step in the development of the theory of action, which is based on the dualistic conception of the system of action as a zone of interpenetration of conditional and categorical action orientations. Of particular significance in the development we are considering here is the discovery that money as the medium of the economic system functions both as the bearer of the institutional core of that system and as the means by which the economic rationality institutionalized in the economic system penetrates into other social subsystems. When money assumes a special role in the establishment of relations of differentiation and interpenetration, then the other subsystems must develop media with a similar capability if they are to maintain an equal status with regard to the processes of differentiation and penetration. That the development of generalized value-commitments permits the differentiation of the fiduciary system and the penetration of other areas of life by the cultural value pattern is one proof of the correctness of this statement of the function of the generalized media of interchange. It was not, therefore, from the purely economic properties of money that Parsons extrapolated when he formulated the theory of the generalized media of interchange—it was instead the logic of the analysis of the relation of interpenetration, which always undergirded the theory of action. It was the classification of the theory of money within this theoretical framework which opened the way to the general theory of the media of interchange. Without the help of this framework, there would have been no bridge from money to

the other media of interchange.¹⁸ The theory of media represents a new level of theoretical explanatory ability if it allows us to use the institutionalization of generalized and symbolic media which guide action to explain higher levels of the integration of individual freedom and social order than we were able to account for previously. The questions which the theory of media allows us to formulate are bound to the same fundamental normative idea of modernity which was present in the theory of action from the beginning: "This is the theme that the media constitute mechanisms whereby an action system is able to achieve a new level of 'value-added' combination. This implies, on the one hand, a freedom of action for its individual component member-units, but on the other hand, new mechanisms of control which make the functioning of such freedoms feasible at increasingly generalized levels" (Parsons 1977g, p. 130).

The swallowing up of economic theory by the general theory of action opened up for Parsons a new way to refine his theory, a way which indicated to him the future development of the theory. In the wake of *Economy and Society*, Parsons added to the theory, as further media of interchange, political power for the political system (1969b), influence for the societal community (1969a), and value-commitments for the cultural system (1969c). Through all of this, he remained firm in the idea that these media of interchange were to be viewed as mechanisms which secured the interpenetration of subsystems and their simultaneous differentiation. A characteristic example of this conception at work is the analysis of the "political support system" as a zone of interpenetration of the political system and the societal community (Parsons 1969b, pp. 373-83, 399-404; 1969d; Parsons and Platt 1973, pp. 426-34). This zone of interpenetration forms in the relation between citizens and political representatives. There is both an interchange of factors (the demands of interests and policy decisions) and an interchange of products (political support and leadership responsibility), which are mediated by political power and influence. As long as the articulation of political interests relies on the wielding of influence, which was created and generalized to serve the societal community, the articulation of political interests is not simply a question of naked power, but is connected to the legitimization of the community. The articulation of interests provides the matter which is given form by the reliance of political decision making on political power—for example, by the reliance on majority rule. The more political support is conferred according to the criterion of the performance of the political representatives on behalf of the societal community, the more is the acquisition of political power determined by the order of the community. Conversely, leadership responsibility is not an affair of pure power politics either, at least to the extent that it must rely on influence in

¹⁸ It is misleading to place the "economic analogy" in the foreground of Parsons's theory of media without making qualifications.

order to be effective for more than a short time. The political power which can be thus acquired on the political market, as it were, is here the matter which is given form through being connected to the criteria of the acquisition of influence (see fig. 5).

B. The Interpenetration of the Action Subsystems: *The American University*

The elaboration of the theory of the generalized media of interchange in terms of the social system came to a close in 1968 with the publication of Parsons's essay on value-commitments. The development of the theory from here on took place on the level of the system of action as a whole. Parsons worked out this part of the theory in the course of the comprehensive examination of the educational system, especially the university, as the core of the cognitive and professional complexes, an examination he made between 1968 and 1974. Here the generalized media of interchange became "definition of the situation" for the cultural system, "affect" for the social system, "performance capacity" for the personality system, and "intelligence" for the behavioral system.¹⁹ What this theoretical accomplishment means for the analysis of relations of interpenetration is shown most clearly by the theoretical consideration of the cognitive complex in *The American University* (1973), a monograph coauthored by Parsons and G. M. Platt. On the level of the system of action, the university appears as the core of the cognitive complex, which forms a zone of interpenetration of all four action subsystems (Parsons and Platt 1973, pp. 33-102), with priority given to cognitive rationality, and which includes all four media of the action system in relations of interchange.²⁰

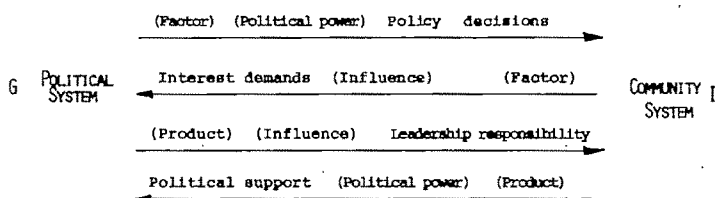


FIG. 5.—Interpenetration of political and community systems (based closely on Parsons 1969b, p. 399).

¹⁹ Cf. Parsons's earlier formulation of the theory of interpenetration on the level of the general system of action before the formulation of the theory of media (Parsons 1959).

²⁰ Warner (1978) advocates an expansion of the theory of action to include cognitive elements, without, however, taking up Parsons's work on the cognitive complex. The cognitive aspect of action is already integrated into Parsons's theoretical model. Warner is suggesting that the cognitive orientation toward norms and/or common cognitions

Cognitive learning and the acquisition of competence are the primary achievements of the personality, and these in turn require the application of performance capacity as a generalized medium, which determines the ends of action in this learning process. The acquisition of cognitive competence presupposes further capacities, which do not lie within the sphere of the personality system. There must also be present an obligation to standards of rationality, which can come about only through their institutionalization in a social system and through the attachment of the individual to the group. The penetration of the social system into the other subsystems requires that it be generalized through a medium of interchange that we can call affect. The affective grounding of cognitive rationality, the generalization of affective ties as the medium of solidarity relations, and the role these processes play in the development of a pluralistic society—these are the real subjects of Parsons and Platt's study of student socialization. Every learning process presupposes, to the extent that it is intended to be generalizable and communicable, the penetration of that process by a common framework. In the case of cognitive learning, this is the function of knowledge which is anchored in the cultural system and therefore lies outside the personality system. The penetration of cultural knowledge into different personality systems requires a "relay mechanism" which permits abstraction from concrete knowledge and which also preserves the identity of this knowledge, not allowing it to be completely "personalized." This is possible only through a generalization of knowledge beyond concrete situations; definition of the situation as a generalized medium of interchange has to develop as a response to this need. It guarantees a common overarching perspective on all concrete situations in which knowledge is instantiated. Knowledge can then be specified and applied to various problems without losing its identity. It can take on a multiplicity of specifications while its essential core stays consistent. Here again, zero-sum conditions are overcome through the control of the relations of interchange by generalized media. Beyond these specific capacities of the personality system, the social system, and the cultural system, cognitive learning also requires certain other capacities—the ability to perceive, to symbolize, to remember—which originate in the behavioral system. Progress in cognitive learning can be made only when these abilities are united with the capacities anchored in the other subsystems. That means that these capacities must also be generalized to be able to enter into multifaceted relations with the concrete products of the other subsystems—

could provide just as adequate foundation for order as moral obligations and common norms. But purely cognitive orientations toward norms would provide only a contingent, factual order, such as can exist only in limiting cases. Common cognitions can exist only where the validity of common norms has already been accepted. Warner's attempt to interpret Calvinism as an example of a purely cognitive system which yet provides a foundation for order ignores these moral and societal fundamentals (cf. Parsons 1978a; Pope and Cohen 1978; Münch 1980c).

concrete knowledge, concrete obligations to rationality, concrete personal goals—and use them as matter on which they impose a form. This generalizing capacity we can call intelligence; it raises the level of penetration of the cultural, social, and personality systems by the behavioral system (see fig. 6).²¹ Clearly, achievements in the area of knowledge are based only partially on pure intelligence. It is not the case, as is still widely believed, that given complete equality of opportunity, the intelligence of individuals would become the sole factor in the distribution of income, position, power, and prestige.

The development of generalized media of interchange as a mechanism for connecting differentiated subsystems is here also a condition of the interpenetration of subsystems. The media allow the subsystems to develop simultaneously without mutual interference and thus provide a crucial prerequisite for the development of the cognitive complex in general and cognitive competence in particular. On the level of the general system of action, the theory of media is a significant theoretical tool for the exploration of relations of interpenetration. How these relations of interpenetration are

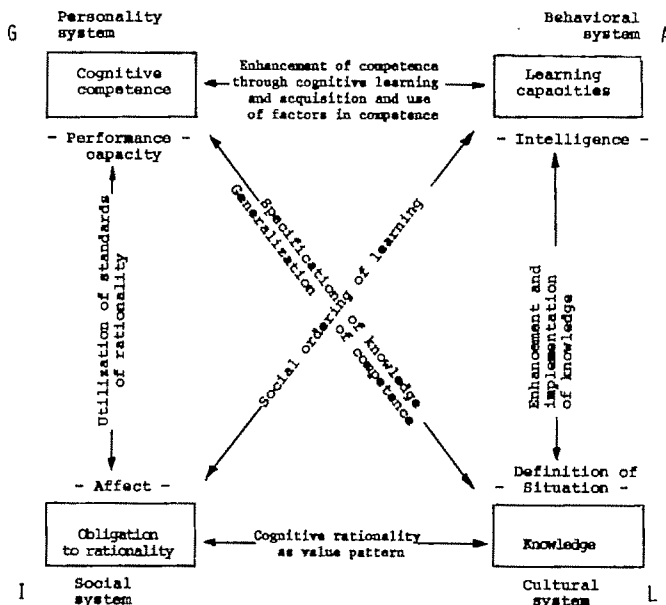


FIG. 6.—The zones of interpenetration of the cognitive complex as cultural-adaptive aspect of the action system (based closely on Parsons and Platt 1973, p. 57).

²¹ The introduction of the behavioral system as a replacement for the behavioral organism in the general system of action goes back to Lidz and Lidz (1976). The organism then occupies the place of the goal-specifying subsystem (G) of the human condition system (Parsons 1978d).

shaped by the interchanges controlled by the media can be seen in, for example, the relation of interchange between the cultural system and the behavioral system. The cognitive standards of validity and significance are cultural presuppositions which through a generalized definition of the situation enter the behavioral system as input factors. Conversely, the use of intelligence undergirds these cultural standards as a condition of their application, and in this way the special capacities of the behavioral system penetrate the cultural system. Also, the special capacities of the behavioral systems are carried over into the cultural system as product through the selection of knowledge, while conversely the generalized definition of the situation puts its mark on the formation of all knowledge and thus makes possible the cultural penetration of the behavioral system. The development of knowledge is in this sense a product of the interpenetration of the cultural and the behavioral systems, an interpenetration controlled by definition of the situation and intelligence as media of interchange (see fig. 7).

In their investigation of the American university, Parsons and Platt use this method of analyzing relations of interpenetration to explore the possibility of certain crises, either of inflation or deflation, which may afflict the various media (Parsons and Platt 1973, pp. 310-45). In so doing, they demonstrate the aptness of this theoretical schema for clarifying dynamic processes. The modern educational institution can be viewed as an intelligence bank, and specific educational programs can be viewed as firms in which intelligence can be invested, in order to increase the "supply" of intelligence or to produce knowledge. Parsons interprets the crisis which struck American education in the 1960s, the high point of which was the student protests of the late sixties, as the consequence of an inflationary phase followed by a deflationary phase which led to a new consolidation of the educational system. The period of student protest was preceded by a period of rapidly increasing investment of intelligence in the educational system. Intelligence was expended in a great undertaking to expand cognitive competence. This led to an overinvestment in cognitive rationality, creating expectations which educational institutions were unable to satisfy, particularly since

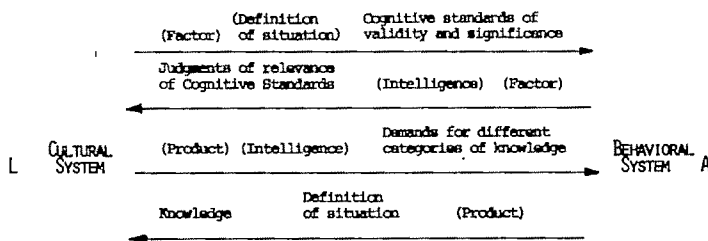


FIG. 7.—Interpenetration of cultural and behavioral systems (based closely on Parsons and Platt 1973, p. 75).

these expectations had a moral and expressive component. This was an inflation of intelligence; intelligence lost its worth, in the sense that an increase in the supply of intelligence made available for cognitive ends was not accompanied by a corresponding increase in the ability to "cash in" the increased expectations.

The consequence of the decrease in value of investments in cognitive intelligence was the withdrawal of invested intelligence from cognitive concerns and its "deposit" in intellectual attitudes which met moral and expressive needs. This withdrawal of intelligence from cognitive undertakings was a deflationary phase, which manifested itself in decreased expectations of intelligence invested in cognitive concerns. This in turn led to a shortage of investments of intelligence in the cognitive undertakings of educational institutions. The characteristic expression of this deflationary phase was the student protest, in which the students withdrew their adherence from the university and invested their intelligence in moral and expressive undertakings. The ebbing of this phase led, in Parsons's understanding of it, to a consolidation which was marked by an increased investment in cognitive concerns and a new grounding of cognitive rationality in moral and expressive standards (Parsons and Platt 1973, pp. 346-88; Parsons 1978*c*, 1978*g*, 1978*h*, 1978*i*; cf. Smelser 1973). And it is only under such conditions as the increase in the long-term investment of intelligence—in which educational institutions play a special role as intelligence banks—that an increase in cognitive, moral, and expressive products is possible and zero-sum conditions are overcome. An increase in cognitive rationality in such cases cannot exclude an increase in moral rationality. Inflationary and deflationary developments strengthen the incompatibility between these two symbolic orientations and create a zero-sum condition between them. The analysis made possible by the theory of media clarifies the conditions under which the threshold of incompatibility can be pushed back and zero-sum conditions can be overcome. Therefore the theory represents a real step forward in the working out of the fundamental ideas which were present at the very beginnings of the theory of action.

C. The Interpenetration of the Subsystems of the Human Condition:
Action Theory and the Human Condition

In his last years, Parsons added the final building block to his theory of action (Parsons 1978*d*): the extension of the theory in order to understand the interpenetration of the system of action with its environment. The whole framework was now entitled the system of the human condition, in which the system of action performed the integrative function, the telic system performed the latent pattern-maintenance function, the system of the hu-

man organism performed the function of goal attainment (selection), and the physicochemical system performed the adaptive function. Again, evolutionary development signifies here the interpenetration of these analytically delimited subsystems, which is facilitated by generalized media permitting the accomplishments of one subsystem to be carried over into the others. The media of the systems which constitute the environment of the system of action are not symbolic in nature, but they are generalized mechanisms, and it is this property which allows them to impose form on the concrete material of the other subsystems (Parsons 1978*d*, pp. 392–414).

The telic system is formed by the transcendental conditions of possibility of human existence. To this system belong, for example, space and time, the categories of human understanding, the categorical imperative, and the categories of judgment, in the sense which these terms have in Kant's transcendental philosophy. Here we also find the possible attitudes which man can take toward the world, attitudes which Parsons extracted from Weber's comparative sociology of religion and classified into four fundamental kinds by using the two axes of this worldly–otherworldly and ascetic–mystical. What we are talking about here is a typology of attitudes which as transcendental conditions determine the specific mode of the meaningfulness of the world. We are not saying that it is necessary for human life to be guided by one or another concrete, existing religion. We are instead talking about the more general assumptions that humans have made about the meaning of life and about the striving for meaning which is one of the fundamental characteristics of human existence. When we see today in the realization of the normative idea of modernity the culmination of the meaning of social development, this gives a form of legitimation to our institutions through the giving of a specific answer to very general questions concerning the constitution of meaning as a transcendental condition of our existence (Bellah 1970, 1975). The medium which is anchored in the telic system and makes possible the penetration of this system into the other subsystems of the human condition is transcendental ordering, to be understood as a generalized capacity to establish order which can be applied to any range of empirical objects whatever (Parsons 1978*d*, pp. 355, 397–401).

The system of action consists of action which is intentional, oriented toward meaning. The medium localized in this system is meaning, and the structure of the medium is provided by language. Man's orientation to the world which surrounds him is determined by symbols. The penetration of the other subsystems of the human condition by this fundamental character of human action is made possible by the fact that meaning as a generalized medium allows form to be imposed on any matter whatsoever according to the standards of symbol formation (Parsons 1978*d*, pp. 368–70, 396–97).

The human organism is to be understood as a goal-attaining system, the

relation of which to its environment is controlled by cybernetic feedback processes. The medium which operates here is health, which is a generalized expression for very different characteristics of specific parts of the organism. Health can be acquired, increased, and used up. It is the medium through which matter supplied by neighboring subsystems can be given form in accordance with the needs of the functioning of the organism (Parsons 1978*d*, pp. 401-3).

Finally, the physicochemical system is the least ordered subsystem of the human condition. It functions as a source of resources for the entire system, and it is subject to the control of the other subsystems, which attempt to make its resources useful for themselves. This control is exercised through the media of the other subsystems. Through the transcendental constitution of order, there first emerges a recognizable order of nature; through meaningful categorization and treatment, nature is rendered comprehensible. Through the medium of health, physicochemical nature is adapted to the demands of the organism. The medium which effects the penetration of the other subsystems by the physicochemical system is the empirical ordering, in the sense of a reality governed by empirical causal laws which, as particular, specific laws, are to be distinguished from the transcendental order of space, time, and causality (Parsons 1978*d*, pp. 397-400).

In the light of this theory of the human condition, every empirical phenomenon is the product of a degree of mutual penetration of these analytically differentiated subsystems, a penetration regulated by the appropriate media. The securing of a functioning state of the human organism requires, for example, the penetration of the adjoining systems by the medium of health, which orients these systems toward the needs of the human organism. This control by the conditions of the human organism cannot, however, overcome the internal laws of the other subsystems. Instead, they relate to the functional needs of the organism as conditioning factors, so that causal laws, transcendental order, and symbolization impose a form on the material of the organism through their respective media (see fig. 8).

We can illustrate these relations of interpenetration between the subsystems of the human condition through the example of the relation of interchange between the telic system and the physicochemical system. This relation, in fact, is the subject of Kant's *Critique of Pure Reason* ([1781] 1956). The order of nature is a product of the interpenetration of the telic and physicochemical systems, and one can break this interpenetration down into an interchange of factors and an interchange of products. In the interchange of factors, the categories of the understanding penetrate the physicochemical order through the medium of transcendental ordering and, conversely, sense data penetrate the telic system through the empirical constitution of order. In the interchange of products, the generalization of

knowledge as the achievement of the telic system penetrates the physico-chemical system, but this generalized knowledge is bound to the conditions of the empirical order of causal laws. Conversely, the orderliness of nature penetrates the telic system, but remains bound to the conditions of transcendental order (see fig. 9).

Parsons conceptualizes the relation between the telic system and the action system according to the model provided by Kant's *Critique of Practical Reason* ([1797] 1967; Parsons 1978d, pp. 411-13). The most fundamental conditions of the order of action, that is, the constitution of action as meaningful, are located here. The various rules that apply to human action are given a consistent meaning through their anchoring in this com-

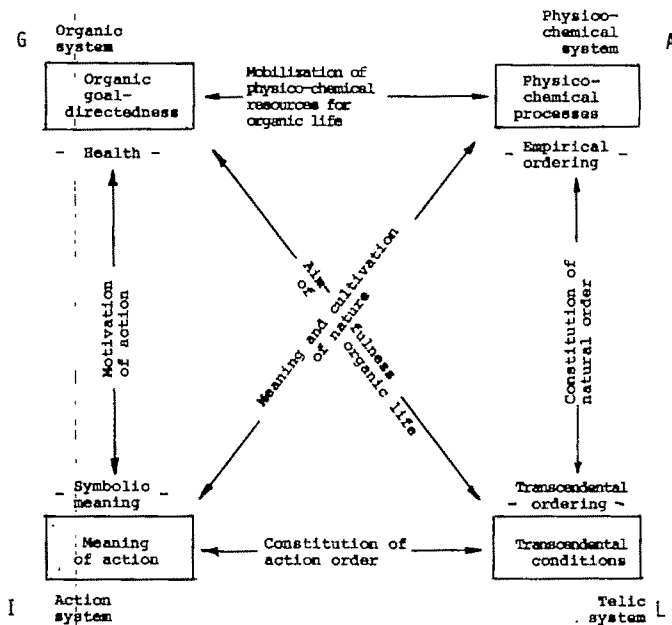


FIG. 8.—The zones of interpenetration of the human condition system

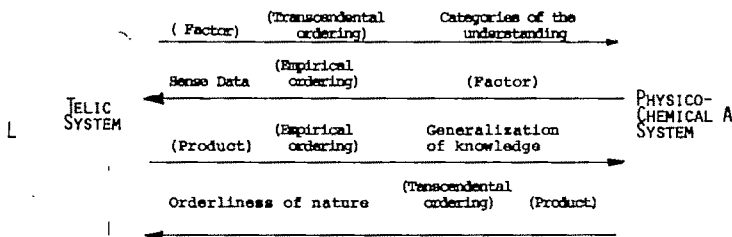


FIG. 9.—Interpenetration of telic and physicochemical systems (based closely on Parsons 1978d, pp. 407-8).

ponent of the human condition system. The obligatory character of the order of action results from its anchoring in the community component of the action system. This is the general sense of Durkheim's thesis that morality requires a religious foundation and, conversely, that religion requires a moral foundation (Durkheim [1924] 1953, [1912] 1954). In this context also belongs Weber's thesis that the orderedness of action is determined essentially by the solution that man at any given time has found to the problem of the constitution of meaning in the face of the tension between a perfect ideal order and an imperfect real world (Weber [1920] 1972, [1922] 1976).

Parsons likewise conceptualizes the relation between the telic system and the system of the human organism according to the model provided by Kant's *Critique of Judgment* ([1799] 1968). We are dealing here with the constitution of an organic order, with the goal directedness of organic processes within the framework of an encompassing order. The factual organic order is the result of the degree to which, for example, human intervention in organic processes is integrated with or cuts against the grain of the constitutive conditions of organic order.

Out of the relation between the system of action and the physicochemical system there arises "cultivated nature," nature as it has been shaped and formed by the human hand. To what extent in any concrete case order or lack of order is actually present depends on the extent to which man's treatment of nature is integrated into a consistent value system.

Henderson's conception of the "fitness of the environment" provides the model for the relation between the physicochemical system and the system of the human organism (Henderson 1913; Parsons 1978*d*, pp. 409-10). Like all systems, the organism and physicochemical nature can stand in various relations to each other, of which interpenetration is only one. For the human organism, interpenetration with physicochemical nature means the active use of this nature, but not its exploitation.

The interpenetration of the action system and the system of the human organism provides Parsons with a new access to the work of Freud (Parsons 1978*d*, pp. 410, 414-33; Freud 1972). Here we find the context for the organic element of the theory of socialization. The interpenetration of the structure of organic drives with intentional action is possible only through a successful socialization process, in the course of which the individual becomes capable of autonomous action as a personality. The personality, as a zone of interpenetration, is distinguishable both from the structure of organic drives, on the one hand, and from the material, social, and symbolic environment, on the other.

This final theoretical development is another of Parsons's attempts to work out, in a new context, the conditions which underlie the attainment of continually higher degrees of the integration of freedom and order.

7. APPLICATIONS OF THE THEORY OF ACTION

Talcott Parsons worked out the development of the theory which I have sketched here in a wide variety of contexts which we cannot look at individually here. Here belong, for example, the three essays on social stratification, the work on the development of modern society from an evolutionary perspective, and a large number of studies on particular problems in the study of religion, the modern professions, politics, economics, socialization, and other areas. All these works share the theoretical perspective I have set forth here, the core of which is the theory of interpenetration. This point is particularly worth making with regard to Parsons's work on social stratification and on the evolution of modern society, since this work is frequently misunderstood. It is this work which we will look at now.

A. The Theory of Stratification as an Application of the Theory of Action

Social stratification is the result of a kind of relation between the economic, political, communal, and cultural subsystems of social action. Social stratification as a system of legitimized inequality first arises from the mutual penetration of economic, political, and cultural dynamics and normative culture (Parsons 1940, 1954, 1977*d*). The arising of inequality, then, is ethically regulated and, conversely, the internal dynamics of inequality has an effect on the normative culture. Legitimate stratification arises in the zone of interpenetration between economic acquisitiveness, the acquisition of political power, the acquisition of professional competence, and communally grounded ethics. It can be explained neither purely economically, politically, and professionally nor purely ethically, but only through the interpenetration of these spheres. These are the terms of the problem which Parsons's theory of stratification set out to address from its very beginning (Parsons 1940). This theory is a logical consequence of the voluntaristic theory of action; in this area of application, the very statement of the problem serves as a counterthesis to utilitarian or power-oriented theories of stratification. Parsons's theory is also distinguished, however, from all purely functionalist theories of stratification, such as those of Davis and Moore. Around the latter issue there has grown quite a long-standing debate (Davis and Moore 1945; Davis 1953, 1959; Moore 1963*a*, 1963*b*; Tumin 1953*a*, 1953*b*, 1963; Buckley 1958, 1959, 1963; Levy 1959; Huaco 1963; Stinchcombe 1963; Lenski 1966). Incomprehensibly, this debate has proceeded with those concerned taking for granted that the hypothesis of Davis and Moore would be a more precise version of the older hypothesis of Parsons. The hypotheses, however, do not appear in general to have anything in common. Davis and Moore look for

the cause of the universality of stratification, in the sense of differential rewards in their functional value for the society, and they seek to explain concrete stratification structures in terms of the functional significance of roles for a society and the relative scarcity of people available to fill the roles. There are certain obvious objections to this theory. The consequences of concrete stratification structures, for example, the social conflict they engender, are not always functional, so that we cannot in general pronounce that stratification is always functional. The contribution of a given stratum to the persistence of a society can be determined neither by the actors involved nor by an observer. Therefore, the "correct" distribution of rewards is inaccessible to human comprehension; its appearance in a concrete society would be a miracle which we could not even recognize. The functionalist theory in this form is a modern version of the organic theory of society, which has the ideological consequence that everyone is perceived as in his place and as given the proper reward according to his contribution to the social whole. The existing structure of stratification is always the best possible for a given society.

Now, Parsons's theory goes in a completely different direction. As an antiutilitarian theory, it makes a basic distinction between quasi-naturalistic stratification structures (which simply *are*) and stratification structures which can make a claim to justification. A stratification structure can be justified only to the extent that its distribution of income, power, expertise, influence, and prestige follows rules which are themselves incorporated in a generally shared system of values. Concrete stratification structures are not, according to this hypothesis, simply justified functionally by their existence. They require justification, and attempts at justification can succeed or fail. The entire debate over the functionalist theory of stratification has overlooked this central problematic of the voluntaristic theory of stratification. Instead, the debate has run itself into the dead end of two false alternatives: the organic theory of society on the one hand, and the positivistic theory based on power and conflict on the other.

B. The Theory of Evolution as an Application of the Theory of Action

From the middle of the 1960s, Parsons increasingly applied himself to the task of incorporating the modern theory of evolution into his account of social-cultural evolution (1966, 1967*c*, 1971, 1977*c*). Although from 1964 on he devoted himself to the problem of evolution, he was far from committing the mistakes Spencer had made. In 1937, he had begun his critique of the positivistic-utilitarian theories represented by Spencer with the words "Spencer is dead" ([1937] 1968*b*, p. 3). It was not the theme of evolution

as such, but the positivistic-utilitarian theory of Spencer against which Parsons had arrayed Weber, Durkheim, Marshall, and Pareto. Parsons came around to the idea of evolution again on the basis of the voluntaristic theory of action in the 1960s. But one can understand what Parsons does with the notion of evolution only if one keeps the perspective of the general theory in mind. Evolutionary development is to be understood as the result of differentiation *and* interpenetration, through which limitations are overcome and more room can be opened up for the operation of various analytically differentiated subsystems, between which there existed sharper incompatibilities at the lower level of evolution. The characteristics of modern society—such as the market system, the modern bureaucracy, universalized law, democratic associations, or the modern professions—are not, as “evolutionary universals,” differentiated and isolated subsystems of society, but are instead products of interpenetration of inherently opposed forces, and in this form—on this point Parsons aligns himself with Weber—are found only in the West.

Within the framework of the voluntaristic theory of action, in which “meaning” is the constitutive characteristic of human action and “understanding” (*verstehen*) is the corresponding method of explanation, sociocultural evolution, the evolution of action, cannot be understood as a quasi-naturalistic process (Parsons [1937] 1968*b*, pp. 583–85, 588–89, 634–39, 641–42, 681, 764–65; 1967*g*, 1970, 1978*d*, pp. 382, 389–90, 393, 397, 403; 1980). Parsons had already refuted a version of such a theory in 1937, in his critique of Darwinism as a radically anti-intellectualistic variant of positivism ([1937] 1968*b*, pp. 67–68, 80–86, 110–14, 122–25, 219–28). Parsons held to this position up to the very end of his career. That means that we must give to the general categories of the theory of evolution a meaning consistent with the theory of action. Indeed, we can understand sociocultural evolution as a sequence consisting of the constitution of genotypes out of a gene pool, the reproduction and variation of the genotypes, the construction and selection of phenotypes, and, finally, the feedback effect of this process on the composition and structure of the gene pool (Mayr 1970; cf. Giesen 1980, pp. 56–76). But what is important here is that these terms be understood strictly in terms of the theory of action. So, by “gene pool” we understand a value system, such as the modern occidental value system rooted in Judeo-Christian culture. The constitution of the genotypes corresponds to the interpretation of this value system, their reproduction and variation to the traditionalization and socialization of the interpretation and to innovations in it. The construction and selection of phenotypes corresponds to the institutionalization of interpretations in social systems and their internalization in personality systems. As the theory of selection forms the core of every theory of

evolution, so the theory of institutionalization and internalization forms the core of the theory of social-cultural evolution (Parsons 1977e, pp. 5-7; 1977f, pp. 110-16; Münch 1980c). That means that this theory of evolution grows out of a voluntaristic core.²²

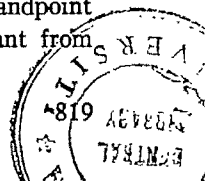
More specifically, we can say that the selection of phenotypes in this system is determined by the kind of relation obtaining between the interpretations of the value system as the L component of the four-dimensional action framework and the other three poles: community (I), goal attainment (G), and the articulation of interests (A). Selection is determined, in its content, by the content of the interpretations (L) and the content of the societal verities (I), goals (G), and interests (A) involved, as well as the relations between all of these, which may cover the full range of possible relations between potentially ordering and dynamizing systems: reconciliation, mutual isolation, interpenetration, domination of one system by the other. Evolutionary development presupposes interpenetration, which leads to higher orders of complexity. The selection of a phenotype requires the communal anchoring of the interpretations of a value system, their rational specification to goals and the normative limitation of interests by the values, as well as the opening of the value system to the articulation of interests, the demands of goal attainment, and the duties which the societal community holds to be self-evident. From the perspective of goal-oriented societal collectivities and personalities, which approach the G pole of the action framework, evolutionary development means increased interpenetration with the material, social, and symbolic environment. Corresponding to these three dimensions, one can understand the three concepts of "adaptive upgrading" (G-A), "value generalization" (G-L), and "inclusion" (G-I) as so many specifications of interpenetration. The fourth concept, "structural differentiation," refers to the product of this increasing interpenetration: this concept denotes a normatively integrated form of differentiation, the mechanism of which is the formation of continually expanding zones of interpenetration between the components of the action space (Parsons and Platt 1973, pp. 169-81; Parsons 1977c). We can see that a voluntaristic theory of evolution includes as essential components not only constellations of interests and power, but also the process of the formation of community and the processes of the discursive grounding of norms. The kind of relation which obtains among these elements, how much weight each has and what effect each has on the others, can be discovered by using the model of possible relations between the subsystems of action.

²² Explanation within the framework of such an evolutionary theory of action cannot, therefore, have a purely teleonomic character, as Bershadsky (1973, 1977) assumes. Cf. Parsons 1977g, pp. 130-32; Münch 1980c.

8. CLOSING REMARKS

Talcott Parsons possessed a deep faith in the fundamental institutions of American society. This faith showed itself most clearly in his concrete analyses. These analyses bear witness to a great faith in the ability of those institutions to regenerate themselves in the face of crisis. We can see this in his analyses of the political system, the integration of blacks, adolescents, the family, the structure of personality, the educational system. And, not infrequently, one can recognize the model of American society in Parsons's analytical theory construction also. For example, the purely controlling function which Parsons ascribes to the cultural system is a product of the interpenetration of intellectual culture and the white Anglo-Saxon Protestant societal community which is typical of American society. In continental Europe, however, the two spheres have remained quite distinct, with the result that a pure intellectual culture has appeared, and this culture has been able to have a much more dynamic effect (Tocqueville [1835/40] 1966). A social scientist might use this fact to accuse me, a European sociologist, of "European bias" in the assignment of a dynamizing function to the cultural system relative to the societal community. Does the detection of such biases invalidate the theory of action? No; what is important is that the very terms which we use to detect and pinpoint these biases are the terms of the theory of action itself. The power of the theory of action derives in no small part from its ability to turn back on itself and self-critically locate, and thus neutralize, the biases inherent in any given formulation of it. No social theory is independent of a normative foundation. The theory of action is able to identify its own normative grounds and demonstrate precisely how and where they function; far from giving us ground for rejecting the theory, this demonstration anchors us even more firmly within it.

The "American biases" within Parsons's sociology do not hinder us from separating analytical theory from empirical assertion and from deriving from both a set of results such as has been yielded by no other sociology since Weber and Durkheim. There is no comparable theory in sociology today which has established such a depth and breadth in the understanding of change, conflict, power, authority, and other areas of study which have been favored by "critical" sociologies. All attempts to oppose Parsons with a theory based on conflict or with some other form of "critical" theory fail from the start; these attempts only go to show how little Parsons's criticism of the positivist tradition has been understood, because they imply nothing other than a Hobbesian variant of positivism (Dahrendorf 1958a, 1958b; Mills 1959; Collins 1968; Gouldner 1971). And all attempts to argue against Parsons's normative theory of order from the standpoint of "modern" economic theory look like attempts to criticize Kant from



the standpoint of Hume's empiricism (Coleman 1971; Ellis 1971; Vanberg 1978).

Parsons's theory of action is the perspective which has allowed the most fruitful interpretation of the classics of the field: Durkheim, Weber, and Freud.²³ This is especially true in the case of Max Weber. While a substantial number of readers of Weber try to extract from his works the correct understanding of the "dialectic" of ideas and interests and his "individualistic" theory of action, and to oppose this Weberian position to what they take to be the sociological reductionism of Parsons, Parsons developed long ago a theory which unites the contributions of Weber, Durkheim, and Freud in a general theory of action.

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²³ The criticisms directed by Pope, Cohen, Hazelrigg, and Collins against Parsons's thesis of the convergence of Durkheim and Weber in the voluntaristic theory of action clearly overemphasize the element of sociological positivism in Durkheim and the element of positivistic-individualistic theory of power and interest in Weber (Collins 1968; Bendix 1971; Pope 1973, 1975, 1977; Cohen 1975; Cohen, Hazelrigg, and Pope 1975; Pope, Cohen, and Hazelrigg 1975; Parsons 1972, 1975, 1976). This is a reductionist interpretation of Durkheim and Weber. Parsons has already shown that the two variants of positivism can account for the existence of factual order, but not normative order. Durkheim cast off the concept of sociological positivism through the notion of moral obligation. Weber cast off pure idealism early, but he did not work within the framework of an individualistic positivism either. If one assumes such a framework, Weber's studies of the development of modern society cannot be coherently reconstructed. The more recent attempt by Schluchter (1976, 1979, 1980) to interpret Weber's work consists essentially in a combination of a logic of the development of rationalization and a positivistic-individualistic theory of power and interests. The logic of the development of rationalization as a world view was first set forth by Tenbruck (1975) in his interpretation of Weber's sociology of religion. This idealistic interpretation is as one-sided in its way as are the theories which rely solely on power and interests (Münch 1980a, 1980b).

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The Age Structures of Occupations and Jobs¹

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Age segmentation in the labor force can be analyzed in terms of the age distributions of occupations. In this paper we show that the majority of detailed census occupations conform to one of five basic age profiles. Further, these age profiles have meaning as they derive from the operation of well-defined institutional forces. We discuss the relevance of industry for a refined understanding of occupational-age patterns and conclude with some observations about the likely consequence of a change in the age of compulsory retirement for the age distributions of different occupations.

In attempts to comprehend the process of individual achievement, increasing attention has been given to job change behavior (Parnes 1970; Doeringer and Piore 1971; Stinchcombe 1975; Leigh 1976). The importance of this consideration is apparent with respect to status attainment since, by definition, a change in occupational status can occur only as a result of a job shift. This is not the case with earnings, which can evolve during the course of employment in a single position, yet job switching remains an important mechanism for securing earnings growth over the life cycle. There is evidence, for instance, that a job shift is frequently accompanied by an increase in salary (Ornstein 1976, pp. 109–29; Parnes et al. 1974, pp. 103–13). There is also reason to suspect that, especially among young workers, job changing is used to situate oneself favorably with respect to long-term earnings prospects, if not immediate returns (Spilerman and Miller 1977).

Job changing is hardly a random phenomenon, in regard to rate of movement or choice of destination. Rather, characteristic patterns can be asso-

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ciated with particular origin occupations and industries (Palmer 1954, pp. 62-80; Reynolds 1951, pp. 28-37). This presence of determinable avenues of movement is another way of saying that the labor market is "Balkanized" (Kerr 1954), "segmented" (Piore 1975), or organized into "career lines" (Spilerman 1977), in the sense of there existing sequences of positions with high rates of being traversed. Irrespective of terminology, the implication is that the labor market is structured with respect to movement, so that, for most workers, job changing is not a chaotic process.

If job shifting is an important mechanism in status and earnings attainment, and if this behavior is patterned, it would appear desirable to understand the linkages which exist between jobs. Indeed, the notion of labor market *structure* is not alien to occupational sociology (though an interest in movement patterns per se has been only an incidental concern). Thus, with respect to the professions and craft occupations, there is the concept of a "life-time commitment" (Caplow 1954, p. 107; Ritzer 1972, p. 203), which implies a low rate of occupational change. In contrast, descriptions of careers of foremen and managers typically contain lengthy discussions of prior and subsequent occupations (Sofer 1970, pp. 234-51).

The traditional approach to labor market structure has been to examine organizational arrangements and institutional rules which impede job changing or, conversely, define conduits along which individuals do move. With respect to craft occupations, for instance, it has been noted (e.g., Stinchcombe 1959) that the number of entrants into a union is limited, a union is commonly coterminous with a single craft, employment is obtained through union referrals, and duration with an employer tends to be brief. In this circumstance, a considerable investment is built up by a worker in his craft and in its union, and a low rate of occupational turnover can be expected. In industrial firms, by comparison, jobs are organized into hierarchical sequences (job ladders) and seniority with the firm is a principal consideration in promotion, as well as a determinant of job security and pension rights. Employment therefore tends to be of long duration and is punctuated by periodic occupational shifts.

If the traditional approach to labor market structure emphasizes the institutional arrangements which underlie movement patterns, a second strategy would be to focus directly on the transition probabilities among jobs. This approach would complement the study of norms and rules in that information would be obtained on behavior, as well as on prescription. Yet, the task of delineating movement patterns is not simple, even when attention is restricted to transitions between pairs of jobs, rather than full historical sequences (careers) being examined. Because transferability of work skills often is limited to technologically related occupations and industries, detailed occupation and industry categories are necessary in order to cap-

ture the fine structure of movement. (In aggregate classifications technologically proximate categories tend to be grouped together, which results in an underestimation of the true amount of movement.) Also, most jobs lead to multiple destinations, and the probabilities of transition to the various destinations change with a worker's age. These sorts of considerations complicate the endeavor of describing the structure of work careers from job change data, but see Spilerman (1977) and Rosenbaum (1979) for attempts at such delineations.

As an alternative to examining job transitions directly, we consider one of the implied consequences of a labor market in which movement is patterned. If determinable job sequences exist, such linkages should find expression in the *age distributions* of occupations and jobs.² Some positions, at the entry level into career lines, should have a concentration of young workers; others, at the apex of career lines, should contain an overrepresentation of middle-aged persons. Conversely, given the presence of a diversity of occupational-age distributions, one may be able to infer particular movement patterns; *thus, the age characteristics of work positions can serve as an instrument for probing labor market structure*. An investigation of the age features of occupations should be informative about more than job linkages, since occupations with similar age profiles may have in common other aspects of the organization of work as well. Further, an understanding of age segmentation is pertinent to forecasting some societal consequences of a change in the age of compulsory retirement, a topic currently of public concern.

In this paper we examine the age distributions of occupations and jobs to ascertain what these facts may indicate about labor market structure and ancillary matters.³ We first outline the mechanisms which generate different occupational-age patterns and provide illustrations of the occupations that belong to each age category. This account is then supported by a formal analysis of the causal forces which contribute to the observed age patterns. In the concluding sections we sketch some complications which arise when considerations of industry are introduced, and review policy implications which derive from this study.

² Where we distinguish "job" from "occupation" we shall mean by job an occupation \times industry position. Common parlance would associate job with an occupation \times employer position, but lacking data at the employer level we use three-digit industry codes as a first approximation. (Thus we exploit the fact that firms in the same industry tend to employ similar technologies; as a result, much of the interfirm variation in the organization of work can be attributed to industry differences.) Where confusion is unlikely, we shall use "job" interchangeably with work position or occupation.

³ For discussions of occupational age structures from the special perspectives of counseling and industrial psychology, the reader is referred to Griew and Tucker (1958), Murrell (1962), Richardson (1953), and Smith (1968, 1969, 1973).

MECHANISMS WHICH GENERATE OCCUPATIONAL-AGE DISTRIBUTIONS

The presence of a variety of age distributions among occupations derives from the operation of multiple factors. To cite a few: (1) Jobs differ in task demands, which make them differentially attractive to the various age groups. (2) In large organizations, work positions are commonly arranged in hierarchies (job ladders) in which progression is unilineal; the age-grading of occupations is one consequence. (3) The replacement of old technologies by more efficient ones can generate a decline in demand for labor; often this is implemented via low rates of hiring into contracting occupations (and industries), with a consequent increase in the average age of incumbents. We now review considerations of these sorts.

Physical Requirements of Tasks

Occupations differ in the extent to which the work is physically difficult or exhausting. Some manual jobs (e.g., warehouseman, electrical lineman, fireman) necessitate exertions of the kind that can be performed on a daily basis only by the able-bodied. We expect these occupations to exhibit an age distribution skewed to the young end, as workers in middle age transfer to other lines of endeavor. There also are occupations which, while not exhausting, require quick reflexes and a high level of motor coordination (e.g., airline pilot, professional athlete). The need for such abilities should also result in truncation of the age distribution at the upper end.

Retirement Rules

Closely related to physical requirements of tasks are formal rules that compel retirement by a certain age. In some instances, retirement rules are intended to insure that, where performance declines sharply with age (e.g., airline pilot, policeman), employees depart from the occupation rather than reduce their quality of work. Yet retirement rules are more pervasive than can be accounted for by a narrow consideration of task demands. Large bureaucracies have age ceilings even when little justification is apparent in terms of work requirements. Rather, the reasons appear to be ones of discouraging rancorous conflict between age cohorts over the timing of succession (*New York Times* 1977, p. 38), permitting the infusion of young talent and new ideas into an organization (Breen 1966, p. 386), and enabling a firm to divest itself gracefully of workers who have reached an age in which illness is likely to take an increasing toll in work regularity (Report 1973, pp. 69-70).

While the age specified in retirement rules places an upper bound to the age distribution of an occupation, the typical age of departure may be

younger. Considerations relevant to early retirement decisions include pension plan features and skill transfer prospects. Pension plan features are consequential because retirement plans differ in the length of service at which attractive benefits become available (instances of extreme generosity are the plans of postal workers, military personnel, and New York City policemen and firemen; typically they permit retirement after 20 years employment at one-half the terminal year's salary). Skill transfer prospects are relevant because many early "retirees" wish to remain in the labor force and initiate retirement only after assuring themselves of other job opportunities.⁴ (For example, the availability of watchman and bank guard positions for former policemen probably contributes to a high rate of early retirement from that occupation.)

Work Setting

If many occupations have formal rules which compel departure by a certain age, we can also suggest the kinds of positions which permit individuals to remain in the labor force until a comparatively late age. Elderly workers require occupations in which they can vary the hours of employment and set the rate of work flow according to their daily condition. The most obvious circumstance in which this is possible pertains to free professionals (doctors, lawyers) and self-employed businessmen, such as retail shopkeepers. Some employment situations permit similar flexibilities: Realtors and insurance salesmen are often paid on a commission basis and tie down few organizational resources (machinery, office space); employers therefore might not be averse to a leisurely work tempo for individuals in these occupations. Building janitors provide another example of an occupation in which employees have latitude in scheduling tasks.

Education and Training

To this point we have discussed job features which constrain occupational-age distributions at the upper end of the age continuum. For some occupations, however, the age range is truncated at the lower end, as a consequence of education and training requirements. This is most apparent with respect to professions in which graduate study is a prerequisite for certification (e.g., medicine, law, college teaching), though lengthy training periods are not unusual in blue-collar crafts, where they take the guise of apprenticeship programs.

⁴ More generally, workers may delay changing employers until they have accumulated enough years of service to qualify for a pension, or until rights to a pension have become vested.

Job Linkages

In discussing organizational requirements we have stressed constraints on age of input (education and training prerequisites) and limits to continued membership (retirement rules). We wish now to emphasize age patterns which arise from a different kind of organizational consideration, the division of labor and linkages among jobs within a firm. In large industrial enterprises, work positions are arranged in hierarchies (job ladders), with service at one occupational rung a prerequisite for employment at the next higher level. In part, the presence of job hierarchies reflects the need for acquiring skills and experience before advancement; in part, it results from pressure by unions to reserve upper-level slots for currently employed workers (internal labor market). Whatever the explanation in a particular firm, the consequence for occupational-age structures is that each position in a job sequence will have a narrow age distribution, with the mean age increasing over the successive positions.

Additional Institutional Considerations

Occupations that are linked together by a promotion ladder should have narrow age distributions. Institutional arrangements of another sort associate a "life-time commitment" with many professions and craft trades (Ritzer 1972, p. 203). Provided that these occupations are neither expanding nor contracting in employment, their age distributions should be flat, relative to the age distribution of the total labor force. An occupational-age structure of a still different kind is expected for positions that belong, institutionally, to the "secondary labor market."

The concept of a "dual labor market" is associated with the writings of Doeringer and Piore (1971), although the central ideas appear in earlier publications by Kerr (1954) and Dunlop (1957). The basic notion involves the dichotomization of jobs according to whether employers invest resources in the training of workers (primary labor market) or refrain from doing so (secondary market). With respect to positions of the former type, hierarchical job sequences and the provision of periodic salary raises constitute the reward structure in terms of which employers remunerate workers for training and experience. In contrast, the secondary sector consists of jobs for which employers choose *not* to upgrade worker skills. As a result, employees with several years of experience are no more attractive to an employer than new entrants into the firm, and there is little reason for the salaries offered to the two groups to differ. Returns to seniority are minimal; in this sense jobs in the secondary sector can be said to lack a future.

Because jobs in the secondary labor market are easy to enter and because

new entrants are soon on an equal footing with veteran employees, secondary positions are often turned to for second careers, such as when task difficulty compels a job change. These positions should therefore have an overrepresentation of older workers. However, many secondary positions (e.g., gas station attendant, dishwasher, food service worker) also appeal to young individuals who have not yet committed themselves to a line of work or for other reasons desire intermittent employment (Doeringer and Piore 1971, pp. 170-80). In comparison, we expect a low rate of representation in these jobs by workers in their prime years, when they are able-bodied and have embarked upon careers.

Demographic Factors

New occupations are continually being created, accompanying the development of emergent technologies (e.g., computers, television); other occupations decline in employment as their functions become obviated by technological advance (e.g., blacksmith, railroad fireman). Employees in expanding occupations, especially when specialized training is required, tend to be drawn from recent entrants into the labor force; such occupations should therefore have a young age structure. Contracting occupations, in comparison, evolve by not replacing workers who have departed and should have an age distribution skewed to the old end. These comments pertain to the initial years of occupational growth or decline. Eventually, growth slows in expanding occupations, and replacement becomes necessary in contracting ones. The consequence of such changes in the rate of recruitment is to generate "waves" in the age distribution of the occupation; passage of these waves over the age range may be observed in successive decennial population censuses (e.g., Smith 1973).

Summary

From the preceding comments it is evident that systematic forces of an institutional and a demographic nature operate on occupations and are capable of creating a diversity of age patterns. Extrapolating from these remarks, we expect to find evidence for five types of occupational-age distributions:

1. Occupations in which young workers are overrepresented. These should include entry-level positions in job sequences and occupations organized around emergent technologies.
2. Occupations in which middle-aged workers are concentrated. We expect senior positions in job sequences (supervisors, foremen, managers) to fall in this category.
3. Occupations in which the elderly are overrepresented. These should

be primarily the sorts of jobs which permit a worker flexibility in setting his rate of work and scheduling hours of employment. We also expect contracting occupations to be in this category.

4. Occupations with a uniform age distribution. We expect to find the free professions and craft occupations here, since affiliation with these positions tends to be of long duration, spanning much of an individual's work life.

5. Occupations with a U-shaped age distribution. This category should contain jobs of low desirability, having poor advancement prospects.

We wish to emphasize that several of the factors we have enumerated as influencing the age distribution of an occupation may operate simultaneously, making a unique assignment of it to one of the five categories an impossible task. For instance, dentistry is a profession characterized by a lifetime commitment; hence, a uniform age distribution would be expected. At the same time, dentists are self-employed and control their hours of work; thus, they should be able to remain in the labor force until a comparatively late age. Consequently, the age distribution of this profession should reflect the operation of two distinct considerations.

Finally, in addition to the five patterns we have proposed, other irregular age distributions can be expected to characterize some occupations. These would reflect shaping forces such as abrupt changes in the rate of employment growth in prior decades (population waves), the idiosyncratic effects of industry (which we discuss in the final section), and interactions among the factors we have enumerated when several pertain to an occupation. Issues of these sorts, however, are second-order considerations in that they require a deep appreciation of the historical circumstance and organizational context surrounding an individual occupation. In the present paper, we avoid particularistic patterns, and concentrate instead upon unimodal and other simple age distributions which have broad relevance for our comprehension of labor market structure.

EVIDENCE FOR A DIVERSITY OF OCCUPATIONAL-AGE DISTRIBUTIONS

To pursue these issues we used the 1970 Census 1/100 sample tape (5% county group file), taking as observations all males over 17 years of age who were employed full-time in 1969⁵ and were not disabled. Because we wished to examine age profiles for detailed occupations (three-digit census codes), a very large sample was required. We would have preferred to limit the investigation to a single labor market area, in recognition of the fact that age patterns differ somewhat among communities, but this would not

⁵ Working full-time was defined as employed 40 or more weeks in 1969 and having earned \$2,000 in that year.

have provided a sample of sufficient size. Consequently, data from 12 large northern SMSAs were pooled.⁶ To a degree, this regional and city-size restriction serves to reduce the magnitude of city differences in labor market organization.

Occupational-age patterns were constructed in the following manner.⁷ The age distribution of each position was standardized against the age distribution of the total sample. That is, if P_{ao} = proportion of individuals in age category a of occupation o , and P_{a+} = proportion of the total sample in age category a , then our index of representation in age category a is given by $E_{ao} = P_{ao}/P_{a+}$. Thus, to the extent that E_{ao} exceeds 1.00, age category a is overrepresented in occupation o ; to the extent E_{ao} is below 1.00, the age category is underrepresented in the occupation. The age profile for occupation o is given by the row of entries, $\{E_{ao}|a = 18-21, 22-27, \dots, \geq 65\}$.

We excluded from the analysis all occupations with fewer than 75 individuals in our sample (to obtain stable age patterns) and all those which represent census codes for allocated categories. This left 201 occupations, 80% of which ($N = 160$) could be classified, by inspection, into one of the five age patterns. That is to say, a *subjective* decision was made concerning the category to which an occupation would be assigned. An objective, confirmatory analysis was also performed and we compare the results from the two procedures later in this section.⁸

The first set of age profiles, reported in table 1, illustrates occupations in which young, middle-aged, and older workers are concentrated. The specific occupations listed were chosen to depict the variety of positions associated with an age category. Turning to occupations in which the young are overrepresented ($N = 43$), we find, consistent with expectation, a cluster which can be identified with heavy physical labor (electrical lineman, stock handler, warehouseman).⁹ A second cluster contains positions that are entry-level occupations into career lines (assembler, bank teller, drafts-

⁶ The 12 SMSAs are: Boston, Chicago, Detroit, Kansas City, Los Angeles, Minneapolis-St. Paul, New York, Philadelphia, Pittsburgh, San Francisco-San Jose, St. Louis, Washington-Baltimore.

⁷ Similar approaches to age standardization have been taken by Lehman (1953), Murrell (1962), and Smith (1968, 1969, 1973).

⁸ The subjective assignment must be carried out prior to completion of the objective procedure, since it serves as a guide in assigning age category boundaries once the occupations have been ordered using the objective scheme. We discuss our findings in reference to the subjective assignment because it is more revealing about substantive considerations which produce age patterns at the borderline between two categories. As we report later, the thrust of our results is the same irrespective of the classificatory procedure used.

⁹ To conserve space, not all occupations mentioned in the text are reported in tables 1 and 2.

TABLE 1

ILLUSTRATIVE AGE DISTRIBUTIONS: YOUNG, MIDDLE-AGE, AND OLD-AGE PROFILES^a

OCCUPATION	N*	MEAN		AGE GROUPS ^b									
		AGE		18-21	22-27	28-33	34-39	40-45	46-51	52-57	58-64	≥ 65	
A. Young-age occupations: ^d													
Electrical lineman (433).....	183	36.7		1.43	1.22	1.65	1.37	1.00	.45	.59	.63	.15	
Stock handler (762).....	638	34.7		6.87	1.60	.80	.71	.49	.74	.59	.59	.59	
Assembler (602).....	1,243	37.8		2.02	1.69	1.10	.87	.78	.78	.84	.80	.32	
Policemen (964).....	1,356	37.1		.55	1.41	1.57	1.21	1.12	.77	.56	.34	.21	
Gas station attendant (623).....	347	36.0		6.79	1.45	.69	.59	.79	.55	.84	.64	1.25	
Computer programmer (3).....	498	30.5		.92	2.79	2.28	.93	.46	.22	.10	.06	.05	
Computer repairman (475).....	127	31.9		1.03	2.38	2.00	1.35	.51	.27	.19	.16	.00	
B. Middle-age occupations: ^e													
Engineer; chemical (10).....	170	41.2		.00	.68	1.29	1.22	1.31	1.29	.83	.43	.51	
Engineer; mechanical (14).....	719	42.2		.00	.65	1.12	1.14	1.32	1.29	.97	.67	.48	
Bulldozer operator (412).....	88	40.3		.37	.82	.93	1.62	1.27	1.17	.66	.72	.00	
School administrator (240).....	283	44.9		.00	.17	.70	1.34	1.71	1.31	1.03	1.04	.71	
Sales manager, except retail (233).....	881	43.6		.07	.41	.85	1.32	1.37	1.32	1.07	.81	.69	
Pilot (163).....	170	37.3		.00	.63	2.34	1.47	.85	1.13	.54	.06	.00	
C. Old-age occupations: ^f													
Barber (935).....	307	44.4		.42	1.08	.74	1.02	.75	.98	1.14	1.20	2.26	
Tailor (551).....	203	49.7		.48	.28	.71	.63	.81	.88	1.31	2.34	3.56	
Sewer and stitchee (663).....	168	46.9		.97	.38	.86	.85	.82	.69	1.34	2.07	2.58	
Real estate agent (270).....	473	46.9		.27	.47	.66	1.03	.98	1.06	1.34	1.43	2.44	
Guard, watchman (962).....	723	47.5		.90	.68	.58	.63	.79	1.01	1.34	1.75	3.20	
Locomotive engineer (455).....	115	50.0		.00	.12	.12	.37	1.25	1.85	2.10	1.83	.75	
Railroad car shop repairman (486).....	156	45.3		.00	.69	.75	.45	1.09	1.54	1.49	1.55	.37	

^a Occupations were selected to illustrate the variety of positions in an age category. Following each occupational title the three-digit 1970 census code is shown (in parentheses). Data are for males.

^b Each entry reports the proportion of the occupation's members in the age category, relative to this figure for the total labor force. Entry > 1 indicates overrepresentation of the age group in the occupation; entry < 1 indicates underrepresentation. In particular, an entry equal to 2 means twice the representation that is expected; an entry equal to .5 means half the expected representation.

^c Number of individuals in the occupation in our sample.

^d Number of occupations in the young-age category equals 43.

^e Number of occupations in the middle-age category equals 30.

^f Number of occupations in the old-age category equals 38.

man), in that foremen, managers, and supervisors are commonly recruited from them.

The remaining occupations of young individuals are of diverse sorts. Police work entails difficult physical labor, but generous early retirement programs also contribute to a high departure rate in middle age. Notice, incidentally, that the very young are underrepresented in police work; often there are legal minimum age requirements in protection services. Attendant work in a gas station has many of the features we attributed to "secondary labor market" positions; pay is low, turnover is high, the job is easy to enter but leads nowhere though some workers do acquire mechanic skills. There is even a tendency to a U-shaped distribution ($E_{65+} = 1.25$), although the effect is modest in comparison with the overrepresentation of youth (e.g., $E_{18-21} = 6.79$). For this reason we placed the occupation in the young-age category. The final occupations in panel *A* (computer repairman, computer programmer) illustrate industry effects, a topic which we pursue in a subsequent section. Computer services utilize a new technology, and many affiliated occupations have young age distributions.

Middle-aged occupations are more varied in kind (panel *B*). Of the 30 positions in this category, several are engineering occupations¹⁰ and a few are craft jobs, in particular, heavy machinery operators.¹¹ Yet the dominant cluster of occupations involves supervisory and middle-level tasks (school administrator, sales manager, vocational counselor). The middle-aged character of these occupations is probably due to their location in job sequences: promotion is an important avenue of entrance. This is especially apparent for school administrators, invariably former teachers; the representation rate of individuals under 30 is extremely low. Airline pilot is another middle-aged occupation. Underrepresentation of the young results from lengthy training requirements; underrepresentation after age 50 stems from health and retirement regulations. To summarize, the majority of middle-aged occupations can be understood in terms of placement in a job sequence, or in terms of entry credentials and retirement rules.

Occupations in which the elderly are concentrated (panel *C*, $N = 38$) conform closely to our expectations. One cluster with an overrepresentation of older workers consists of positions with high rates of self-employment (barber, tailor, taxi driver). Indeed, our data exhibit a monotonic increase with age in proportion self-employed. In the age range 28–33, total sample,

¹⁰ All the engineering occupations were classified as middle-aged. This age pattern of engineers probably stems from truncation at the lower end due to educational requirements and, in later life, opportunities for movement into managerial positions.

¹¹ Bulldozer operators and cranimen are entrusted with expensive machinery and often have life-and-death control over other workers. Mature individuals are therefore sought for these jobs and the very young are underrepresented (interview with Mr. Reese Hammond, Research and Education Director, International Union of Operating Engineers).

the rate is 6.3%, in the interval 46-51 it is 12.1%, and among the older-than-65 population, proportion self-employed equals 24.7%.¹²

Self-employment provides an individual with the opportunity to schedule his hours of work in accordance with personal needs. For the same reason, the free professions (doctor, lawyer, dentist) should contain an overrepresentation of the elderly, and they indeed do. Yet these occupations also have age features which resemble a uniform distribution and, for consistency with the literature on professions, they were assigned to the latter age profile category. As we have noted, multiple processes often influence the age distribution of an occupation and, at the boundary, assignment to a unique category is somewhat arbitrary.

Some situations of employment permit flexibilities in controlling the rate and hours of work similar to those of the self-employed. Consistent with our earlier discussion, real estate salesman, cleaner, and guard and watchman are found in this age category. Finally, there again is evidence of industry effects. Our data suggest that railroads and apparel manufacturing are industries of the elderly, presumably because of an employment decline in recent decades.¹³ Notice, however, the impact of retirement rules (which are common in large enterprises) on the age structure of railroad occupations. After age 65 the representation rates fall below even the norm for the total labor force. For this reason, one might alternatively have assigned these occupations to the middle-age category.

In addition to concentrated, unimodal occupational-age distributions, we have postulated age patterns of two additional sorts. Extrapolating from the organizational situations of the free professions and craft occupations, we suggested that there should be a class of occupations which have a flat age profile. The reasons why these occupations would employ roughly the same percentage of each age group in the labor force are several. First, professional associations and craft unions control the supply of personnel in order to maintain high and steady demand (Hall 1975, pp. 70-71, 188-89; Ritzer 1972, pp. 60-61, 202-3). Craft unions do this through dominating apprenticeship programs; professions, by setting licensing standards and influencing the number of slots in professional schools. Since demand for these occupations is responsive to total population size, it is reasonable that shifts in supply would reflect changes in cohort size. Second, membership in these occupations tends to be a "life-time commitment" (Hall

¹² In every age group, proportion self-employed is higher in the old-age occupation profile category. In comparison with the text figures for the total sample, proportion self-employed in the old-age occupation category is 12.0%, 17.4%, and 31.9% for the three age groups, respectively.

¹³ Between 1950 and 1970 employment in apparel and fabricated textile manufacturing declined by 12.1%; in railroad transportation the decline was 54.8%. During this same period total employment in all industries *increased* by 15.5%. Data are from the U.S. Bureau of the Census (1954, 1972).

1975, p. 189), obviating any need to overemploy young workers in order to compensate for later outmovement. For both these reasons uniform age patterns would be expected.

Representative occupations from the collection judged to have a flat age profile ($N = 29$) are presented in table 2, panel *A*. As anticipated, many crafts (e.g., carpenter, electrician, plumber, compositor and typesetter) have age distributions which parallel that of the total labor force (i.e., the entries all approximate 1.00 in value). For the free professions (dentist, doctor, lawyer) the age pattern is somewhat different, though over the range of middle years (28–64), it is quite flat. There is considerable underrepresentation of the youngest age groups, a consequence surely of the lengthy education period necessary for entrance. There is overrepresentation in the late ages, due probably to the self-employed character and physical ease of the pursuits. As we have remarked, these professions might equally be viewed as having old age distributions.

A final type of age pattern was deduced from the features of "secondary labor market" positions, which are, descriptively, "dead end" jobs. By the latter term we mean occupations with low status and earnings, which require minimal skill and training, and, especially, are not part of a coherent job sequence. Such occupations tend to absorb the more marginal workers in the labor force—the young seeking temporary work and the elderly who are compelled to change jobs because of task difficulty or retirement rules. For these reasons the age distributions of many "dead end" occupations should be U-shaped.

In panel *B* of table 2 we report age profiles for a representative selection of occupations which have a U-shaped pattern ($N = 20$). With the exception of bookkeeper, the presence of each occupation on the list is comprehensible in terms of the preceding discussion. The most striking examples of U-shaped distributions are the age profiles of food service worker, and office and messenger boy. In the latter instance, the youngest age group is overrepresented in the occupation by a factor of 4.7, the oldest age group by a factor of 8.0, while individuals of prime working age are substantially underrepresented (0.4 for the interval 34–39). Not only are the U-shapes pronounced, but the positions constitute classic examples of dead-end occupations: remuneration is poor and prospects for promotion or upgrading are dim.

To conclude the description of occupational-age patterns, we summarize the data in a way which emphasizes the magnitude of differences between the age categories. For this purpose, Indices of Dissimilarity¹⁴ (Taeuber and Taeuber 1965, p. 236) were computed for each pair of the 201 occu-

¹⁴ For two probability distributions $\{P_{1i}\}$ and $\{P_{2i}\}$, the Index of Dissimilarity is defined as $50 \sum |P_{1i} - P_{2i}|$. The extent of difference between the distributions ranges from 0 to 100.

TABLE 2
ILLUSTRATIVE AGE DISTRIBUTIONS: UNIFORM AND U-SHAPED PROFILES

OCCUPATION	N ^a	MEAN											AGE GROUP				
		Age	18-21	22-27	28-33	34-39	40-45	46-51	52-57	58-64	≥ 65						
A. Uniformly distributed occupations: ^b																	
Carpenter (415).....	1,566	42.6	.79	.80	.98	.97	1.16	1.17	1.10	.98	.81						
Electrician (430).....	1,085	41.8	.63	1.03	1.06	.93	.93	1.11	1.03	1.04	.77						
Plumber and pipefitter (522).....	921	43.2	.85	.83	.83	.98	1.02	1.07	1.13	1.21	1.10						
Compositor and typesetter (422).....	455	43.1	1.22	.76	.94	1.04	1.03	.85	1.01	1.28	1.59						
Dentist (62).....	326	47.6	.00	.15	1.03	.98	1.04	1.09	1.12	1.23	3.46						
Doctor (65).....	860	44.7	.07	.34	1.14	1.39	1.04	.85	1.01	1.27	1.85						
Lawyer (31).....	1,065	45.4	.06	.46	1.20	1.22	.96	.80	.83	1.35	2.82						
B. U-shaped profile occupations: ^c																	
Bookkeeper (305).....	848	41.9	1.46	1.37	.82	.73	.85	.88	.86	1.20	1.91						
Food service worker (916).....	125	42.7	3.66	.86	.55	.80	.68	.99	1.20	1.18	2.08						
Office and messenger boy (333).....	104	47.4	4.72	.76	.46	.41	.50	.33	.72	1.42	8.07						
Elevator operator (943).....	134	51.5	1.46	.16	.51	.32	.63	1.02	1.55	1.97	5.61						
Counter clerk except food (314).....	189	39.4	1.90	1.98	.80	.56	.76	.62	.84	1.06	1.83						
Gardener (755).....	504	44.8	1.23	.98	.64	.78	.70	1.05	1.12	1.46	2.58						
Mail handler except post office (332).....	209	39.3	4.23	1.69	.69	.58	.54	.49	.76	1.52	1.94						

^a Number of individuals in the occupation in our sample. See notes to table 1 for additional details.

^b Number of occupations with uniform age profiles equals 29.

^c Number of occupations with U-shaped age profiles equals 20.

pational-age distributions. The means of the indices between occupations in the same age category, and the means between occupations in different categories, are presented in table 3. Notice that the smallest distances are always within-category (main diagonal entries) and the largest are between very different sorts of age profiles (e.g., between young and old occupations), except in the case of the unclassified occupations, which are not very distant from any of the other categories as they constitute a heterogeneous, residual class.

This clear division of occupations into different age categories reflects, in many respects, segmentation of the labor market along the lines of institutional forces which operate to mold career lines in diverse ways. On the one hand, there are the crafts and professions, with their life-time commitments and control over occupational entry. At the opposite extreme, in regard to individual commitment and associational power, are positions in the secondary labor market. The age profiles observed for these two occupational groupings follow from the sorts of theoretical considerations that we have outlined. In comparison, concentrated, unimodal age distributions arise from a variety of factors; probably the most significant theoretically is the fact of linkages among occupations in career sequences. In short, unimodal distributions speak to patterns of movement among jobs.

A Confirmatory Analysis of Occupational-Age Patterns

Our subjective assignment of occupations to age categories may be checked by an objective assignment, according to the following scheme. The Indices of Dissimilarity between all pairs of the 201 occupations were analyzed by means of a Guttman-Lingoes Smallest Space Analysis (SSA) (Guttman 1968; Roskam and Lingoes 1970). The purpose of SSA is to determine the smallest euclidean space, in terms of number of dimensions, in which the

TABLE 3
MEANS OF INDICES OF DISSIMILARITY* BETWEEN OCCUPATIONS CLASSIFIED
IN SAME AND IN DIFFERENT AGE CATEGORIES

Age Category	N ^b	Young	Middle-Aged	Old	Uniform	U-Shaped	Unclassified
Young.....	43	19.1					
Middle-aged....	30	31.7	13.3				
Old.....	38	24.1	22.3	14.2			
Uniform.....	29	22.1	16.4	16.1	10.5		
U-shaped.....	20	27.5	28.8	21.7	19.5	18.9	
Unclassified ^c	41	23.1	18.2	17.9	13.4	20.6	15.2

* Index of Dissimilarity between occupations O_1 and O_2 equals $50 \sum_i |P_{O_1i} - P_{O_2i}|$. Index varies from 0 to 100; large values indicate great differences between occupational-age patterns.

^b Number of occupations in the age category.

^c Occupations with complex age profiles, which were not classified into one of the five categories.

rank order of the distances between points (occupations in the present application) is reasonably well preserved. The Index of Dissimilarity between two occupational-age distributions was used as the measure of distance.

Given size limitations for the SSA program it was not possible to perform the analysis on all 201 occupations simultaneously. Consequently, we used SSA to produce a spatial configuration for 99 of the 201 occupations (randomly chosen) and then added the remaining 102 occupations to this initial configuration. For the first 99 occupations the coefficient of alienation, which indicates how well the rank order of original distances is preserved, was .319, .113, and .088 for one, two, and three dimensions, respectively. Because the two-dimensional solution adequately fits the data and because of the ease of interpreting two, as opposed to three, dimensions, we chose to work with the former solution. The remaining 102 occupations were added one at a time to this two-dimensional space with an average coefficient of alienation¹⁵ equal to .108, indicating that the rank order of the distances between the additional occupations and the first 99 is well maintained.

In figure 1, the two-dimensional solution is reported with the 160 occupations that could be classified by inspection indicated on the output. Each occupation is labeled by its subjective category: young (1), middle-aged (2), old (3), uniform (4), and U-shaped (5); the *locations* of the points, though, were determined by the objective SSA routine. Using the subjective assignment as a guide in defining boundaries, we see that the space can be partitioned into five regions, each clearly though not perfectly enveloping occupations in one of the five subjectively determined age categories. Even using simple, linear partitions, such as the ones shown, only 15 of the 160 occupations classified by inspection appear in the wrong sector, and the majority of these are close to a boundary division.

Of equal relevance, the two dimensions of the space can be interpreted. The horizontal axis reflects the mean age of an occupation, and ranges (left to right) from young, to intermediate, to old. The vertical axis depicts the extent to which occupations have a concentration of workers in the middle years, and ranges (top to bottom) from overrepresented, to uniform, to underrepresented. Overall, the results of the SSA procedure support the contention that occupations have distinctive age structures, and indicate that our subjective assignment of them to categories approximates the partitions that would be drawn on the basis of an objective assessment of distances between occupational-age profiles.¹⁶

¹⁵ For each occupation added to the initial configuration, SSA computes a coefficient of alienation which indicates how well its placement preserves the rank order of the distances to the occupations in the initial configuration. These values were averaged to produce a summary measure of fit.

¹⁶ Why, then, was an objective procedure not used? In particular, the subjective assign-

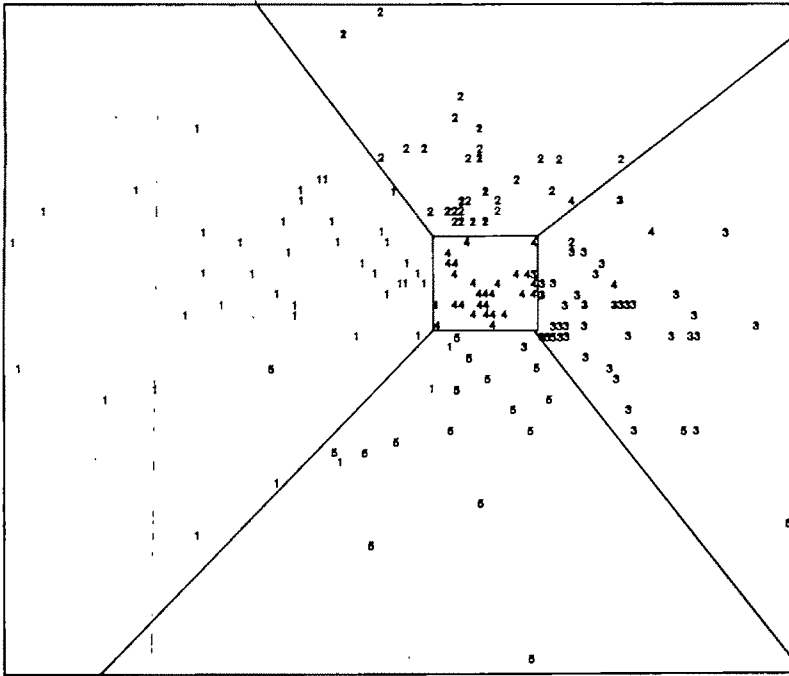


FIG. 1.—Smallest space analysis of the occupational-age profiles. Numbers refer to subjective classification: 1 = young, 2 = middle-aged, 3 = old, 4 = uniform, 5 = U-shaped. Locations of points were determined by the objective SSA procedure. Lines indicate linear partitions for defining occupational-age categories.

EVIDENCE FOR MECHANISMS WHICH GENERATE OCCUPATIONAL-AGE PATTERNS

Now that a factual basis has been established for the five occupational-age distributions that were postulated, we turn to the matter of validating some

ment could have been employed as a first stage, to define partitions on the SSA output; in turn, the partitions could have been used to classify occupations into age categories. Our objection to total reliance on the objective procedure stemmed from our desire to emphasize certain organizational forces where several act on an occupation. For example, while SSA would have placed dentist, doctor, and lawyer in the old-age category, we preferred to emphasize other aspects of their age structures, namely, the uniformity of the age profile over the major portion of the age range and the organizational considerations which give rise to this uniformity.

We could have defined "distance" differently, such as by assigning a weight to each portion of the age range, and thereby given precedence to one or another feature of the age profile as a determinant of the category to which an occupation would be assigned. However, because the Index of Dissimilarity is a familiar measure, and because the thrust of our analysis is well supported using this simple distance measure, we chose not to complicate the calculations and instead view the objective procedure as a confirmatory test of the main theme of our investigation.

of the mechanisms that were offered to explain their presence. In this section we report an elementary analysis which relates the occupational-age patterns to broader issues of labor market structure. In the next section we indicate some limitations of our formulation, particularly in relation to its neglect of industry effects.

To ascertain how different causal forces contribute to the observed occupational-age distributions, a multivariate logit analysis (Nerlov and Press 1973) was carried out. Multivariate logit regression is appropriate for the following formulation: Assume that an occupation can be in one of five classes (age categories); the classes constitute the set of dependent variables. The regressors, in turn, are measures of factors which, drawing upon our prior discussion, are believed to influence an occupation's presence in one or another of the classes.

The following variables were used as regressors:

a) Occupational growth—measured by percentage increase in occupational size, 1950–70. This variable was expected to differentiate between young-age occupations (high growth rate) and the two categories, old-age plus uniform-age distribution occupations (low rate of growth). Data are from U.S. Bureau of the Census (1968, 1973).

b) Specific vocational preparation (SVP). This measure is from the Dictionary of Occupational Titles (U.S. Department of Labor 1965) and indexes the amount of specialized training necessary to perform a job task. Our expectation is that low SVP should be associated with the U-shaped occupational-age category. We suggested that this category would contain many low-skilled "secondary labor market" positions, and low SVP should predict to these occupations. This variable was constructed from a merging of our census data tape with a file containing DOT information, to which census occupational codes had been assigned.

c) Percentage stayers in an occupation, 1965–70. This variable was also expected to distinguish the U-shaped age category from several others. In particular, if the U-shaped category contains "secondary labor market" positions—jobs characterized by little opportunity for skill development and a low return to seniority—attachments to the occupations should be low. At the opposite end of the attachment continuum would be occupations with a uniform age distribution. Many of these are professions and crafts, in which training is long and investment in occupationally specific skills is considerable.

For a different reason a low rate of attachment should characterize occupations in the young-age category, and a high rate occupations in the old-age category. In this instance, the difference in rate of staying would be derivative of an individual-level attribute, the tendency of young persons to be more mobile occupationally than mature workers.

d) Percentage self-employed. This variable was expected to distinguish

old-age occupations and uniform-age distribution occupations (both expected to have high rates of self-employment) from the other categories. Percentage self-employed was computed from the 1970 Census 1/100 sample tape.

e) Mean education. This term was introduced as a control, to hold constant an occupational characteristic that both is a determinant of occupational-age category and is correlated with several of the preceding variables that are of more central substantive interest. Mean education was calculated for each occupation from the 1970 Census 1/100 sample tape.

The results of the multivariate logit analysis are reported in table 4. In most respects our prior expectations are supported. In particular, occupations in the young-age category have low rates of staying and high growth rates (col. 1). Old-age occupations, in contrast, have high rates of staying, high rates of percentage self-employed, and low growth rates (col. 3). The latter figures are significantly different from those for the young-age category (though some are not statistically different from the population mean); more to the point, there is a consistent shift in the values of the parameters as one moves from young to middle-aged to old-age occupations.

Uniform-age distribution occupations have high rates of self-employment and low rates of growth (col. 4); both effects correspond with our expectations. Yet, surely the most interesting findings pertain to the U-shaped age category (col. 5). Percentage stayers is substantially negative, in comparison with the other occupational-age categories. Similarly, in conformity

TABLE 4
LOGIT REGRESSION ANALYSIS OF THE OCCUPATIONAL-AGE PATTERNS^a

Independent Variable	Young	Middle-Aged	Old	Uniform	U-Shaped
Occupational growth..	.00260* (2.51)	.00213** (1.95)	.00041 (.29)	-.00604* (-2.16)	.00090 (.50)
SVP.....	.05183 (.33)	.41176** (1.92)	.11364 (.74)	.00705 (.04)	-.58428* (-2.43)
% stayers ^b	-.55242* (-2.29)	.31691 (.98)	.79578* (2.33)	.34510 (1.00)	-.90536* (-2.86)
% self-employed ^b	-.16356 (-1.54)	-.11406 (-1.11)	.04200 (.47)	.16590** (1.80)	.06971 (.46)
Mean education.....	.06021 (.57)	.17391 (1.53)	-.29325* (-2.54)	.06532 (.54)	-.00620 (-.03)
Constant.....	3.92256** (1.89)	-7.34907* (-2.64)	-3.55207 (-1.22)	-2.80635 (-.97)	9.78493* (3.33)

^a Entries are unstandardized logit regression coefficients; *t*-ratios are in parentheses. For a given age category (column) the coefficients report the effects of the independent variables on the log of the odds of being in that age category.

^b The square roots of these variables were used in the analysis to reduce skewness.

* *P* < .05, two-tail test.

** *P* < .10, two-tail test.

with our argument, the SVP measure is negatively associated with presence in the U-shaped category. We conclude that occupations with this age distribution display features which many authors (e.g., Doeringer and Piore 1971; Reich, Gordon, and Edwards 1973) have associated with the "secondary labor market"—low requirements for job-specific skills and high rates of turnover.

Thus, despite the small number of occupations ($N = 160$) available for this multivariate analysis, our results are consistent with the operation of mechanisms of the sort postulated above as determinants of occupational-age patterns. In general, we find compelling evidence for the contention that the age categories derive from fundamental organizational features of the labor market. In particular, we find especially impressive the correspondence of a distinctive age structure with occupations that have little future or otherwise are of low desirability.

INDUSTRY EFFECTS ON OCCUPATIONAL-AGE PROFILES

To this point we have discussed occupations as undifferentiated entities with respect to the determination of their age profiles. There exist, however, systematic, often divergent, influences on the age structures of *segments* of occupations. Except for the free professions and vocations in which self-employment is common, occupations are not organized as autonomous units; rather, performance of their roles requires employment in an industry (more precisely, in a firm).¹⁷ Growth or decline in occupational size is one consequence of growth or decline in total employment in the industries in which the occupation is concentrated. Thus, occupations associated with several industries may be experiencing simultaneous expansion and contraction in different industry sectors. For example, during the period 1950–70, employment of managers and operatives increased in computer manufacturing while it decreased in apparel manufacturing (U.S. Bureau of the Census 1954, 1972).

The importance of industry growth and decline for occupational-age profiles is that contracting segments of occupations tend to have an old age structure, while expanding segments have a young age distribution. Contracting industries reduce personnel through layoff (which usually proceeds in reverse order of seniority) and by not replacing voluntary departers. In each case the effect is to create an older work force. Expanding industries, in comparison, hire disproportionately from among new entrants to the

¹⁷ Although firms are the employing units, lacking data at the firm level we use industry as a proxy. As remarked in n. 2 above, much of the interfirm variation in organizational features arises from industry differences. Firms in the same product category industry tend to use similar technologies, which limits the variety in organizational arrangements that can be established.

labor market. Salaries are lower for young workers and a longer expected duration of employment means greater potential return to the firm from training costs.¹⁸ Reinforcing this hiring preference, mature workers often are reluctant to change employers because investments have been made in seniority and pension rights. In summary, occupations associated with multiple industries may be expanding and contracting in different sectors. This makes for more complex age profiles than the results we have presented, in which occupations were viewed as undifferentiated units.¹⁹

For the purpose of understanding industry effects on occupational-age profiles, industries can usefully be considered as bundles of occupations in which the occupational mix is determined by technology.²⁰ To a considerable extent, expanding and contracting industries must maintain their technologically determined occupational mix, so changes in total employment in an industry carry analogous implications for the age structures of most associated occupations. Evidence in support of this contention is reported in Kaufman (1977).

By virtue of their growth histories, industries therefore contribute second-order effects, modulating the dominant age structure of an occupation. Nor is this the only manner by which industries influence occupational-age patterns. Industries also differ in the rules that are followed when filling upper manual and lower white-collar positions. In some, component firms traditionally promote from lower ranks (internal labor market);²¹ while in other

¹⁸ Bogue (1959, p. 501) reaches a similar conclusion: "... the younger generations enter new fields that are just opening up, and avoid fields that are declining in importance."

¹⁹ For example, the mean ages of managers in computer manufacturing and apparel manufacturing are 39 and 47, respectively. The corresponding mean ages of operatives in the two industries are 33 and 46. Systematic calculations reveal that in every major occupational category there is a correspondence between industry growth rate and age distribution. Thus, in industries which expanded by less than 70% between 1950 and 1970, the proportion of managers older than 52 years equals 38%, while in industries which expanded by more than 140% the proportion of managers older than 52 equals 26%. For sales workers the corresponding figures are 39% and 24%; for operatives they are 32% and 24%. The analysis presented above cannot be carried out with detailed occupations (three-digit census codes) because many of them (e.g., policeman, teacher, railroad engineer, typesetter) are industry specific. Data are from our tape on 12 SMSAs.

²⁰ The extent of technological determination of the occupational mix differs by industry. In some (e.g., auto assembly production), few alternate technologies (to the assembly line) are available, and there is a considerable degree of determination of firm organization and occupational mix by technology. In other industries (e.g., housing construction) alternative technologies are in use (site construction, prefabrication) and variability is present among firms in occupational composition. For enlightening discussions of the relation between technology and firm organization see Woodward (1965), Blauner (1964), Stinchcombe (1959), and Perrow (1967).

²¹ See Kerr (1954), Doeringer and Piore (1971), Stinchcombe (1959), and Zald (1971, pp. 61-65) for discussions of promotion rules, labor market organization, and how these considerations relate to industry.

industries, there is a tendency to hire directly into upper-level slots from outside the firm. Such considerations are relevant to occupational-age distributions, as they speak to the rate at which workers pass through an occupation and to the movement prospects of individuals with high seniority (presumably older workers).

The intent of our argument here is to suggest the utility of taking *occupation* \times *industry* as the unit of observation in analyses of age segmentation, rather than occupation alone. Such an approach has the additional virtue that occupation \times industry positions constitute the building blocks of moderately realistic descriptions of career trajectories (sequences of jobs), in that they permit consideration of the two main dimensions of movement—across occupations and across industries. Moreover, career trajectory categories (e.g., orderly career, chaotic career, craft career) carry strong implications for the age composition of the component positions in a trajectory (Spilerman 1977),²² so that the intimate association between age segmentation and movement patterns is made especially evident.

With all this said about the desirability of using occupation \times industry positions as the units of observation, the practical problems of analyzing age segmentation at this detailed level are immense. For this reason we limited the present investigation to a more aggregate characterization of labor force composition, in which the occupational dimension is emphasized.

IMPLICATIONS OF THE STUDY

One can hardly write about occupational-age structures without noting their special relevance to policy issues facing this nation. In particular, if compulsory retirement rules are eliminated or the age of compulsory retirement is raised, our analysis suggests a very different impact on an occupation according to the age category it is in. Occupations with an old-age profile or with a U-shaped profile would be little affected, as few currently have retirement requirements. To the extent that they are affected, these occupations stand to *lose* elderly workers since many are entered late in life by individuals who have departed from a prior career line, often as a consequence of retirement rules. Raising the age of compulsory retire-

²² An "orderly" career trajectory may be thought of as a collection of jobs in which movement among the component positions is principally in one direction. This usually entails performance of low occupational tasks at the outset, followed by higher-level positions within the firm (Slocum 1966, p. 5). The implications for age segmentation are that the successive jobs would be characterized by correspondingly higher mean ages of incumbents, and that the standard deviation of worker ages in each position would be small. In comparison, a "chaotic" career trajectory consists of a collection of jobs in which there is a high density of movement but without a coherent direction; in short, cycling among the positions is common. This implies little difference among the component jobs in mean age of workers and a large standard deviation of worker ages for each position.

ment would therefore reduce the inflow of older workers into these two age-profile categories.

Occupations with a uniform age distribution would also be affected only marginally by a change in retirement policy. This category contains the free professions, in which self-employment and absence of retirement rules is the norm. Also present here are the craft trades. Many (e.g., carpenter, electrician) provide opportunities for part-time or occasional work after formal retirement, though this is not common in other crafts (e.g., heavy machinery operator). Raising the age of retirement would permit individuals in the latter trades to remain in the labor force. Thus, the average age of workers should increase, though the aggregate effect on employment in this age-profile category would be small.

A change in retirement policy would have greater impact on the young and middle-aged categories. Occupations with a young age distribution that are part of a career track would experience an *increase* in the mean age of incumbents. That is, if the proportion of senior-level slots in the economy remains constant, raising or eliminating the age ceiling should produce a later average age of retirement from them and, as a result, a later average age of promotion from positions in the young-aged category.

Yet the impact of a change in the age of compulsory retirement would be most consequential for occupations in the middle-aged category. We refer especially to senior-level positions, in hierarchical organizations, which are entered late in an individual's work life. Managers, administrators, and other supervisory personnel constitute the most evident examples. Our data show the highest decline in representation rate after age 60 for occupations in this age-profile category. Because many of these positions have a concentrated age distribution (e.g., in the interval 50–65), removal of the upper-age constraint would have a considerable impact on their age composition.²³ The fact that these occupations tend to be ones of power and influence in our society should create special cause for concern about the consequences of altering current retirement policy.

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²³ Ignored in this brief discussion are demographic considerations. For example, the short-term consequence of raising the retirement age is a function of the current age distribution in the occupation. Similarly, if an occupation is expanding in size, raising the retirement age will have a smaller impact on its age composition than if it is contracting.

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Lenski Revisited: The Linkage Role of Religion in Primary and Secondary Groups¹

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This study replicates and extends the hypotheses proposed by Gerhard Lenski in *The Religious Factor* regarding differential Protestant-Catholic primary and secondary involvements. The data consist of three National Opinion Research Center General Social Surveys involving national samples. The study demonstrates that a "religious factor" remains, but it now operates in a similar manner for both Protestants and Catholics.

Without doubt, one of the more important and influential analyses of the relationship between religious values and behavior has been Gerhard Lenski's *The Religious Factor* (1963). Although this pioneering work was exploratory and to some extent polemic² in its rejection of the secularization and massification theses, it can be considered a modern landmark in the sociology of religion (Schuman 1971).

This paper replicates and extends a number of the more neglected findings presented by Lenski; namely, those dealing with the relation between religious affiliation and its effect on participation in primary and secondary

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² Lenski's interest in the applicability of Weber's Protestant ethic in contemporary America was in part motivated by a number of studies suggesting that Protestant-Catholic differences in occupational achievement (Mack, Murphy, and Yellin 1956) and occupational mobility (Lipset and Bendix 1959) no longer existed. In *The Religious Factor* (1963, pp. 84-85), Lenski criticizes these findings on the basis of the results of his own data and those of Weller (1960). See Winter (1977, pp. 52-53) for a discussion and summary of post-Lenski research conclusions on this matter. Unfortunately, it is generally forgotten that Weber never assumed that all Protestants would adhere to an ethic of ascetic Protestantism. Again, we are faced with the problem that Protestantism in America and elsewhere is highly heterogeneous. Some denominations encourage worldly asceticism more than others (see Johnson 1961).

groups. This aspect of *The Religious Factor*, although partially supported by the work of Winch (1977) and Abramson (1973), has not been fully developed previously.

Our data are based on a late-1970 series of national samples of the American population. We are therefore able not only to reexamine certain of Lenski's findings by utilizing more recent and more generalizable samples, but also to introduce variables such as community size not available in his Detroit Area 1958 samples. Moreover, because of the larger size of our composite sample, we are able to introduce a greater number of variables (e.g., gender, ethnicity, social class) in a multivariate analysis of primary and secondary involvements.

The significance of *The Religious Factor* stems in part from the fact that so little research had, until its publication, attempted to continue Weber's analysis of the interaction between religion and the secular order (Demerath and Hammond 1969). Lenski's topical comprehensiveness, as well as his theoretical sensitivity, allowed him to relate religious commitment to a wider range of issues than had been the custom before—and since—the publication of his work.

Unfortunately, the research that has followed Lenski's has generally been limited to that part of *The Religious Factor* dealing with differences in Protestant-Catholic socioeconomic achievements (Schuman 1971; Greeley 1964; Glenn and Hyland 1967). The issue of a Protestant-Catholic difference in socioeconomic position has largely been settled in favor of a gradual Protestant-Catholic convergence (Glenn and Hyland 1967; Alston 1969; see Roof [1979] for a dissenting view). Convergence can also be found in attitudes on such issues as abortion, political position, and church attendance (Davis 1976; McCready and Greeley 1972; McIntosh and Alston 1977). This apparent tendency toward convergence does not extend to all issues, in that Protestants and Catholics continue to differ with regard to parochial schools, availability of birth control measures, and censorship (see Greeley 1977; Greeley, McCready, and McCourt 1976; Hynson 1976; Argyle and Beit-Hallahmi 1975; Johnstone 1975). Furthermore, the issues selected by researchers may have diverted attention away from the more fundamental interinstitutional relationships among religion, family, and the economy that concerned Weber (see Lenski 1963, p. 8).

Moreover, Lenski ignored the possible effects of denomination and ethnicity on behavior and attitudes. He had assumed, like Herberg (1960), that urbanism and religious intermarriage would increasingly create a tripartite melting pot of Protestants, Catholics, and Jews in which intrareligious and interethnic differences would gradually disappear (1963, p. 21). More recent research on the effect of denominational affiliation (McIntosh, Alston, and Alston 1979; Lazerwitz 1971) and on Catholic ethnicity (Greeley

1977), as well as the increased popular interest in ethnicity per se, suggests that Lenski's assumptions about the eventual disappearance of such differences were premature.

THE LENSKI HYPOTHESES

While *The Religious Factor* dealt with a variety of religious effects, most subsequent research focused on the consequences of religious affiliation for socioeconomic values and achievement. We have thus chosen to retest a number of the effects ignored by other researchers, namely, involvement in voluntary associations and primary groups. Specifically, we concentrate on Lenski's assertion that religious affiliation results in more than definitional categories; that is, religious groups also form self-conscious subcommunities (see Roof 1976). A similar position has been taken by Gannon (1978), who states that religious ties can link people into "multiple communities." That is, contemporary urban communities contain diverse interests and sentiments which depend on religion as an underlying source of unity and mobilization.

Religion as subcommunity has been ignored by many since Lenski's work. The reason for this has been the assumption that massification and secularization would force (or would be associated with) a withdrawal of the religious influence from society at large (Nisbet 1962; Stein 1969) and the increasing "privatization" of religion at the individual level (Luckmann 1967).

Lenski (1963) presents the following general theoretical statements in *The Religious Factor*: (1) that the normative structure of religious organizations affects involvement in secular organizations (p. 8); (2) that the normative structures of Protestantism and Catholicism differ (p. 8); and, therefore, (3) that involvement in Catholicism leads to differing patterns of involvement in social organizations than involvement in Protestantism does (pp. 21, 249-50). From the above statements he then proposes more specific hypotheses: (3a) The more highly involved an individual is in the Protestant church, the more highly involved he or she is in secondary groups (voluntary organizations) (pp. 244-45), and the less involved in primary groups (kin and neighbors) (pp. 243-44); and (3b) The more highly involved an individual is in the Catholic church, the less highly involved he or she is in secondary groups (voluntary organizations) (p. 250), and the more involved in primary groups (kin and neighbors) (pp. 247-48).

Lenski defends these hypotheses with several arguments. First, the sectarian movement during the establishment of Protestantism was accompanied by attempts to isolate the individual from all secular attachments,

particularly those of the extended family. Obviously, contemporary Protestantism as a whole is no longer sectarian, but a residue of this other-worldly emphasis still exists in its "attempts to create through church-related organizations a substitute for secular social relationships, especially those of the extended family" (Lenski 1963, pp. 246-47). Hargrove (1979, p. 162) notes that, while major Protestant denominations no longer fit the sect criteria, the congregational nature of modern Protestantism has "provided a substitute for the larger kin groups which have been difficult to maintain in a highly mobile society."

At the same time, Protestants' involvement in the church serves as a "training ground" for, and a stimulus to participate in, voluntary organizations outside the church (Lenski 1963, p. 247). Thus, religious commitment encourages members to weaken their ties to extended family relations and at the same time compensates for this loss of familial ties by encouraging activities outside the family. The ideal-typical Protestant thus becomes less involved in the family and more involved in other groups.

Catholics, on the other hand, are encouraged to develop a different set of relationships which center on a church-family nexus. In contrast to the Protestant church, which seems to compete with the extended family, the Catholic church and the institution of the family form a more complementary union (Lenski 1963, pp. 247-48). Consequently, marriage is considered a sacrament and home and family receive great moral support (Salisbury 1962; Hargrove 1979, p. 161).

In support of this Protestant-Catholic contrast, Lenski finds that Catholics who are more highly involved in their church's activities tend to be more involved with their families as well as less involved in outside organizations. He finds the opposite pattern for the Protestant members of his sample.

The question, of course, is whether or not these Protestant-Catholic differences in associations can be interpreted as being caused, in part or wholly, by religious factors (i.e., affiliation) rather than by other, more secular, variables (e.g., social class). The literature suggests at least five major alternative approaches to explaining primary and secondary involvements. After we discuss each of these five alternative sets of hypotheses, we will not only retest Lenski's contention that religious affiliation can be considered a valid causal explanation of secular involvements but also test the five competing sets of hypotheses.

In this manner we may find support or lack of support for Lenski; we may also find, however, that the religious factor holds only for certain categories of residence, denominational affiliation, ethnic background, or social class.

THE ALTERNATIVE HYPOTHESES

Socioeconomic Background

Research has shown that voluntary organizational participation increases with status (see Curtis and Jackson 1977, p. 196; Tomeh 1973, p. 137) while kin involvements either decline or remain stable (Curtis and Jackson 1977; Bott 1968; Adams 1967; Sussman 1953; Hodge and Treiman 1968; Klatsky 1972; Hausknecht 1962).

Other scholars have held that people of middle-to-high socioeconomic position tend to be "joiners," regardless of whether religious or secular secondary organizations are being considered. Such findings would account for the religious and secular involvements of upper-status Catholics and Protestants (Goode 1966; Lazerwitz 1962; Berger 1961), thus rendering the religious variable irrelevant. While some have argued that such memberships reduce involvements with kin, friends, and neighbors, others have suggested that attachments tend to be general, and those who are inclined to do so participate equally across the entire spectrum of primary and secondary associations. In many cases, these associations overlap: kin, friends, and neighbors may also be fellow churchgoers or members of the same bowling league or neighborhood improvement group.

The research in this area, however, has led to mixed results. While a consistent relationship between class and voluntary organizational participation has been found (see Tomeh 1973; Axelrod 1956; Greer 1956; Bell and Force 1956; Dotson 1951; Komarovsky 1946), there is increasing evidence that social class explains little of the variation found in religious participation (Mueller and Johnson 1975; Davidson 1977; Alston and McIntosh 1979). This suggests that religious participation (i.e., church attendance) cannot be explained away by a general propensity to join secondary organizations. Other factors, such as religious salience (Alston and McIntosh 1979) and localism (Roof 1978), have been found to affect the individual's nature and level of involvement. Religious salience is defined as the perceived importance of religion in the individual's everyday life. Localism is more of a secular measure defined as an attachment to and orientation toward the life of the immediate community as opposed to the larger social world (Roof 1978, p. 42).

Still others have found that members of the lower class tend to associate with family, friends, and neighbors but to avoid voluntary organizational attachments (Rainwater, Coleman, and Handel 1959). For members of the lower classes, involvement in primary and secondary associations consists of two sets of independent dimensions; this is not true for members of the middle and upper classes (Tomeh 1973).

We suggest that social class background explains the propensities to associate with friends and to join organizations, but, given that we cannot

readily predict religious participation in the same way, it is unlikely that Lenski's hypotheses are merely artifacts of underlying class-participation relationships.

Urbanization-Massification

Theories of urbanization have suggested that urban life liberates the individual from the restrictions of an earlier, more traditional, rural, and communal existence (Lerner 1958; Armer and Isaac 1978). Other theorists, describing a similar process, point not to the increased individual freedom afforded, but to the "loss of community" and the consequent individual maladjustment (Tönnies [1887] 1957; Wirth 1938; Nisbet 1953). Both views see modern man as less involved in long-lasting, emotionally supportive social networks.

Descriptions by Wirth (1938), Bibby (1979), and others suggest that urban life promotes secularization and secondary relationships, thereby reducing primary relationships and religious commitment. These changes are attributed to the increasing size and heterogeneity of the urban population and to occupational and geographical mobility. By implication, rural life, because of its smaller population, homogeneity, and lower rates of mobility, tends to foster primary over secondary ties.

Such a perspective suggests that population size and other community attributes could serve as the basis of additional if not alternative hypotheses to Lenski's. The research on urban life over the past 30 years, however, shows no such decline in primary group involvements (see Axelrod 1956; Greer 1956; Bell and Boat 1957; Litwak 1960*a*, 1960*b*; Sussman 1965). Mobile urban residents either move to areas that contain their relatives or compensate for geographical separation by mail and phone (Hareven 1978; Adams 1968; Litwak 1960*b*; MacDonald and MacDonald 1964). Instead of the isolated, anomic life we would have expected, friendships are maintained (Reiss 1959; Fischer 1977; Suttles 1968; Gans 1962*a*, 1962*b*), and mutual aid is common (Wellman 1979).

Other research disputes whether urbanites are much more likely than ruralists to join voluntary organizations (Lazerwitz 1962; Fischer 1976; Babchuck and Booth 1969). And while Hougland, Kim, and Christenson (1979) find that secondary involvements increase with population size and density, they also find that socioeconomic factors are far better predictors of such activity than are size and density alone.

These findings raise the question of the general utility of looking for explanations for organizational involvements in strictly "residential" theories to the neglect of other variables. While it is possible that urban living has some effects on involvements, these are likely to stem from the effects of the heterogeneity of the urban scene. As Glenn and Hill (1977), Fischer (1976,

1977), and Gannon (1978) have noted, size and density may be of less importance than was once thought. The subcultural heterogeneity fostered by urban life may produce behaviors that differ from those taking place within a rural environment. Religion and ethnic background might interact with residence to produce differences in the style and intensity of organizational involvements (see Greeley [1974] and Abramson [1973] for supporting evidence).

Family

Like the nature of residence in modern society, the family is said to have undergone a far-reaching transformation. As a result of the industrial revolution and concomitant urbanization, the traditional extended family became nuclear, shorn of many kin ties (Parsons 1955; Goode 1963). In addition, it underwent differentiation in such a way that its functions were passed on to other institutions, leaving behind a more specialized and isolated family group (Smelser 1959; Burgess, Locke, and Thomas 1963; Wirth 1938).

In spite of this, family researchers have uncovered evidence which makes the "isolation" hypothesis increasingly untenable. Although few would deny Parsons's (1943) assertion that the family is no longer the center of societal economic activity, it is clear that, despite increasing differentiation-specialization, social and economic interdependencies between the nuclear unit and extended kin remain (Sussman and Burchinal 1962; Cumming and Schneider 1961; Sussman 1965; Adams 1968; Curtis and Jackson 1977; Parsons 1977). This lack of isolation is not merely a manifestation of class but appears to be more universal (Sussman 1953, 1965), with extended kinship sociability occurring among both lower- and middle-class kin groups. Such research indicates that extended family ties continue to be maintained in contemporary America. As there is nothing in this literature to indicate that Protestant or Catholic families would be any more or less successful at weathering the onslaught of industrialization, we are led to anticipate little variation in kin involvement by religious affiliation.

Ethnic and Denominational Differences

While we might not expect American-born Catholics and Protestants to differ much overall with regard to kin involvements, it is possible that less assimilated ethnic groups might show variation in kin ties. In particular, various religioethnic groups may vary considerably, in part dependent upon each group's degree of assimilation into the contemporary, more industrialized sector of American life.

Religious involvement and voluntary organizational membership and activity have been found to vary by ethnicity (see Greeley 1974, 1977; Abramson 1973; Lazerwitz 1973). Italian Catholics, for instance, attend church less often and belong to fewer organizations than do Irish Catholics, although both groups enjoy similar occupational and socioeconomic status vis-à-vis American society. Similar differences have been suggested among Protestant denominations (Greeley 1974; Anderson 1970; Stark and Glock 1968). Furthermore, Greeley (1972, pp. 184–85) argues that denominational distinctions continue to widen rather than the opposite, which the “melting pot thesis” would predict. He also suggests that American religious groups have come to serve as quasi-ethnic groups.

More to the point, the involvement with kin and voluntary organizations varies with the historical experience of the ethnic or denominational group, including the recency of the immigration (see Habenstein and Mindel 1976, pp. 415–18; Lopreato 1970; Greeley 1977).

Such findings have led critics of Lenski to state that neither Catholicism nor Protestantism can be considered homogeneous in modern America, despite Herberg's (1960) thesis to the contrary (see Rosen 1962; Schuman 1971). In fairness to Lenski, we should note that he conducted his analysis at a time when ethnicity was considered increasingly unimportant, and thus there was little reason for him to pursue the ethnic variable.

Other Sociodemographic Variables

Sex, age, and geographical mobility variables have been linked to kin, religious, and voluntary organizational involvements. Women and older people are more likely to interact with kin (Rainwater et al. 1959; Komarovskiy 1964), although Duncan and Duncan (1978, pp. 187–88) report that such differences markedly declined during the period from 1959 to 1971.

Other studies point to greater female involvement in church (Argyle and Beit-Hallahmi 1975) and voluntary activities (Glock, Ringer, and Babbie 1967; Tomeh 1973; Duncan and Duncan 1978); and age exhibits a linear, a curvilinear, or no effect on such participation, depending on the type of cohort or sample used (Wingrove and Alston 1974; Alston and McIntosh 1979; Hausknecht 1962; Bell and Force 1956). Finally, geographical distance has proved a major factor in limiting kin interactions in the United States (Adams 1968; Klatzky 1972).

The above description of the five general alternative models of sources of secular involvement suggests that a number of variables might serve in a series of alternative hypotheses which would compete with those of Lenski. That is, other variables exist which may, in combination or isolation, prove that the simple Protestant-Catholic dichotomy needs further refinement as

an explanatory model. To support, modify, or reject the religious factor, the original Lenski hypotheses will be tested and compared with the results of the tests of alternative hypotheses.

DATA AND METHODOLOGY

The sources of our data are the combined 1974, 1975, and 1977 General Social Surveys (NORCGSS) (Davis 1977) conducted by the National Opinion Research Center.³ The sampling procedures used in these surveys result in a representative sample of the adult, noninstitutionalized, American population. The present study, given the focus of the analysis, includes only self-identified white Protestants and Catholics. This selection results in 2,285 Protestants and 987 Catholics.

We introduce two classes of dependent variables, depending on the degree to which each reflects secondary or primary types of relationships. The variable measuring the secondary dimension is defined as the respondents' total number of memberships in voluntary associations, excluding labor unions and church groups. Labor union memberships are excluded because they are not always considered voluntary. Membership in church-related groups is included in our analysis as a separate variable (see below). While our composite variable does not tap the extent of organizational involvement (i.e., the number of meetings attended and offices held), it essentially replicates what Lenski (1963, p. 244) describes as his measure of voluntary organizational involvement.

Three items measuring various types of primary sociability are analyzed separately and are formed by asking respondents the frequency with which

³ As our data consist of observations made at three times, a test for autocorrelation was conducted. The Durbin-Watson test yielded a value of 1.85, which is above both the lower and upper limits (see Johnston 1972, p. 252), indicating the absence of serially correlated error. We can thus safely ignore time and its attendant changes in the analysis. In addition, the distributions of the variables were examined for normality and homogeneity of variance. While almost all of the independent variables were normally distributed, the dependent variable *memvol* was found to fit a negative binomial distribution when a goodness-of-fit routine for various empirical to theoretical distributions was used. Heterogeneity of variance was also found to be present, and so, at the suggestion of Bartlett (1947) and Kendall and Stuart (1976), an inverse hyperbolic sine transformation was performed. The formula used was:

$$\begin{aligned} X &= \sinh^{-1} \sqrt{\frac{X+C}{K-2C}} \\ &= \ln \left(\sqrt{\frac{X+C}{K-2C}} + \sqrt{\frac{X+C}{K-2C} + 1} \right) \end{aligned}$$

(see Anscombe 1948, p. 246). In this formula, K is a known constant, in this case provided by the goodness-of-fit routine (see Gates and Ethridge 1970), and C is a constant which provides an optimal solution within a range of 0.20-0.40 (Anscombe 1948).

they visit relatives, friends, and neighbors (see Davis 1977, pp. 107-8). The main thrust of our analysis is on Lenski's concern for the interrelationships among religion, family, and voluntary organizations. However, Lenski (1963, pp. 216-17) does suggest that the religious factor has an impact on neighboring and, by implication, friendship patterns, and we have included the frequency of participation with friends and neighbors as two additional dependent variables.

Predictors of primary interest in this study were those reflecting religious commitment. Lenski (1963, p. 8) posited two types of commitment. One type involves "commitment of individuals to a socio-religious group" and the second concerns "commitment to a type of religious orientation which transcends socio-religious group lines." He (1963, pp. 20-23) further divided socioreligious group commitment into *associationalism* (attendance of religious services and of church-related group meetings) and *communalism* (having kin and friends with similar religious preferences). Religious orientation was divided into two separate types, *doctrinal orthodoxy* (agreement with the "prescribed doctrines of the church") and *devotionalism* (engagement in "private, or personal, communion with God") (Lenski 1963, pp. 24-26).

We utilized the following measures of religious commitment available in the NORCGSS: the frequency of church attendance, whether or not the respondent belongs to a church-affiliated group, and how intensely the respondent identifies with his or her religion. Obviously, our measures do not represent any aspect of communalism and devotionalism and only partially reflect associationalism and devotional orthodoxy. In particular, our indicators of church group participation do not measure the complexity of these involvements as well as Lenski's operationalizations did. The NORCGSS items measure only whether or not the respondent belongs to a church organization, while Lenski formed a composite index for associational involvement in terms of the number of meetings attended and offices held. Our indicator of orthodoxy measures essentially the intensity of religious identification, while Lenski measured the extent of acceptance of church doctrine. The operationalization of our measures of religious commitment as well as other variables is contained in the Appendix of this paper.

Education, income, age, sex, and size of community of residence were all measured by conventional questions. Ethnicity was measured by asking the respondent from what part of the world his ancestors came. In cases where more than one country was named, the respondent was asked to name the country to which he felt closest (see Davis 1977, pp. 39-40). Approximately 28% of the respondents either did not identify their background or did not choose only one country, and these are excluded from this part of the analysis. This exclusion may result in a sample bias, in part because "old line" Americans may be so well assimilated that they cannot conceive

of themselves as other than Americans. In addition, it is quite possible that a respondent is aware of more than one ethnic heritage and refuses to identify with one group to the exclusion of another. Consequently, the segment of the analysis involving ethnic background must be viewed with some care in that it is based on a smaller sample than the remainder of the analysis.

Given the fact that intra-Protestant (denominational) contrasts may be as great or greater than Protestant-Catholic differences themselves (Greeley 1972; Glock and Stark 1965), we have included the major denominational affiliation of Protestants (Baptist, Methodist, Lutheran, Presbyterian, Episcopalian, and other) in our model. We are aware that these categories include disparate groups (i.e., American and Southern Baptists were coded into the single Baptist category), but NORCGSS data permit no further disaggregation. Accordingly, this portion of the analysis should be interpreted with some caution.

We have chosen the general linear model, utilizing the multiple regression mode to test specific hypotheses and alternatives. As we are concerned with the differential effects of religious commitment and other factors across categories of religious and denominational preference and ethnicity, we also conducted an analysis of covariance with the appropriate test for homogeneity and commonness of slopes. Using a saturated model, which contains the additive and interactive effects of categorical variables (e.g., religious preference) and metric variables (e.g., education), we were able to ascertain whether Protestants and Catholics vary in their involvement in voluntary organizations, in social class, and so on. We could also test whether religious commitment or other factors are more important in determining membership in voluntary organizations for one religious group compared with another; and finally we could learn whether religious commitment and other factors are important determinants of organizational involvement, regardless of religious preference.

The relatively smaller sample size used by Lenski and the state of sociological statistics at that time precluded his use of an extensive multivariate analysis. We hope, given our advantage, to refine and extend the hypotheses that Lenski admitted were highly exploratory. Given the general lack of continuity between recognized "classics" such as *The Religious Factor* (Demerath and Hammond 1969) and subsequent research, one purpose here is to continue the analysis begun by Lenski.

FINDINGS

Zero-Order Relationships

Examining the zero-order correlation coefficients between religion and the remainder of the variables in the study, we find that Protestants are more

likely (45%) to belong to more church organizations than are Catholics (33%). However, Catholics attend church more frequently (see table 1). Protestants visit friends slightly more often than Catholics, but few differences are apparent in visiting relatives, neighboring, or joining voluntary organizations.⁴ Protestants in the sample tend to be slightly older than Catholics; Catholics reside in larger towns and cities than the Protestants. These two groups do not differ significantly in terms of their religious intensity, or in sex, income, education, and mobility.

Table 2 presents the zero-order Pearson correlation coefficients for Protestants and Catholics. We find that the number of voluntary organizational memberships for both groups is largely determined by similar variables, such as membership in church groups, frequent church attendance, frequent interaction with friends and neighbors, and higher socioeconomic status. Sociability with kin is unrelated to such secondary involvement for either group.

Both female Protestants and Catholics are more likely than males to join church organizations, but the opposite is true for secular organizations. Older Protestants are more likely to be members, but no such relationship is apparent for Catholics. On the other hand, Catholics from smaller cities and towns are less frequent joiners than are those from larger cities. It is now recognized that religious groups owe much of their communal solidarity (as evidenced in rates of intermarriage) to the fact that most are characterized by high territorial concentrations (Gannon 1978). Because Catholic immigrants tend to be concentrated in a few relatively large urban centers, Catholics in smaller urban areas are smaller minorities (proportionally as

TABLE 1
ZERO-ORDER PEARSON PRODUCT-MOMENT CORRELATION COEFFICIENTS FOR
RELIGION WITH ALL INDEPENDENT AND DEPENDENT VARIABLES, $N = 3,278$
(NORCGSS, 1974, 1975, 1977)

	Mem- church	Attend	Reliten	Soc- rel	Soc- friend	So- commun	Age	Sex
Religion.....	-.119*	-.119*	.003	.042*	.052*	.021	.089*	.003
	Size	Mobility		Income		Educ		Memvol
Religion.....	-.198*	.050*		-.049*		-.009		-.027

NOTE.—Religion is coded as a dummy variable; Protestant = 1, Catholic = 0. See the Appendix for an explanation of the variable abbreviations.

* Indicates a statistically significant correlation at the .05 level or better.

⁴ While Protestants and Catholics do not differ overall, they do differ in types of organizational memberships. Protestants are more likely to belong to fraternal, service, and farm organizations, while Catholics are more prevalent in sports and nationality groups. Extent of membership differs little in professional, literary, youth, hobby, Greek, school (i.e., PTA), political, or veterans' organizations.

TABLE 2

ZERO-ORDER PEARSON PRODUCT-MOMENT CORRELATION COEFFICIENTS FOR CATHOLIC ($N = 987$) AND PROTESTANT ($N = 2,285$) SUBSAMPLES
(NORCGSS, 1974, 1975, 1977)

	1	2	3	4	5	6	7	8	9	10	11	12	13	\bar{X}	SD
1 Reiten.....	1.0	.281*	-.431*	.030	-.008	-.006	-.155*	-.043	.001	.034	.046	.022	.100*	1.69	.64
2 Memchurch.....	.296*	1.0	-.469*	.005	.070*	-.017	.171*	-.069*	-.023	-.080*	-.080*	.056	.343*	1.67	.48
3 Attend.....	-.416*	-.609*	1.0	-.010	.028	.033	.173*	.109*	.012	.087*	.077*	-.053	-.204*	4.71	2.55
4 Socrel.....	.004	.045*	-.043*	1.0	.138*	.135*	.077*	-.073*	.196*	.107*	.119*	-.024	.004	3.29	1.55
5 Soccommun.....	-.013	.071*	-.088*	.132*	1.0	.217*	.188*	-.030	.022	.080*	-.090*	-.032	.073*	4.15	2.00
6 Socfrend.....	-.021	.038*	-.038*	.144*	.221*	1.0	.301*	.042	.095*	-.011	-.188*	.039	.138*	3.93	1.61
7 Age.....	-.124*	-.111*	.096*	.101*	.086*	.303*	1.0	.065*	.141*	-.146*	-.331*	.010	.050	42.94	16.62
8 Sex.....	-.118*	-.086*	.114*	-.045*	.004*	.022	-.012	1.0	.039	-.092*	-.046	.059	.113*	1.54	.50
9 Mobility.....	.029*	.017	-.004	.235*	-.016	-.035*	.100*	.003	.016	.002	.016	-.078*	-.020	1.83	.64
10 Income.....	.033*	-.056*	.014	.029	.031*	-.158*	-.244*	-.097*	.043*	.384*	.329*	-.076*	-.233*	8.52	2.91
11 Educ.....	.010	-.114*	.101*	.111*	-.047*	.192*	.313*	-.024	.141*	-.384*	.384*	-.046	-.357*	11.80	2.90
12 Size.....	.067*	.033*	.053*	.100*	.010	-.035*	.017	.013	.101*	-.019	.067*	-.012	.116*	12.23	19.13
13 Memvol.....	.053*	.263*	-.152*	-.029	.114*	.141*	.055*	.065*	-.081*	-.288*	-.403*	-.012	26.64	26.73	1.64
\bar{X}	1.69	1.55	4.01	3.44	4.25	4.10	46.11	1.54	1.90	8.15	11.74	6.87	26.64		
SD.....	.63	.50	2.61	1.58	1.97	1.58	17.23	.50	.83	3.06	3.08	10.36	1.75		

NOTE.—Catholic sample above the diagonal; Protestant below the diagonal.

* Indicates a significant coefficient; coefficients greater than .030, .042, and .059 are significant at the .05, .01, and .001 levels, respectively, for the Protestant subsample. Coefficients greater than .060, .081, and .096 are significant at the .05, .01, and .001 levels for the Catholic subsample.

well as numerically). This suggests that Catholics in smaller towns are more isolated from the mainstream of American Catholicism, especially if the local group cannot afford to support a parochial school system or other ancillary organizations, such as the Knights of Columbus. This leads to low religious solidarity via organized religion, thus lowering the number of memberships in voluntary organizations.

Younger, more poorly educated Protestants living in larger communities who socialize frequently with both neighbors and friends also socialize more frequently with relatives than do older, better-educated Protestants. Also, those Protestants who belong to church groups and attend church regularly interact more frequently with kin. These relationships, while not particularly strong, are in the direction opposite to that proposed by Lenski.

Lenski had assumed, prematurely perhaps, that massification would affect Protestant ethnic groups first because of their earlier immigration. It may be that Lenski's findings are limited because of the particular historical period during which his data were collected. At that time, Detroit, like other northern cities, had experienced a large and recent immigration of Protestants from the southern portion of the United States. These migrants, many of whom were male and temporary residents, would have had fewer relatives living in the city to visit.⁵

Both Hargrove (1979) and Lenski (1963) believe that Protestant theology, with its emphasis on the individual, encourages a tendency toward social atomization. A decrease in the strength of Protestant ethnic solidarity would merely reinforce this propensity to individualism. However, this view of Protestantism as the basis for nonfamily individualism is not supported by our data.

Catholics, unlike what Lenski would have led us to expect, are not more likely to be sociable with relatives when their church attendance is high or when membership in church groups is frequent. Family sociability for Catholics is more likely among those with lower income and education, females, younger age groups, and those who tend to be sociable with others.

Predicting Voluntary Organizational Involvement

The Lenski hypotheses are next examined by controlling for the alternative variables, shown above to be related to sociability and membership in voluntary organizations. Table 3 presents the results of regressing voluntary organizational membership on the Lenski variables plus socioeconomic variables, sociodemographic variables, and variables representing alternative forms of sociability.⁶ Roughly 25% of the variance in membership is ex-

⁵ This information was provided to us by Richard A. Hoehn on an earlier draft of this paper.

⁶ We included the three forms of sociability as predictors of involvement in voluntary

plained, but the standardized beta coefficients suggest only partial support for Lenski in that religious affiliation, frequent church attendance, and strong religious intensity are not significant predictors. And while it should be remembered that Lenski's (1963) measures of religious commitment differ somewhat from ours, once again it appears that his model should be revised.

TABLE 3
REGRESSION COEFFICIENTS FOR PREDICTION OF VOLUNTARY ORGANIZATIONAL INVOLVEMENT FOR THE MODIFIED LENSKI MODEL AND ADDITIONAL PREDICTORS, PROTESTANTS AND CATHOLICS (SE in Parentheses)

PREDICTOR VARIABLES	MEANVOL	
	b	Beta
Religion.....	.042 (.061)	.001
Reliten.....	.017 (.047)	.006
Memchurch.....	.842 (.067)***	.241
Attend.....	.006 (.013)	.009
Socrel.....	.018 (.018)	.016
Socfrend.....	.069 (.018)**	.064
Socommun.....	.054 (.014)**	.062
Age.....	-.007 (.002)**	-.069
Sex.....	.239 (.054)**	.069
Size.....	.005 (.002)*	.040
Mobility.....	-.044 (.033)	-.021
Income.....	-.081 (.010)***	-.142
Education.....	-.181 (.010)***	-.320
Constant = 27.480		
Adjusted R^2 = .250***		
Δ adjusted R^2 (saturated model-additive model)* = .012*		

* The change in the amount of variance explained and its statistical significance for the saturated model, with all possible interactions, versus the additive model, with main effects only.

* Significant at the .05 level.

** Significant at the .01 level.

*** Significant at the .001 level.

organizations to separate the possible contaminating effects of belonging to organizations *because* friends, neighbors, or relatives are also members. This permits an even clearer test of the religious factor. We also examined the effects of parental SES, generation, and feelings of alienation/life satisfaction on these models. These variables tend to be signifi-

A strong positive relationship is found for membership in church groups. That is, irrespective of religious affiliation, membership in church-related groups is associated with memberships in other voluntary associations. The best predictor, however, is the educational background of the respondent. Those Protestants and Catholics with higher levels of education are more likely to be joiners of larger numbers of voluntary groups. High income leads to similar results, as does being male, older, and sociable with friends and neighbors.

Given that Lenski suggests opposing effects of religious participation on secular involvements depending on religious affiliation, we suspect a statistical interaction. The test for homogeneity of slopes of the predictors in the model shows that significant interactions between religious preference and other variables indeed exist, suggesting different effects of various variables depending on whether the respondents are Protestant or Catholic.⁷ However, the substantive effect of the interaction is marginal. The addition of 10 interaction terms to the equation contributes only an additional 1% variance explained (see the last row of table 3).

Predicting Involvement in Primary Groups

Turning to sociability as a dependent variable, we find we are able to explain much less variation. There is some partial support for Lenski (see table 4). We find, as Lenski suggests, that higher frequency of membership in church groups leads to more frequent interaction with relatives for Catholics; but, unlike Lenski, we find that the same is true for Protestants. Education and age, however, are more powerful predictors than religious commitment. The younger and the less well educated tend to be among those who spend more time with kin, as do those who have remained stationary. Socializing with friends is not predicted by any of the religious variables (attendance, affiliation, membership in a church-related group) but instead is a function of age, education, and sex. Males, the better educated, and younger persons spend the most time with their friends. In this instance, the religious factor is nil.

cantly correlated with organizational membership and kin involvement. Their relationship, however, stems from the high intercorrelations among respondent SES, parental SES, and anomie. Finally, the effects of region were the subject of inquiry, given the potential regional biases contained in Lenski's (1963) work. A dummy variable for the midwestern (Michigan, Ohio, Indiana, Illinois, and Wisconsin) region versus other regions was formed, and the additive and interactive contributions to our ability to predict primary and secondary involvements were examined. No additional variance of a statistical or substantive nature was explained. An analysis of southern versus other regions was performed with a similar lack of additional predictive power.

⁷ The interactions provided little support for Lenski's religious factor. Catholics who attend church services and visit relatives more frequently, who have higher incomes, and who are more geographically mobile join organizations at greater rates than Protestants.

TABLE 4

REGRESSION COEFFICIENTS FOR THE PREDICTION OF INVOLVEMENT WITH KIN, FRIENDS, AND NEIGHBORS FOR THE MODIFIED LENSKI MODEL AND ADDITIONAL PREDICTORS, PROTESTANTS AND CATHOLICS (SE in Parentheses)

	SOCREL		SOCIATEND		SOCOMUN	
	b	Beta	b	Beta	b	Beta
Religion.....	.120	.035	.069	.020	.056	.013
	(.062)		(.061)		(.080)	
Reliten.....	-.025	-.010	.017	.007	-.093	-.030
	(.048)		(.047)		(.061)	
Memchurch.....	.143	.045	.130	.040	.288	.072
	(.068)*		(.068)		(.087)**	
Attend.....	-.015	-.025	-.011	-.018	-.049	-.065
	(.013)		(.013)		(.017)*	
Age.....	.011	.115	.027	.276	.163	.141
	(.002)***		(.002)***		(.002)***	
Sex.....	-.132	-.042	.121	.038	.048	.012
	(.055)*		(.055)*		(.071)	
Size.....	.004	.034	-.002	-.017	.001	-.009
	(.002)*		(.002)		(.002)	
Mobility.....	.365	.195	.037	.020	-.059	-.025
	(.033)***		(.033)		(.042)	
Income.....	.010	.019	-.010	-.019	.062	.094
	(.010)		(.010)		(.013)**	
Educ.....	.069	.134	-.045	-.087	-.017	-.025
	(.010)**		(.010)***		(.013)	
Constant.....	1.30		3.10		3.08	
Adjusted R ²077*		.103*		.029*	
Δ adjusted R ² (saturated model- additive model)*..	.000		.010*		.003	

* See table 3 for explanation.

* Significant at the .05 level.

** Significant at the .01 level.

*** Significant at the .001 level.

The young and those with lower incomes are also more likely to socialize with neighbors. Socializing with neighbors, on the other hand, is predicted by religious commitment, although the pattern is uniform for both Protestants and Catholics. Those who attend church services often and who join church groups also tend to spend more time with their neighbors, regardless of religious identification. According to these findings, religious commitment does not seem to reduce neighborhood sociability. On the contrary, religious activism in the form of membership in church groups fosters outgoing behavior.⁸

Summing up our results thus far, we note that Lenski predicted that high commitment to Catholicism leads to low attachments to voluntary organizations and high kin attachment. For Protestants, such commitment causes

⁸ With all three sociability models, we tested for statistical interactions using the same procedures as with the voluntary organizational model. Again, while we found that statistically the same regression line does not fit Protestant and Catholic responses equally well, the substantive differences are quite minor.

low kin involvement and high voluntary organizational membership. The present study finds that the effects of commitment operate in the *same fashion* for both Protestants and Catholics. Belonging to church organizations, in particular, increases the likelihood of membership in voluntary organizations and frequent interaction with kin and neighbors. In addition, secular factors such as education and age are more important predictors of involvements with kin, neighbors, friends, and voluntary organizations than is religious commitment.

The Effect of Ethnicity

We next examined 10 religioethnic groups (after Greeley 1974) to determine the possible existence of an "ethnic factor" in explaining the differential secular participation rates of Protestants and Catholics (see table 5). Ten religioethnic dummy variables were created and were entered into the equations utilized earlier in the analysis of organizational involvement and sociability. The interactions between the religioethnic dummy variables and other predictors were then examined.

This analysis resulted in no major deviation from the findings presented above for Protestants and Catholics. Membership in church groups tends, for all Catholic and Protestant ethnic groups, to be positively related to membership in voluntary organizations, with the dummy variables accounting for no real increase in the variance explained. In our analysis of interaction, however, belonging to church groups is a more powerful predictor of voluntary organization membership for Italian and German Catholics than it is for Polish or Irish Catholics. This interaction, however, adds little to the variance explained in the dependent variables (see the last row of table 5).

Thus adding ethnic differences to the analysis does not alter our original findings nor does it give any hint of additional support for Lenski's hypotheses. Ethnics tend to join different types of organizations,⁹ but the background factors that encourage joining tend, largely, to be the same.

In table 6 we have included the 10 religioethnic groups in the sociability equations summarized in table 4. Here we find that Italian Catholics have higher rates of kin and neighbor sociability, and Irish Protestants and Catholics also spend more time with their neighbors. These differences, however, contribute insignificant amounts to the variance explained by the three sociability models, and the interaction effects of the dummy religio-

⁹ For example, British Protestants are more likely (18%) to join professional societies than are other ethnic groups, with Italian Catholics the least likely (10%) to do so. Polish Catholics are most frequently associated with nationality organizations (11%), while German Protestants (1%) are the least likely to belong to such organizations. We will supply these data and others regarding ethnic voluntary organizational membership on request.

ethnic variables with the other predictors are similarly nil. In fact, the only change of importance from the findings in table 4 is that membership in church groups is no longer significantly related to sociability with kin.

The Denominational Effect

Given that the strength of religious commitment varies among Protestant denominations, it is possible that the effect of commitment on secular involvements varies as well. We thus examined the same models of primary and secondary secular involvements for various Protestant denominational affiliations by strength of religious commitment and background factors, treating the Protestant denominations available to us—Episcopal, Presby-

TABLE 5
REGRESSION COEFFICIENTS FOR THE PREDICTION OF INVOLVEMENT
IN VOLUNTARY ORGANIZATIONS FOR THE MODIFIED LENSKI MODEL
AND ADDITIONAL PREDICTORS, CATHOLICS AND PROTESTANT
ETHNIC GROUPS

PREDICTORS	MEMVOL		
	b	Beta	SE
British Protestant.....	-.051	-.011	.084
British Catholic.....	-.109	-.013	.140
German Protestant.....	-.033	-.007	.080
German Catholic.....	-.024	-.003	.134
Irish Protestant.....	.048	.006	.134
Irish Catholic.....	-.059	-.008	.113
Scandinavian Protestant.....	.264	.032	.297
Scandinavian Catholic.....	-.247	-.032	.280
Italian Catholic.....	-.198	-.024	.134
Polish Catholic.....	-.152	-.013	.186
Reliten.....	.018	.007	.047
Memchurch.....	.841	.241	.067***
Attend.....	.005	.008	.013
Socrel.....	.013	.015	.018
Socommun.....	.053	.061	.014**
Socfrend.....	.069	.064	.018**
Age.....	-.007	-.067	.002**
Sex.....	.235	.068	.054**
Size.....	.005	.042	.002*
Mobility.....	-.045	-.022	.033
Income.....	-.080	-.142	.010***
Education.....	-.180	-.318	.010***

Constant = 27.50

Adjusted R^2 = .249

Δ adjusted R^2 (saturated model-additive model)^a = .003

^a See table 3 for explanation.

* Significant at the .05 level.

** Significant at the .01 level.

*** Significant at the .001 level.

TABLE 6

REGRESSION COEFFICIENTS FOR THE PREDICTION OF INVOLVEMENT WITH KIN, FRIENDS, AND NEIGHBORS FOR THE MODIFIED LENSKI MODEL AND ADDITIONAL PREDICTORS, CATHOLIC AND PROTESTANT ETHNIC GROUPS (SE in Parentheses)

PREDICTORS	SOCREL		SOCFRIEND		SOCOMMUN	
	b	Beta	b	Beta	b	Beta
British Protestant..	.050 (.086)	.011	-.083 (.085)	-.019	-.211 (.110)	-.039
British Catholic....	.220 (.142)	.028	-.089 (.141)	.011	-.008 (.182)	-.001
German Protestant.....	.097 (.081)	.022	-.115 (.080)	-.026	.029 (.104)	.005
German Catholic....	.217 (.136)	.028	-.163 (.135)	-.021	-.034 (.078)	-.004
Irish Protestant....	.118 (.136)	.015	-.202 (.135)	-.026	-.358 (.175)*	-.037
Irish Catholic.....	-.091 (.115)	-.014	-.021 (.114)	-.003	-.478 (.147)*	-.059
Scandinavian Protestant.....	.302 (.301)	.041	.053 (.300)	.007	-.356 (.388)	-.038
Scandinavian Catholic.....	-.348 (.284)	-.049	-.449 (.283)	-.063	.137 (.366)	.015
Italian Catholic....	-.523 (.136)**	-.069	-.145 (.135)	-.019	-.470 (.175)*	-.049
Polish Catholic....	-.190 (.189)	-.018	.093 (.188)	.008	-.022 (.240)	-.002
Reliten.....	-.025 (.048)	-.010	.013 (.047)	.005	.093 (.061)	-.030
Memchurch.....	.130 (.067)	.041	.110 (.067)	.034	.283 (.087)**	.070
Attend.....	-.020 (.013)	-.033	-.015 (.013)	-.025	.052 (.017)**	-.069
Age.....	-.011 (.002)***	.117	.026 (.002)***	.280	.017 (.002)***	.147
Sex.....	-.125 (.055)*	-.040	.125 (.055)*	.039	.059 (.071)	.015
Size.....	.004 (.002)*	.039	-.002 (.002)	-.021	-.001 (.002)	-.008
Mobility.....	.362 (.033)***	.193	.036 (.033)	.019	-.053 (.042)	-.023
Income.....	.008 (.010)	.016	-.008 (.010)	-.017	.062 (.012)***	.095
Education.....	.067 (.010)***	.129	-.042 (.010)***	-.081	-.011 (.013)	-.018
Constant.....	1.44		3.15		3.12	
Adjusted R^2079		.104		.033	
Δ adjusted R^2 due to saturated model..	.001		.010		.023*	

* Significant at the .05 level.

** Significant at the .01 level.

*** Significant at the .001 level.

terian, Baptist, Lutheran, Methodist, and other Protestants—as dummy variables. The results, shown in tables 7 and 8, again differ little from our earlier analysis. Although Episcopalians belong to significantly more organizations than do other Protestants, these dummy variables contribute only an additional 0.2% to the variance explained. The interaction of denomina-

TABLE 7
REGRESSION COEFFICIENTS FOR THE PREDICTION
OF INVOLVEMENT IN VOLUNTARY ORGANIZATIONS
FOR THE MODIFIED LENSKI MODEL AND ADDI-
TIONAL PREDICTORS, PROTESTANT DENOMI-
NATIONS (SE in Parentheses)

PREDICTORS	MEMVOL	
	b	Beta
Baptist.....	.092 (.075)	.021
Methodist.....	-.134 (.083)	-.027
Lutheran.....	-.034 (.089)	-.006
Presbyterian.....	-.213 (.118)	-.029
Episcopalian.....	-.356 (.157)*	-.036
Reliten.....	.021 (.047)	.008
Memchurch.....	.832 (.066)***	.238
Attend.....	.001 (.013)	.000
Socrel.....	.020 (.018)	.018
Socommun.....	.053 (.014)**	.061
Socfrend.....	.065 (.018)**	.060
Age.....	-.006 (.002)**	-.057
Sex.....	.247 (.054)***	.072
Size.....	.005 (.002)*	.040
Mobility.....	-.038 (.033)	-.019
Income.....	-.078 (.010)***	-.138
Education.....	-.174 (.010)***	-.308
Constant = 27.38		
Adjusted R ² = .252		
Δ adjusted R ² due to saturated model* = .000		

* See table 3 for explanation.

* Significant at .05 level.

** Significant at .01 level.

*** Significant at .001 level.

tion with the other predictors contributes an even smaller amount (see table 7).

Turning to sociability, we again find that Episcopalians spend less time with their relatives, but that no other denominational affiliation significantly predicts sociability with kin, friends, or neighbors (see table 8). Again, little additional variance is explained either by the inclusion of the dummy denomination variables or by their interaction with other predictors. Thus, denominational background, like ethnicity, alters our original conclusions concerning the Lenski hypotheses very little.

TABLE 8
REGRESSION COEFFICIENTS FOR THE PREDICTION OF INVOLVEMENT WITH KIN,
FRIENDS, AND NEIGHBORS FOR THE MODIFIED LENSKI MODEL AND ADDI-
TIONAL PREDICTORS, PROTESTANT DENOMINATIONS (SE in Parentheses)

PREDICTORS	SOCREL		SOCFRIEND		SOCOMMUN	
	b	Beta	b	Beta	b	Beta
Baptist.....	.039 (.077)	.009	.221 (.076)*	.053	-.063 (.099)	-.012
Methodist.....	.013 (.084)	.003	-.006 (.083)	-.001	-.308 (.108)	-.054
Lutheran.....	-.056 (.091)	-.011	-.110 (.090)	-.022	-.006 (.117)	-.001
Presbyterian.....	-.075 (.121)	-.011	-.188 (.119)	-.028	-.220 (.155)	-.026
Episcopalian.....	.429 (.160)*	.048	-.265 (.158)	-.029	-.045 (.205)	-.004
Reliten.....	-.025 (.048)	-.010	.085 (.047)	.007	-.086 (.061)	-.028
Memchurch.....	.116 (.067)	.036	.118 (.067)	.037	.249 (.087)*	.062
Attend.....	-.020 (.013)	-.032	-.017 (.013)	-.028	-.060 (.017)**	-.079
Age.....	.001 (.002)***	.115	.027 (.002)***	.288	.017 (.002)	.149
Sex.....	.135 (.055)*	-.043	.130 (.055)*	.041	.049 (.071)	.012
Size.....	.003 (.002)	.027	-.002 (.002)	-.017	-.002 (.002)	-.017
Mobility.....	.362 (.033)***	.194	.043 (.033)	.023	-.056 (.042)	-.024
Income.....	.008 (.010)	.016	-.007 (.010)	-.013	-.061 (.013)***	.094
Education.....	.068 (.010)***	.131	-.037 (.010)**	-.071	-.013 (.013)	-.020
Constant.....	1.48		2.92		3.20	
Adjusted R ²074		.107		.030	
Δ adjusted R ² due to saturated model..	.002		.000		.010*	

* See table 3 for explanation.

* Significant at the .05 level.

** Significant at the .01 level.

*** Significant at the .001 level.

SUMMARY OF FINDINGS

Our analyses lend scant support to the religious affiliation-secular involvement hypotheses found in *The Religious Factor*. On the other hand, the alternative models of primary and secondary involvement fare little better. Religious commitment does foster greater participation in secondary organizations, but it does so for both Protestants and Catholics.

With regard to primary ties, we again found only partial support for Lenski. Religious commitment leads to greater kin and neighborhood involvements for Catholics, supporting Lenski's contention, but the same is true for Protestants, which is the opposite of what *The Religious Factor* proposed. Thus, we find a religious factor of "commitment" operating similarly for Protestants and Catholics. In this context, religious commitment aids in providing linkages (through religious involvement) from the individual to the world at large, irrespective of religious affiliation.

We find little evidence for an ethnic factor in explaining primary and secondary relationships. Religious commitment, while slightly more powerful for some groups than others, tends to be associated with increased organizational joining for all of our religioethnic groups. An ethnic factor is more powerful when we examine kin and neighborhood interaction, but the support for Lenski is once again only partial.

Somewhat more consistent support for the religious factor appears when we examine intra-Protestant denominational differences. Episcopalians belong to more organizations and interact less with their relatives than do other Protestants, but the variance explained by denomination in both membership in voluntary organizations and sociability with relatives is low. Furthermore, the religious commitment of this or any other Protestant denomination has little effect on sociability with kin.

The remaining alternative models (the socioeconomic, sociodemographic, and urbanization-massification) provide additional predictive power, but they do not explain away the effect of religious commitment on organizational and primary group participation. In this respect, the religious factor, while operating in the same fashion for Protestants and Catholics, remains significant. But the effects of such a religious factor must be understood in concert with more secular variables, as Lenski (1963, pp. 26-27) suggested. Lenski was well aware that a multivariate approach would be necessary to explain secular movements fully.

Higher income and education, older age, living in larger towns, and being male, along with a stronger religious commitment, explain such involvement. This holds true for both Catholics and Protestants, although some variation in this general pattern does occur when more specific subgroups are examined. Similar statements can be made about the sociability vari-

ables. Education, age, sex, mobility, and city size explain as much, or more, of the variation in sociability with relatives as do religious preference and commitment. Individuals who have many of the characteristics that cause them to join organizations are those who are less likely to interact frequently with kin. Those who are younger, female, and have fewer educational attainments spend more time with their relatives. However, those who come from larger cities and who have strong religious commitment not only join organizations but also interact frequently with kin. This particular finding is at odds not only with Lenski, but also with what the urbanization literature would have us expect. City size may not decrease kin interaction, as classical theory suggests, but even more recent statements have not led us to expect *greater* kin involvements in cities.

CONCLUSIONS

The principal thrust of Lenski's *Religious Factor* is that religious life links Protestants and Catholics to secular life in different ways. We have found that religion does indeed provide such a linkage, but it operates in largely the same fashion for Protestants and Catholics.

A partial explanation for the lack of concurrence between Lenski's findings and ours is the nature of the two samples used. Lenski's sample is made up entirely of Detroit residents in 1958; ours is composed of national samples taken in 1974, 1975, and 1977. The United States has changed considerably in the past 20 years; in addition there is little to indicate that conditions found in Detroit represented those in the rest of the country during the 1950s.

We believe, however, that a better explanation lies in the changing nature of American Catholicism that D'Antonio (1980), McCready and Greeley (1972), Greeley (1977), and Lenski (1971) himself have noted. Since World War II, the Roman Catholic minority in the United States has become less isolated from American society. The decreasing importance of the parochial school system, Vatican II, and the election of a Catholic as president have all indicated a greater rapprochement of Catholics and Protestants. Additionally, very fundamental differences between Protestants and Catholics on moral issues have eroded to such a degree that McCready and Greeley (1972) speculate on the "end of American Catholicism." Although, as Hammond (1979) has suggested, religion was closely allied with political issues (e.g., abolition and prohibition) in the past, it appears that strong Protestant-Catholic differences have now diminished.

None of this, however, explains why religious commitment, whatever its form, has such an extensive impact on secular life. A number of seculariza-

tion theories have suggested that there is, with the increasing differentiation and specialization in industrial societies, an increase in institutional autonomy (Parsons 1963; Luckmann 1967; Fenn 1972; Greeley 1972). Our data suggest that a major link between otherwise autonomous, dissimilar secular activities lies in religious commitment: those who are active in religious affairs are also more likely to be active in both organizations and primary groups.

The importance of this link becomes clear when we recall the effect of social class on primary and secondary involvements. Social class differences lead to different types of involvement, with upper-status individuals belonging to more organizations, spending more time with neighbors and friends, and spending less time with relatives. That is, social class background further differentiates and divides Americans. What we have shown, however, is that where social class divides, religion unites. Religious commitment leads to greater secondary and primary ties, in particular tying family, neighborhood, and voluntary organizations together.

The idea that religion has a unique effect is lent further weight when it is recalled that the participatory dimension of religious commitment is more than just another object of upper-class consumption. Recent research has demonstrated the weakness of social class as an explanatory variable, accounting for no more than 5% in the variance explained in church attendance (see Mueller and Johnson 1975; Alston and McIntosh 1979). Instead, the most powerful predictors of religious participation tend to be other measures of religious commitment. Those for whom religion has salience and who hold strong religious convictions tend to participate most frequently in religious services (see Alston and McIntosh 1979; Davidson and Knudsen 1977; Roof and Perkins 1975).¹⁰

Religion, from this point of view, serves as a bridge between various institutional involvements and thus is a major source of social cohesion. More specifically, religious affiliation, according to Gannon (1978), links together various social services, continues as a major source of cultural socialization, and provides a sense of "communal identity" in the face of

¹⁰ In addition, we analyzed the membership-in-church-organizations variable, using the same predictors found in table 3 (excluding, of course, memchurch) and found that the most important effects are the intensity of religious beliefs (beta = .271, significant at the .001 level), age (beta = .555, significant at the .001 level), education (beta = .142, significant at the .05 level), religion (beta = .103, significant at the .05 level), income (beta = .061, significant at the .05 level), and sex (beta = .071, significant at the .05 level). Religious intensity and religion are clearly among the more important of the predictors of membership in church groups; this view is reinforced by the fact that over 75% of the variance explained in membership in church organizations is accounted for by the intensity of religious beliefs and religious affiliation.

declining ethnic attachments. In this sense, the church can be viewed as the symbolic, if not the physical, hub of local communities in urban areas.

We propose that Americans are drawn into religious activities by their beliefs and by the salience of those beliefs. These beliefs, in turn, lead to greater involvements with family, neighbors, and organizations. The fact that this process works in the same manner for Protestants and Catholics suggests that religious commitment functions in a general fashion, rather than in the affiliation-specific manner proposed by Lenski.

APPENDIX

Selected Items Utilized in the Retest and Extension of the Lenski Model

1. *Size of place* [Var: size]

Size of place in thousands—a 4-digit number which provides actual size of place of interview (source of data is the 1970 U.S. census).

2. How often do you attend religious services? (Use categories as probes, if necessary.) [Var: attend]

Response	Punch
Never.....	0
Less than once a year.....	1
About once a year.....	2
Several times a year.....	3
About once a month.....	4
2-3 times a month.....	5
Nearly every week.....	6
Every week.....	7
Several times a week.....	8
Don't know, no answer.....	9

3. *Ask everyone with any religious preference named in Q. 82.* Would you call yourself a strong (preference named in Q. 82 or 82-A) or a not very strong (preference named in Q. 82 or 82-A)? [Var: reliten]

Response	Punch
Strong.....	1
Not very strong.....	2
Somewhat strong (Vol.).....	3
Don't know.....	8
No answer.....	9
Not applicable.....	
(Punch 4 in col. 7).....	BK

4. Would you use this card and tell me which answer comes closest to how often you do the following things:



A. Spend a social evening with relatives (friends, neighbors)? [Var: socrel; socrend; soccommun]

Response	Punch
Almost every day.....	1
Once or twice a week.....	2
Several times a month.....	3
About once a month.....	4
Several times a year.....	5
About once a year.....	6
Never.....	7
Don't know.....	8
No answer.....	9
Not applicable.....	BK

5. Now we would like to know something about the groups and organizations to which individuals belong. Here is a list of various kinds of organizations. Could you tell me whether or not you are a member of each type. [Read each item. Code one for each.] [Var: memfrat, memserve, memvet, mempolit, memsport, memyouth, memschl, memhobby, memgreek, memnat, memfarm, memlit, memprof, memchurch, memother] [For more details concerning these and other items found in the analysis, refer to Davis (1977)]. For our analysis of voluntary organizations (memvol), we added up the number of positive responses to the above list of organizations, excluding memchurch.

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Women and Weak Ties: Differences by Sex in the Size of Voluntary Organizations¹

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This paper explores some network consequences of dramatic differences between men and women in the typical size of the voluntary organizations they belong to. These size differences are greatest in organizations that are most economically oriented. Furthermore, the differences are remarkably consistent across social categories; men tend to belong to larger organizations when compared with women in similar categories, whether of work status, age, education, or marital status. Men are located in core organizations which are large and related to economic institutions, while women are located in peripheral organizations which are smaller and more focused on domestic or community affairs. Even though men and women have almost exactly the same number of memberships on the average, the dramatic differences in the sizes and types of their organizations expose men to many more potential contacts and other resources than women.

The informal network of relations which binds society together affects powerfully the distribution of resources to individuals. Hundreds of studies have explored aspects of these networks, from the early community studies which outlined the basic dimensions of class systems to the modern network analyses of elites which construct sophisticated mathematical descriptions of the social organization of power. The links connecting people in this system of informal relationships have been called "weak ties" by Granovetter (1973, 1974), who shows that these ties are the channels through which vital information about the system passes. For instance, Granovetter demonstrates that people first hear through informal contacts about the jobs they later occupy. Similarly, Boissevain (1974) shows that the indirect links generated by informal contacts ("friends of friends") explain a great deal about the operation of educational and employment systems.

Sex differences in the network of informal relationships are less well understood. Impressionistic literature suggests that the Old Boy system of

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recruitment for jobs and the use of personal contacts for instrumental purposes operate to the disadvantage of women and the advantage of men (Lipman-Blumen 1976; Welch 1980). However, most of the information available comes from the Granovetter and Boissevain research, which studied primarily male contacts. The types and amounts of contact between individuals in the system almost certainly affect sex differences in the distribution of opportunities and rewards, but only recently have researchers studied the differences in the social contacts of men and women (Jackall 1978; Fischer and Olicker 1980).

One relevant comparison between men and women which has been studied extensively is the difference in the number and type of voluntary organizations to which men and women belong. For instance, Scott (1957) found that women tend to belong to fewer organizations than men. Similarly, Booth (1972) found that women belong to slightly fewer organizations and also tend to belong to different types of organizations than men do. In particular, women tend to belong to organizations which are "basically accommodative and nurturant in character" (p. 188). Men, on the other hand, tend to belong to organizations "which are organized to cope with the external environment." In other words, women join organizations that are oriented toward domestic affairs, while men join organizations that have instrumental or economic goals.

One reason why sex differences in voluntary association membership are important is that these organizations create an opportunity structure for interpersonal contacts (McPherson 1981*b*). Voluntary associations form important settings for social interaction. Members who become acquainted through organizational activities influence one another's behavior (Whiting 1980) and provide information about matters outside their immediate environments (Jones and Crawford 1980). Thus differences by sex in voluntary affiliation may create differences in the probability that men, as opposed to women, will be exposed to useful information and potentially important acquaintances.

One aspect of voluntary associations which is particularly crucial for the network of informal relations is the size of a given organization. Large organizations generate more potential acquaintances. One could argue that the contacts which occur in a larger organization are more "superficial" than those in smaller organizations. However, as the weak-tie literature shows, this very superficiality may itself be an advantage when combined with a large volume of such contacts (Lin, Ensel, and Vaughn 1980). Information about job openings and other opportunities is likely to flow through weak ties, as opposed to the strong bonds of close friendship or kinship (Granovetter 1973; Lin et al. 1980; Jones and Crawford 1980; Lin, Vaughn, and Ensel, in press). Thus, large organizations provide an opportunity base for large volumes of weak ties.

Another possible objection to our assertion that largeness is advantageous is that a larger organization may have more internal structure, that is, it may contain subgroups or exhibit more formalized operation. Actually, an internal structure may heighten the probability of useful contacts among members. Larger organizations often develop institutionalized arrangements for passing along information to their members. For example, the membership roster distributed to all members of an organization facilitates inter-member contact, even when the organization is large and members are not initially acquainted. The employment bulletin of the American Sociological Association and the placement services at annual meetings are examples of these arrangements.

Just as important is the fact that smaller organizations tend to be less salient in the community than large ones simply because they have fewer members. Larger organizations can generate more publicity for their activities through word of mouth, are more newsworthy, are more likely to have members with media influence, and so forth. In addition, larger organizations have more members to sustain any given activity than smaller organizations.

Thus, there are four major points which argue for the importance of size: (1) any organization, no matter how large, tends to produce a higher density of connections among its members than exists among randomly chosen nonaffiliated persons; (2) any formal structure generated by increased size because of the relationship between size and differentiation (Mayhew et al. 1972) tends to promote the flow of information through the system; (3) an organization's size is likely to determine its salience or importance in the community; and (4) size influences the resources an organization can mobilize to accomplish its goals.

In the paragraphs above we have argued that the size of voluntary organizations is an important element in the opportunity structure of interpersonal contacts. Since the size of such organizations has a strong effect on the network of social relations, it is surprising that no systematic comparison of the organizations men and women belong to has ever been made. In this paper, we will explore the relationship between sex and organization size in several ways. First, we will demonstrate that more men than women belong to large organizations. We will then examine differences in organization size by sex for different types of voluntary associations. Since size is often an advantage in the economic sector, we expect instrumental, economically oriented, voluntary organizations to show a different pattern than social, community, and family-based associations.

In the second part of our analysis, we consider men and women with different social characteristics and examine the size of the organizations to which they belong. We examine differences in men's and women's organization sizes as they are associated with employment status, occupation, age,

education, and marital status. This allows us to ascertain whether these factors are related to organization size in the same ways for men as for women. For example, we ask whether employment status is more closely related to organization size for women than for men. We also examine whether differences in organization size are related to differences in men's and women's involvement in organizations.

We then attempt to account for the sex differences in organization size with the variables we have examined. We ask whether these differences may be explained by the fact that men and women belong to different types of associations, or by the fact that men are more likely than women to have certain social characteristics that are positively related to organization size.

Finally, we discuss a number of structurally important consequences of the results and offer some possible explanations for the remaining differences in organization size for men and for women.

DATA AND METHOD

The data are from a representative probability sample of 1,799 noninstitutionalized adults living in Nebraska in 1977. Organizations respondents belonged to were identified by aided recall techniques (Babchuk and Booth 1969). The records show the name of the organization, its size and other relevant characteristics, and any unusual circumstances noted by the respondent. Extremely detailed information on the sampling design, the interview schedule, and all other features of the study appears in Johnson (1977).

The unique feature of this data set is that it contains information on the size of organizations reported by a probability sample of individuals. When the organization was a local chapter of a state, national, or international organization, the local unit's size was the variable reported. When no local group existed, the size variable was coded "missing" for this analysis. Several different forms of the size variable (e.g., logarithmic, categorical) were analyzed to investigate the consequences of the extremely skewed distribution of organization size. No substantive differences emerged among these alternative analyses.

The other variables we use are sex, age (seven categories), education (five categories), work force status (five categories), marital status (four categories), organization involvement (two categories), and organization type (14 categories). Since our basic interest lies in the relationship between sex and organization size, we regard the other variables primarily as control variables whose effects we want to take into account in examining the sex-size hypothesis. The analysis is a simple least-squares technique which calculates mean of organization size for men and women within categories of the control variables. We then adjust these means for the simul-

taneous effects of other independent variables with a procedure based on dummy variable regression analysis (Suits 1957).

Since the dependent variable is the size of an organization, the unit of analysis is the membership rather than the individual. This shift of focus from the individual to the membership is necessary because an individual will belong to organizations of different sizes; the individual's characteristics remain invariant across multiple memberships. When individuals report multiple memberships, the characteristics of an individual are replicated as many times as there are memberships. This procedure produces a data set of 2,462 observations.² We analyze this sample as though it were a random sample of memberships from the universe defined by all memberships in all organizations in the system. Our sample is similar to such a random sample in several important ways. Most important, the distribution of individual characteristics, such as sex, age, and so forth, in our sample is similar to that which would be found in a true random sample of memberships, since the individuals replicated in our sample stand as proxies for similar individuals who would be found independently in a random sample. For example, since the number of memberships is strongly correlated with education, our sample replicates highly educated individuals more often than those with less education. But, since there are more highly educated individuals in the population of memberships, these replicates merely reflect the fact that there is a greater proportion of highly educated individuals in the population of memberships than in the population of individuals (McPherson 1978). Since using memberships as the unit is somewhat unorthodox, we present evidence later to show that our results do not change substantively when the individual is the unit of analysis.

RESULTS

Type and Size of Organizations

Figure 1 presents a bar chart which compares organization size for men and women. The bars representing the overall sizes of organizations men belong to and those women belong to (the pair at the extreme left) show

² The overall mean affiliation rate is 1.64; the mean numbers are 1.61 for men and 1.69 for women. The distribution of memberships is as follows (first number is number of memberships, second number is frequency): 0-579, 1-455, 2-336, 3-190, 4-109, 5-64, 6-39, 7-20, 8 or more-17. If we had complete data on all memberships, the mean rate would produce 2,963 total observations in the sample of memberships. Because we are interested only in organizations which meet locally, and because we do not have complete information on all organizations mentioned, our final sample of memberships contains 2,462 cases. Approximately 65% of the missing cases are memberships for which the respondent did not report a local organization size. The other 35% were eliminated from the analysis because they were inappropriate (e.g., reporting national membership rather than local membership).

that men's organizations are, on the average, three times the size of women's. This difference is especially striking because it is apparent in almost every category. One of the largest differentials occurs in the fraternal-sororal category, with men's organizations averaging more than four times the size of women's. The percentage of the within-sex total number of memberships constituting each type is presented in parentheses below the bars. As the figures show, fraternal-sororal memberships constitute about 13% and 10% of the total memberships of men and women, respectively. Thus, while there is roughly the same proportion of female and male memberships in fraternal-sororal organizations, the women's memberships are spread out over a larger number of smaller organizations, while the men's organi-

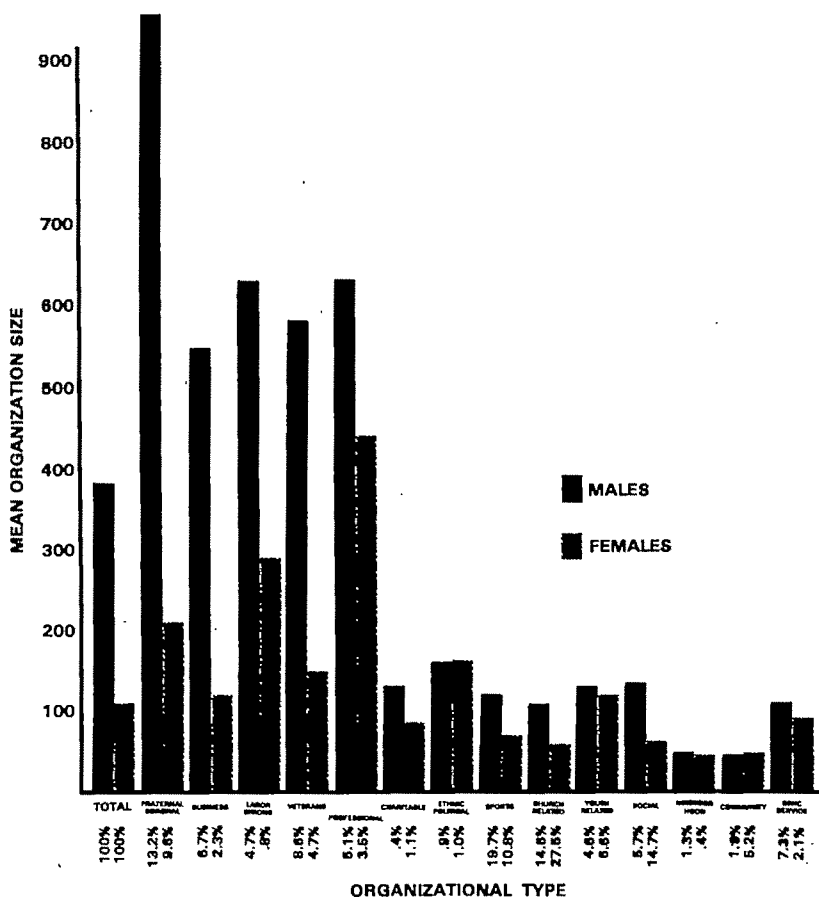


FIG. 1.—Organization size and type of organization by sex. Data for this and all other figures in this article are described by Johnson (1977).

zations are fewer in number but much larger.³ One reason for the size difference is that women often belong to the women's auxiliaries of men's organizations. The fact that women's organizations are subordinate to fraternal associations forces the former to be smaller, since not all wives of male members belong to the auxiliary and not all male members are married. This reasoning also applies to the size differences among veterans' groups.

The differences between men and women are also quite striking in business organizations. The size ratio of men's business associations to women's is over four to one. Here, however, there are nearly three times as many male memberships in business organizations as female memberships. Thus, not only are women less likely to belong to business organizations, but also the organizations that they do belong to are much smaller and therefore less likely to figure prominently in the operation of the system.

The size differential in professional organizations is not as large as that in business-related organizations, probably because professional organizations are usually affiliated with an umbrella organization such as a statewide or nationwide professional association. These umbrella organizations are likely to impose lower limits on the sizes of chapters or local affiliates, thus forcing a reduction in the size differential between men's and women's organizations. Note, however, that even within professional organizations, there is a size difference of approximately 40%—nearly 200 members per organization.

It is surprising that labor unions, for which large size is an obvious advantage, show such a dramatic difference: men's labor unions are over twice as large as women's. Taken together with the finding that there are nearly six times as many male union memberships to begin with, this fact assures male domination in the union sector of the voluntary system.

We thus find dramatic evidence that the organizations which shape the flow of information and other resources in the economic arena differ considerably in size for men and women. As we move away from the business realm to those areas more traditionally occupied by women, such as neighborhood, youth, and community organizations, we see a clear reduction in both the size of organizations and the size differential.

³ At the level of the total system, there is a relationship between the number of memberships and the average size of organizations. This relationship is $\bar{M} = \bar{S}K/N$, where \bar{M} is the mean number of memberships per person, \bar{S} is the average size of organizations, K is the total number of organizations in the system, and N is the number of individuals. The product $\bar{S}K$ is the total number of memberships in the system—the number of organizations times the average size of organizations (McPherson 1978). The usual strategy for past survey research is to compare \bar{M} across different groups or categories. We are interested in \bar{S} , a structural characteristic, which is related to \bar{M} through K , the number of organizations. Since \bar{M} in these data is almost exactly the same for men and women, a large value of \bar{S} for men implies smaller K for men. We use this relationship between \bar{S} and K throughout the paper.

Individual Characteristics and Organization Size

We now turn to the question, Are the size differences consistent across social categories such as employment status, age, and education? Figure 2 presents the mean sizes of organizations men belong to and those women belong to in various categories of work force participation. As the figure shows, there are dramatic differences among the categories for men. Men who work are members of organizations which are over four times the size of those belonged to by men who are keeping house and nearly four times the size of the ones male students belong to.

Even more striking is the fact that the differences between men and women in traditionally female or dependent roles are greatly reduced. When men and women are in school or keeping house, they belong to organizations very similar in size. When women are in the labor force full-time they tend to belong to much larger organizations than housewives but still nowhere near as large as those of men in the labor force full-time. Thus we find very strong evidence that the size differences in men's and women's organizations are related to differing positions held in the economic sector. Obviously there is a reciprocal relationship between membership in a job-related organization and having a job. Since women are less likely to be employed (U.S. Department of Labor 1977), they will be less likely to join such organizations. In addition, since they are more likely to have discontinuous work histories (Polachek 1975, 1976; Smith-Lovin and Tickamyer 1980), participation in work-related organizations is less likely to be beneficial to them even when they are employed (e.g., a women who is employed for one year as a teacher will not be as likely to join a teacher's

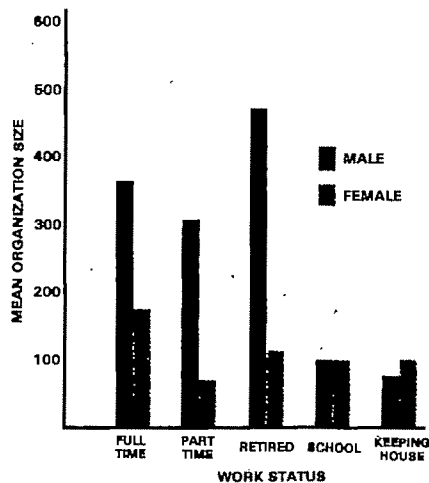


FIG. 2.—Organization size differences by sex for different types of employment status

professional association as the continuously employed career teacher). Thus, we expect to find a smaller proportion of women's memberships in the large job-related organizations, and therefore the average size of women's organizations will be smaller.

Figure 3 amplifies this fact. Among working men and women, there are strikingly consistent size differences across all categories of work. These differences are greatest in the areas traditionally dominated by men (such as craftsmen and foremen) and in areas with sex-segregated categories of employment (such as sales and service). Craftsmen and laborers will be very likely to belong to unions, which are very large, as figure 1 demonstrates. Since the sales jobs occupied by men may rely on contacts in the system more than the sales jobs occupied by women (e.g., insurance sales vs. retail store sales), we might expect men to choose larger organizations in order to generate more contacts. The differences between men and women are smallest in the farm-related category, primarily because organizations in rural areas are smaller than their urban counterparts (McPherson and O'Donnell 1980). Thus, figure 3 shows that the differences between men and women who work are consistent across all categories of employment; in no occupational category do women belong to organizations even half as large as those of men.

Figure 4 examines the relationship between age and organization size for

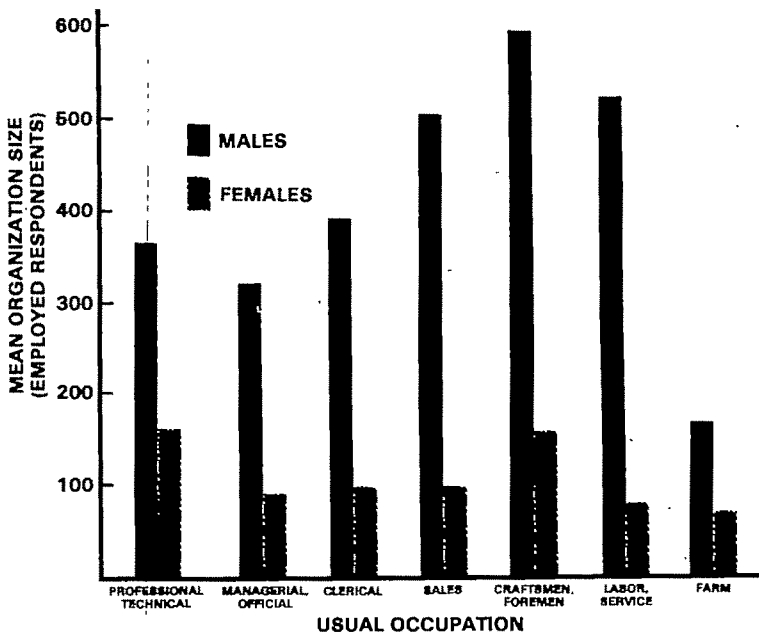
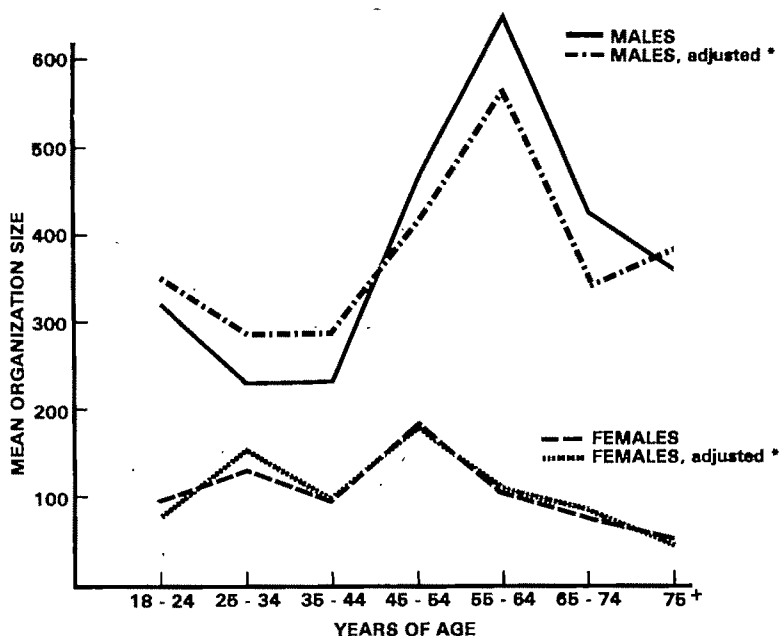


FIG. 3.—Organization size differences by sex for different types of employment



* SEE TEXT FOR DETAILS OF ADJUSTMENT PROCEDURE.

FIG. 4.—Relationship between age and size of voluntary organizations by sex

each sex. This figure shows that there is a peak in the average size of voluntary organizations for men between the early adult years and ages 45–65. Though not presented, there is also a peak in men's rates of membership in this age range. The confluence of these peaks suggests that the increased volume of memberships is not offset by a corresponding increase in the number of organizations in the system (see n. 3). Since previous research has noted that men tend to belong to more job-related organizations in approximately this age range (Knoke and Thomson 1977; McPherson and Lockwood 1980), we need to test the possibility that this peak is due to an increased proportion of memberships in these larger organizations. The broken line for males presents the mean sizes adjusted with a least-squares procedure which statistically equalizes the type of organization across age categories.⁴ The observed peak at ages 45–65 is not due entirely

⁴ The procedure is a dummy variable regression analysis procedure, in which age is represented by a set of six dummy variables and organization type by 13. All variables were entered into a regression equation, and the means for each age category, adjusted for the effects of organization type, were reconstructed from the estimated coefficients. This procedure is followed for men and women separately in an approach related to classical analysis of covariance (Schuessler 1969). The resulting set of means may be interpreted as the means for men (or women) which we would observe in a hypothetical set of men (or women) who did not differ in the type of their memberships. Note

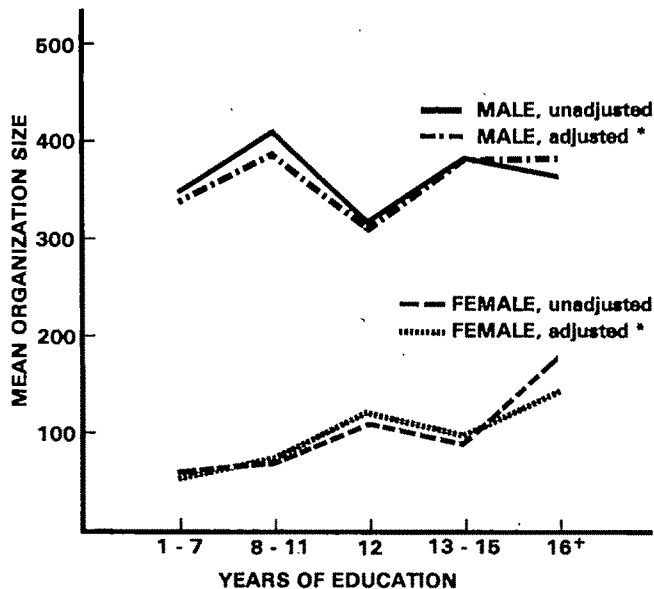
to differences in organizational type. Although the peak is slightly flattened by the adjustment, it is not eliminated. Thus, it could still be the case that large, powerful organizations are more useful to these men and that these men are more useful to the organizations because they are at the peaks of their careers.

Another possible explanation for this peak in the middle-aged range lies in the fact that men during this period in their life cycle are more likely to have extra disposable income and greater control over the use of their time. Larger organizations are more likely to be formalized and to require membership dues. It may be that men in middle age have the time and resources to affiliate with an organization and the connections and position which make affiliation with a large and powerful one more likely.

Women are subject to very different life-cycle pressures. Since they are less likely to have a continuous career in a job which allows substantial advancement (Polachek 1975, 1976; Sewell, Hauser, and Wolfe 1980), their location in the institutional structure would not be expected to change dramatically with age. Instead, family events may be more important in determining their behavior in the voluntary sector. The presence of young children in the home may predispose them to join small, domestic-related groups; it may also limit their time and mobility. On the other hand, after children leave home, women may have fewer ties to the outside community (Fischer and Olicker 1980). This will make them less likely to be recruited for organizations and less likely to seek out membership in large, influential groups. Thus at no point in the typical woman's life course does her family or work situation predispose her toward membership in the voluntary organizations that are likely to be large—job-related and fraternal-sororal groups.

Figure 5 examines the relationship between organization size and education by sex. Since education is one of the strongest predictors of affiliation (McPherson 1981a), it comes as rather a surprise to find that for men there is no consistent relationship between education and organizational size. As the figure shows, however, there is a substantial relationship for women, with the average size for college graduates more than double that for women in the lowest education category. The results adjusted for differences in organizational type show that only part of the relationship is due to the differences in type at differing levels of education. In a more extended analysis (not presented owing to space limitations), we have adjusted the means of men and women in each category of education for the effects of all measured variables (type, age, work status, and marital status), with basically the same results as presented in figure 5. Thus, there is substantial evidence

that this procedure does not alter the grand mean sizes for men or women; an analysis of the sources of this overall mean difference is undertaken below.



* SEE TEXT FOR DETAILS OF ADJUSTMENT PROCEDURE.

FIG. 5.—Relationship between education and the size of voluntary organizations by sex.

that average organization size increases fairly systematically with education for women—but not for men.

Our interpretation of this result is that education is a more differentiating characteristic for women than for men. Because males are generally expected to participate in nonfamilial affairs (Haavio-Mannila 1967) regardless of their educational status, they are likely to be recruited for and to seek out organizations with a broader institutional base. However, highly educated women may experience qualitatively different sex-role expectations than less educated women. Highly educated women may have the interpersonal resources and cosmopolitan background that lead them to seek out more powerful, larger organizations. Thus, these women will be more likely to join the local affiliate of a nationally organized church-related group rather than a purely local church group (e.g., Council of Catholic Women rather than the local church circle); local affiliates of national organizations are generally larger than local groups (McPherson and O'Donnell 1980).

Another potentially important variable is the degree of active participation in the association. Larger groups find it more difficult to meet frequently because of logistic problems; they can also survive more easily without every member's participation because they are more likely to have formal structure which insures that crucial tasks are carried out. On the

other hand, activities that require frequent meetings and a high degree of member interaction will probably be carried out in small organizations that facilitate these behaviors (e.g., bridge clubs). If we find that men and women spend different amounts of time in their organizations, this difference in involvement may explain the sex differences in organizational size. Since we are interested in the potential contacts provided by organization membership, time spent in meetings and activity should be especially important.⁵ Table 1 shows the mean organizational size and involvement in voluntary associations by sex. As predicted, organizations that meet infrequently are much larger than those which meet at least one hour a month. However, the large difference in organizational size for the two sexes is not explained; men show roughly the same degree of involvement as women (86% for men and 91% for women). In fact, if we examine the mean number of hours meetings occupy per month, we see no difference between the sexes; men meet an average of 5.59 hours per month while women meet 5.56 hours. Therefore, involvement cannot explain the difference. In both categories of involvement, men's organizations are roughly three times as large as women's. We should note in passing that we did not find any relationship between size and marital status, nor was there any statistical interaction of marital status with sex.

Partitioning the Differences between Men and Women

In this section we address a basic question: Can we explain the size differences discussed above? Our strategy is to introduce controls for all impor-

TABLE 1
MEAN ORGANIZATION SIZE AND INVOLVEMENT IN
VOLUNTARY ASSOCIATIONS BY SEX

	Male	Female
Involved*.....	306.24 (86%)	98.98 (91%)
Not involved.....	720.63 (14%)	264.70 (9%)
Total mean.....	363.77 (100%)	113.43 (100%)

NOTE.—Description of data in Johnson (1977).

* Spend at least one hour per month involved in organization activities.

⁵ One might argue that only memberships that entail direct participation are relevant for a study of potential contacts. We think that even organizations that seldom meet will increase the probability that two members will communicate, since their formal structures (e.g., membership directories or newsletters) often facilitate contact or information flow. Also membership in such organizations can provide a basis for the establishment of contact with other members. We did parallel analyses only on memberships with one hour or more of involvement per month, with results almost identical with those described in tables 2 and 3. This is not surprising, since nearly 90% of the organization memberships entailed one hour or more of involvement per month.

tant variables, so that we can determine whether, by statistically equalizing men and women on these other characteristics, we would equalize the sizes of their organizations. Specifically, we begin with the observed mean sizes of the organizations for men and women; we then adjust these means statistically for the fact that men and women are distributed differently among occupations. Next, we add age to the equation, and so on.⁶ The results of this analysis are presented in table 2.

The first three columns of table 2 show the analysis, using the sizes of organizations reported in the sample of memberships. We will discuss these results first, before turning to the results from the individual sample in columns 4 through 9. The statistical equalization of men and women with respect to the types of jobs they hold results in an increase in the size difference in their organizations. This increase is produced by the tremendous organization size differences by sex for salespersons, craftsmen, and laborers. The fact that, in these categories, men actually occupy very different positions from women has implications for their voluntary affiliations. For example, men in sales often depend on interpersonal contacts to create business opportunities. Therefore, they would be expected to hold memberships in large voluntary organizations (e.g., Kiwanis Club or chamber of commerce) as a means of promoting these contacts. On the other hand, women in sales are often in retail stores where they do not earn commissions and seek out customers. Therefore, equalizing the proportion of salesmen, craftsmen, laborers, managers, and so on by sex does not reduce the organization size differential but increases it.

Controlling for age also causes a slight increase in the sex-related difference in size, because the difference is relatively small among the young (under 45) and very old (over 65), and because women are overrepresented in both these categories.

All the other variables decrease the difference in size. Organizational type has the strongest effect: statistically equalizing men and women with respect to type of organization produces a 60-unit decrease in the initial difference of around 250, a reduction of approximately 23%. In particular, men's memberships are more likely to be in business-related, labor, and veterans' organizations, which tend to be large. Women's memberships are more likely to be in social, church, or community groups.

Row 5 of the table adjusts for differences in work force status. This ad-

⁶ For this analysis, the men and women are pooled, and a dummy variable representing sex is introduced. The coefficient for this dummy variable is the difference in organization size by sex. The estimates from this equation are transformed into the size estimates which appear in table 2. The addition of subsequent variables adjusts these estimates for the effects of the variables added to the equation. This procedure is similar to a stepwise regression analysis, with all dummy variables for a single conceptual variable (e.g., type, marital status, etc.) added to the equation at the same time.

TABLE 2
ORGANIZATION SIZE DIFFERENCES BETWEEN MEN AND WOMEN

CONTROL VARIABLES*	SIZE (Membership Sample)			AVERAGE SIZE (Individual)			FIRST MENTIONED ORGANIZATION		
	Men (1)	Women (2)	Difference (3)	Men (4)	Women (5)	Difference (6)	Men (7)	Women (8)	Difference (9)
Unadjusted.....	366.8	116.6	250.2	342.2	118.1	224.1	253.4	117.0	136.4
Occupation.....	371.9	114.0	257.9	358.1	111.0	247.1	280.0	102.8	177.2
Age.....	374.3	112.6	261.7	365.8	106.9	258.9	279.5	103.1	176.4
Type.....	335.3	133.4	201.9	†	243.4	122.4	121.0
Work status.....	322.3	140.4	181.9	331.2	125.4	205.8	227.7	130.8	96.9
Involvement.....	320.8	141.2	179.6	†	225.9	131.8	94.1
Education.....	319.5	141.9	177.6	326.9	127.7	199.2	225.4	132.0	93.4
Marital status.....	318.8	142.3	176.5	323.4	129.6	193.8	223.8	132.9	90.9

NOTE.—Description of data in Johnson (1977).
 * Controls are entered stepwise from top; means are adjusted for the effect of the variable in left heading and all others above the row.
 † This variable cannot be controlled since it refers to only one organization.

justment reduces the observed difference to around 130 units, bringing the total reduction to nearly 50%.

The last three rows of table 2 show that the addition of involvement, education, and marital status to the equation does not alter the difference substantially. In fact, after taking all the control variables into account, we are left with the firm conclusion that women report organizations less than 50% the size of men's.

Columns 4-9 of table 2 show that our results and conclusions do not depend on our analysis of memberships rather than individuals. Columns 4 through 6 show a parallel analysis, using as the dependent variable the arithmetic average of the sizes of all organizations to which each individual belongs. Note that the pattern of results is the same: controlling occupation and age increases the difference slightly, while all the other variables tend to decrease it. The remaining difference is larger when we analyze average size, because we cannot control for organizational type or involvement with this variable. The fact that the first mentioned organizations (columns 7 through 9) are somewhat smaller is simply an artifact of the aided recall techniques used; church-related groups were first on the list. Several other forms of the size variable were analyzed, including each ordinal position of reported memberships (e.g., second, third, etc.). All analyses supported the results we have presented.

DISCUSSION

There is one striking finding which emerges from this analysis: women belong to smaller organizations than men. Controlling for organizational type and a number of sociodemographic characteristics accounts for only about half the difference in the reported sizes of men's and women's voluntary organizations. We will now attempt to account theoretically for this unexplained difference.

There are two basic reasons why women might belong to smaller organizations than men: (1) selective recruitment by the organization, and (2) selective joining by the individuals involved. Selective recruitment by sex may occur because of properties of the organizations (e.g., men's clubs), or because the organization selects for some characteristic which is rare among women (e.g., veterans' organizations). Such selectivity is likely to operate in organizations of high prestige or visibility, making them nominally universalistic but operationally sex segregated. Of course, women's organizations may discriminate in precisely the same ways. Sewing clubs select for an interest that is rare among men. Sororal clubs explicitly exclude men. However, it is likely that general interest groups or instrumentally oriented associations will find men to be more attractive recruits

to their membership rolls since men are likely to have higher status, income, and mobility than women. Therefore, we would expect men to be selected more often for membership in large organizations.

This hypothesis will be more persuasive if we consider the conditions under which large organization size is likely to occur. We expect larger organizations when one or more of the following conditions are met: (1) there are many potential members available to the organization; (2) there are strong economic or political reasons for the organization to be large; (3) a very high percentage of the potential members for the organization actually are members of the organization; and (4) the individuals constituting the membership base do not have heavy restrictions on their time or mobility. For instance, labor unions fit all these conditions: the potential membership is limited only by the size of the occupational category; in bargaining situations, the advantage of large unions is obvious; some industries require union membership as a prerequisite for getting a job; and the restrictions imposed by the primary activities of the potential members (i.e., working) do not interfere with participation in the union. A similar argument can be made for most organizations which are closely tied to the economic institution, such as professional organizations.

Women are less likely to be recruited into such organizations because: (1) they are less likely to be in positions where they qualify for membership (e.g., working in jobs that require employees to be unionized); (2) they are less likely to have characteristics that might be valuable to the organization (e.g., high income or political contacts); and (3) they may have restrictions on time or mobility since they are likely to have the primary responsibility for child care or household maintenance or both (Duncan and Duncan 1978).

On the other hand, those organizations which are likely to recruit women actively are unlikely to meet the preconditions for large size. For instance, parent-teacher organizations will have a potential membership limited by the size of the school involved, will have little incentive to attain large size, will probably not be joined by all parents, and will possess a membership base comprised of precisely the individuals who are most pressed for time: young parents, usually young mothers. Thus, we see that large organizations may select against women because of women's location in traditionally defined roles, with the attendant restrictions on time and mobility.

Selective joining may operate in much the same way as selective recruitment. Women who occupy traditionally defined roles may select organizations that will value the skills developed by those positions in the social structure. They may also decide to join organizations made up primarily of other women occupying similar positions; these associations will be most likely to provide information to aid them in their activities and to schedule

organizational affairs in ways that will accommodate the time and mobility problems that these women face (e.g., coordinating family, work, and voluntary association activities). Probably both selective joining and selective recruitment contribute to our empirical result: men belong to larger organizations than women.

There is a clear pattern of women's being segregated into smaller, structurally peripheral organizations. The image of the voluntary sector which emerges from our analysis is a system of organizations divided into large, male-dominated, economically oriented organizations and small, female-dominated, domestically oriented organizations. It is clear, even in the absence of a great deal of direct information, that there is very substantial sex segregation in the voluntary sector.

Since only 10% of women's memberships are in organizations of over 200 members, and fully 30% of men's memberships are in such organizations, we must conclude that there are a large number of small, predominantly female organizations and a smaller number of large, predominantly male organizations. Let us elaborate this point. In these data, men and women possess almost exactly the same number of memberships (see the third row from the bottom in table 3). This fact implies that the total number of male memberships in the system is roughly equal to the total number of female memberships, given a roughly equal distribution of men and women in the population (see n. 3). Now, our results show that the

TABLE 3
N MEMBERSHIPS, SIZE OF ORGANIZATIONS, AND POTENTIAL
CONTACT VOLUMES OF MEN AND WOMEN

Organization	Men	Women	Ratio, Men to Women
Job related:			
Mean <i>N</i>28	.11	
Mean size.....	603.5	309.7	
Contact volume*.....	168.9	34.1	(4.95:1)
Nondomestic:			
Mean <i>N</i>87	.47	
Mean size.....	431.3	145.5	
Contact volume.....	375.2	68.4	(5.49:1)
Domestic:			
Mean <i>N</i>53	1.02	
Mean size.....	74.0	57.8	
Contact volume.....	39.3	59.0	(.67:1)
Total:			
Mean <i>N</i>	1.69	1.61	
Mean size.....	363.8	113.1	
Contact volume.....	614.8	182.1	(3.38:1)

NOTE.—Descriptions of data in Johnson (1977). See text for organizations constituting each category.

* Contact volume is the mean number of comembers per individual; it is the product of the mean number of memberships and the mean size.

organizations to which men belong are roughly three times the size of women's. Therefore, there must be substantially fewer large organizations to absorb the given number of men's memberships than smaller organizations to absorb the same number of women's memberships (McPherson 1978). Thus, we see a clear core-periphery pattern emerging, with men located in large economically oriented organizations in the core and women in smaller, domestically oriented organizations on the periphery.

Since one very important function of voluntary organizations is to provide a set of potential contacts for an individual, we will distinguish among types of organizations likely to provide different types of contacts. We place organizations into three classes: (1) job-related, consisting of union, professional, and business organizations; (2) nondomestic, including fraternal, sports, veterans, political, hobby, and service and civic organizations; (3) domestic, including youth, church-related, social, charitable, neighborhood, and community organizations.

Table 3 presents information on the mean number of memberships, average size, and potential contact volume of men and women in our sample. Although men and women have almost exactly the same mean number of memberships, the great majority of women's memberships occur in domestic-related organizations, while the majority of men's occur in nondomestic ones. Women belong to approximately twice as many domestic organizations as men (1.02 to .53), while men belong to more than twice as many job-related organizations as women (.28 to .11). Thus, while the overall rate of affiliation is virtually identical for men and women, the rates by type of affiliation are dramatically different.

These sex differences in types in combination with the size differences discussed earlier produce striking differences in the volume of potential contacts. Overall, men are exposed to over 600 potential contacts through their average 1.69 memberships, while women are exposed to fewer than 185 such contacts through their 1.61 memberships. Even more crucial is the distribution of those contacts by type. Men are exposed to nearly 170 job-related potential contacts, while women have fewer than 35—a ratio of nearly five to one. A similar disparity occurs in the nondomestic associations, where men average over 375 potential contacts, while women average fewer than 70. Thus, there is a tremendous difference by sex between the potential contact volumes.

If we take the literature on social networks seriously, we must conclude that men are located in positions in the voluntary network which are much more likely to provide access to information about possible jobs, business opportunities, and chances for professional advancement. Women, in contrast, are located in positions more likely to expose them to information about the domestic realm.

SUMMARY AND CONCLUSIONS

We have shown that there is a dramatic difference in the typical size of organizations belonged to by men and women. It is most pronounced in organizations which are economically oriented. The differences between men and women are remarkably consistent across social categories; men tend to belong to larger organizations regardless of labor force status, age, education, marital status, or involvement. Finally, we find that when we statistically equalize men and women on all these variables, a substantial sex difference in organization size remains. We believe that this large difference produces a dramatic difference in the social resources available to women and men in the voluntary sector, even though the gross rates of affiliation are quite similar.

Much remains to be done. It would be very useful to determine the actual composition of the organizations which appear in studies of affiliation and the density of the weak ties in the organizations themselves. We suspect that larger organizations will have lower densities, unless they have explicit mechanisms to facilitate contacts. Another major lacuna in the literature on voluntary organizations which is important from the social network perspective concerns the mechanisms for recruitment to organizations. The limited literature suggests that friendships are primary sources for recruitment, but it is clear that structural features such as labor force participation and geographic location could be critical. In general, then, we need much more detailed information on the social networks in the voluntary sector before we can establish the extent to which the size differences we have found have an impact on the sex differences in income attainment (Rosenfeld 1980), labor force reentry (U.S. Department of Labor 1973), and the overall status attainment process.

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The Occupational and Earnings Returns to Education among Black Men in the North¹

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This paper tests the hypothesis that the lower occupational and earnings returns to each year of education among southern-born black men residing in the North characterize only those men who were educated in the South and migrated to the North during their later teens or as adults. Data from the 1973 Wisconsin Occupational Changes in a Generation Survey provide background and job information for three groups of black men: (1) northern-born men growing up in the North; (2) southern-born men who migrated to the North as children and were educated in the North; and (3) southern-born men who were educated in the South and migrated to the North during their later teens or as adults. Earnings and occupational-status structural equation models are estimated for each group of black men. The analyses reveal that the earnings and status-attainment processes are more meritocratic among men who grew up and were educated in the North. Restricting attention to young black men, there are no differences among the groups in occupational returns to education, but substantial differences in earnings returns to education are observed.

Residential migration has served as the adjustment mechanism by which the supply of labor in an area will approximately match the demand for labor, and changes therein, over time. For population subgroups excluded from the higher-prestige and better-paying jobs of the primary labor market, geographic mobility has allowed a person to relocate into a labor market in which he will not be a victim of discrimination or into an area that has a better supply of secondary labor market jobs. Historically, interstate migration from the American South to the North and West has been a major method by which blacks have escaped from the poor labor-

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market conditions of the South and relocated into labor markets with better opportunities for occupational and earnings attainments (Myrdal 1944; Banton 1967; van den Berghe 1967; Eldridge and Thomas 1964).

Migration is one mechanism removing ascriptive restrictions on achievement associated with community of origin by enabling a person to take advantage of opportunities available in another community (Blau and Duncan 1967). By the act of migration, a man reduces the importance of family background characteristics for his social and economic attainments and creates a discontinuity in his career history. Men who are interregional migrants experience more intergenerational and intragenerational occupational mobility (both upward and downward) than other men. Occupational attainments are more closely associated with level of education, and less closely associated with father's occupation or with previous occupation. The earnings of migrants also are less determined by background characteristics than are the earnings of nonmigrants (Blau and Duncan 1967; Featherman and Hauser 1978; Fredrickson and Hogan 1980). Migration diminishes the importance of job history for later attainments. Conversely, level of education is of enhanced significance for the occupational and earnings attainments of migrants (Fredrickson and Hogan 1980). In other words, when a man who is not a migrant presents himself to an employer for a job, that employer assesses the quality of the prospective employee with reference to his education and his previous job history. The same employer will assess the job qualifications of a migrant with greater attention to his education and less attention to his previous job history. This is an economically rational procedure to the extent that the educational level completed by a man provides a standard scale against which to measure the skills he brings to the labor market. Often a northern employer will not be familiar with the firms with which a migrant from the South has been employed. In this context, the education of the job applicant would assume increased importance and his previous work history would be of lesser importance.

Black migrants from the South are selected positively with regard to their years of schooling, but they tend to have fewer years of schooling than black men born in the North (Hogan and Featherman 1977). By migrating to the North, the southern-born black man enters a more favorable labor market, with higher average wages and jobs of higher status, but he does this at the cost of moving from a position of relative educational superiority to a position of relative educational inferiority. The migrant to the North is further disadvantaged in that he receives a lower rate of occupational-status and earnings returns to each year of schooling than does a black man native to the North. In fact, the migrant experiences about the same rate of occupational and earnings returns to a year of schooling as he would have received if he had remained in the South.

The migrant benefits from his move only because of the higher wage rate prevalent in the North (Hogan and Featherman 1977).

Previous research has contrasted the occupational and earnings experience of first-generation northerners with those of men born in the North (Crain and Weisman 1972; Weiss and Williamson 1972, 1975; Long and Heltman 1975; Lieberman and Wilkinson 1976; Hogan and Featherman 1978). First-generation northerners are a heterogeneous group. The category includes persons born and raised in the South who migrated to the North as adults, as well as persons born in the South who migrated to the North as children.

There are a variety of reasons to expect differences between the attainment processes of child and adult migrants to the North. The adult migrants to the North attended school in the South, whereas the child migrants to the North received at least the later part of their schooling in the North. Persons growing up in the South complete fewer years of schooling than men growing up in the North. They also suffer in comparison with their northern counterparts insofar as each year of schooling in the South has historically involved fewer and shorter days in the classroom (Duncan 1968).

Furthermore, black men in the South, attending primarily black schools, spent fewer and shorter days in the classrooms for each year of schooling than did southern whites (Welch 1973). Historically, southern blacks were more disadvantaged relative to southern whites than were northern blacks relative to northern whites (Duncan 1968; Welch 1973). Employers may perceive southern schooling as inferior, and discount such schooling in decisions about the occupation and earnings of men educated in the South. If this is the case, entry-level occupation would not depend on years of schooling among the southern-educated men, but would be an important factor in the initial occupation of northern-educated men.²

Adult migrants to the North interrupt their life course by migrating. If the interregional move occurs immediately after schooling, the transition from school to work is disrupted. The migrant would be unable to take advantage of any formal or informal contacts between schools and employers. As a result, the entry-level occupation would not be commensurate with the migrant's schooling.

² The inferiority of black schools relative to white schools has decreased over the last several decades (Welch 1973). Welch (1973) has hypothesized that the intercohort improvements in black school quality account for the observed intercohort increases in relative earnings returns to schooling among black men. Welch does not distinguish black men by their region of residence or region of schooling. Hogan and Featherman (1977) have shown that the intercohort increases in occupational and earnings returns are most pronounced among men native to the North. In this paper, we adopt the generational argument of Hogan and Featherman (1977) and elaborate on it by distinguishing region of schooling. We build on Welch's work by examining returns to schooling within a relatively narrow age group, controlling for the age-region-education interaction Welch (1973, n. 9) suggests exists.

surate with educational qualifications. As with the school-quality explanation, this hypothesis about the disruptive effects of migration on the transition from school to work implies that the status of entry-level occupation will be less dependent on education among adult migrants.

A third reason to expect differences between the attainment processes of child and adult migrants is that by migrating an adult member of the labor force interrupts the orderly progression of his career. Under this hypothesis, the entry-level occupational returns to men who later become migrants are similar to those of men who began their careers in the North. However, the migrant breaks his career trajectory, so that initial job status has relatively little impact on later job status. The reduced effect of first-job status on later-job status reduces the indirect impact of education (through first job) on later occupational attainments; the total occupational returns to education consequently are reduced.

The primary objective of this paper is to test the hypothesis that the lower occupational and earnings returns to education which previous research has reported for migrants to the North characterize only those black men who were raised and educated in the South. Men born in the South, but raised and educated in the North, are hypothesized to have processes of occupational and earnings attainments identical to those of Northern natives. The analysis supports this hypothesis when all men aged 20-65 are considered. If we restrict our attention to men aged 20-39, there are no differences among the groups in occupational returns to education, but substantial differences in earnings returns to education are observed. The paper then reports exploratory tests that will suggest which of the three alternative hypotheses best accounts for the differing careers of the adult migrants.

DATA AND METHODS

This analysis requires the comparisons of three groups of black men residing in the North: (1) northern-born men who were raised and educated in the North; (2) southern-born men who moved to the North as children, and who were raised and educated in the North; and (3) southern-born men who were raised and educated in the South, and who migrated to the North as adults.

The data used in this study are drawn from the 1973 Wisconsin Occupational Changes in a Generation (OCG) Survey. In this survey, 640 randomly selected black men aged 20-65 from the Milwaukee metropolitan area were interviewed, for a completion rate of about 75%. The Wisconsin survey is a replicate of the March 1973 U.S. Current Population Survey and the Occupational Changes in a Generation II Survey (see Featherman and Hauser 1978). The national data were used by Hogan and

Featherman (1977), but could not be used for this study because they include no information about region of residence while attending school. The Wisconsin survey included a question ascertaining the address of the head of household with whom the respondent was living in the census year closest to the respondent's sixteenth birthday. The response to this question is used to determine whether the respondent grew up and received his schooling in the South or in the North.

There are obviously some problems with this definition. A person may be as young as 12 or as old as 21 in the census year closest to his sixteenth birthday. A person resident in the South at age 12 may have received the final part of his education in the North. A person residing in the North at age 21 may have received all of his schooling in the South. Unfortunately, we could find no comparable dataset with a sufficient number of northern resident blacks which would provide a better variable for measuring the region in which a man completed his education.³ We believe that the Wisconsin OCG Survey allows us to classify most men correctly by region of residence while growing up and attending school. To the extent that persons are misclassified randomly, any differences observed between southern-born men educated in the North and southern-born men educated in the South will be attenuated, obscuring the hypothesized results. Besides offering the advantages of being a fairly large sample of black men residing in the North, replicating the national OCG study, and providing a variable that permits us to classify men as to region of schooling, the limitation of the sample to a single metropolitan area provides a control for intraregional, intermetropolitan labor market differences.

The analysis will proceed by estimating the same set of structural equations for occupational and earnings attainments as estimated by Hogan and Featherman (1977). The equations include five characteristics of family background: (1) the occupational status (Duncan SEI scale) of the father or head of household when the respondent was growing up; (2) the years of education of the father or head of household; (3) farm background (0 = nonfarm; 1 = farm); (4) number of brothers and sisters not

³ The National Longitudinal Survey does not include the necessary region of residence data. The 1970 Census Public Use Sample tapes permit the identification of a sample of young black men, residing in the North, who resided in the South in 1965, but the census does not include family background information about the respondents. Although family background characteristics have no direct effects, net of education, on career attainments, their total effects are statistically significant. In order to identify the net career returns to education, therefore, it is necessary to control for family background characteristics whenever the socioeconomic origins differ among the comparison groups. In this analysis, the positive selectivity of migrants from the South, and the inferior social background of those migrants compared to men who are northern natives, necessitates the inclusion of controls for family background characteristics in the analysis of career achievements. This prevents the utilization of a dataset that does not include family background information.

counting the respondent; and (5) whether the respondent's parents were living together most of the time while he was growing up (0 = intact family; 1 = broken home). The respondent's own education is coded according to years of schooling completed. A control is included in the earnings-attainment model for years of work experience, and a quadratic term is included to estimate decay in the rate of return to work experience (see Mincer 1974).

FAMILY BACKGROUND

Northern-born black men have the most favorable socioeconomic backgrounds, whereas southern-born men who were adult migrants to the North have the most disadvantaged socioeconomic backgrounds (table 1). Since migrants from the South are positively selected with regard to socioeconomic characteristics, the southern-born men whose parents migrated north during their childhood are expected to have more favorable socioeconomic backgrounds than southern-born men whose parents did not migrate north during their childhood. The most remarkable difference is that 42% of the southern-born adult migrants to the North had farm backgrounds, compared with fewer than 7% of the other men.

Men whose parents migrated north during their childhood are more likely to report growing up in a broken home, and have fewer siblings. This difference is likely to be attributable to the disruptive effects of inter-regional migration on marital stability. It could also result from unmarried women with children migrating to join other family members residing in the North.

The men born and educated in the South completed only 10.1 years of schooling on average compared with 11.5 years among the northern natives and 12.0 years among the southern-born but northern-educated men (table 1). Occupational status in 1973 follows the same rank order, with southern-educated men attaining 23.3 status points, compared with 32.4 status points for men migrating to the North during their childhood. Despite their poorer educations and lower occupational attainments, the men who were born and educated in the South attain the highest earnings. In part, this is accomplished by working more weeks per year and by offering the employer a substantially greater number of years of work experience.

OCCUPATIONAL ATTAINMENT

Net of the effects of years of schooling, family background factors have few direct effects on occupational-status attainments for any of the residence groups (table 2). As hypothesized, the occupational-status returns to a year of schooling are much higher among men educated in the North.

TABLE 1
MEANS AND STANDARD DEVIATIONS OF FAMILY BACKGROUND AND SOCIOECONOMIC
ATTAINMENT VARIABLES BY RESIDENCE GROUP, BLACK MEN AGED 20-65,
EXPERIENCED CIVILIAN LABOR FORCE, MILWAUKEE, MARCH 1973

VARIABLES	RESIDENCE GROUP					
	Northern Born (N = 101)		Southern-born Child Migrants (N = 100)		Southern-born Adult Migrants (N = 299)	
	\bar{X}	SD	\bar{X}	SD	\bar{X}	SD
Father's occupation.....	19.04	13.42	16.87	10.84	14.36	11.69
Father's education.....	8.88	3.44	8.43	3.30	6.14	3.38
Farm background.....	.03	.18	.07	.25	.42	.50
Broken family.....	.33	.47	.41	.49	.31	.46
Number of siblings.....	6.06	3.70	5.28	3.46	7.07	4.28
Education.....	11.47	2.00	11.95	2.03	10.08	2.87
Occupational status.....	27.88	18.51	32.40	21.37	23.27	16.86
Earnings (\$) 6,422	6,422	4,456	7,538	4,568	8,649	3,908
Weeks worked.....	39.65	16.86	40.60	17.19	45.57	12.62
Experience.....	11.48	10.38	10.92	10.23	19.63	11.15
Decay.....	238.30	365.05	222.84	408.35	509.25	502.60

TABLE 2
REGRESSION ANALYSIS OF OCCUPATIONAL ATTAINMENT BY RESIDENCE GROUP, BLACK MEN
AGED 20-65, EXPERIENCED CIVILIAN LABOR FORCE, MILWAUKEE, MARCH 1973

INDEPENDENT VARIABLES	RESIDENCE GROUP					
	Northern Born			Southern-born		
	b	SE(b)		Child Migrants	Adult Migrants	SE(b)
Father's occupation.....	-.04	.15		-.15	.07	.10
Father's education.....	-.81	.63		.42	.16	.37
Farm background.....	-6.66	11.06		.79	-2.02	2.32
Broken family.....	11.11*	4.37		8.53	-.85	2.47
Number of siblings.....	-.19	.58		.20	-.14	.28
Education.....	5.40**	1.08		6.27**	1.83**	.42
Intercept.....	-28.26	...		-48.22	4.97	...
Adjusted R ²270291	.104	...
Error of estimate.....	15.81	...		18.00	15.95	...

NOTE.—An F-test for different regression slopes among the groups is statistically significant ($P < .01$).

* $P < .05$.

** $P < .01$.

Each additional year of schooling produces a 5.4-point occupational-status increase among northern-born men, and a 6.3-point increase among southern-born men educated in the North. This compares with the very small rate of occupational-status return of 1.8 points for each additional year of schooling among men educated in the South.⁴ Because of the greater importance of education on occupational attainments among northern-educated men, the levels of explained variance in the structural equation models estimated for northern-educated men are much higher. The process of occupational attainment among black men residing in Milwaukee is thus more meritocratic among those men educated in the North.

EARNINGS ATTAINMENT

A black man's family background has little direct effect on his earnings attainments, controlling for his education, occupation, weeks worked, and labor-force experience (table 3). Each of these achieved personal characteristics, on the other hand, has a direct effect on earnings attainments. As we hypothesized, the earnings returns to an additional year of schooling are much higher among men educated in the North. An additional year of schooling produces more than \$930 in additional earnings for men educated in the North, regardless of their place of birth, compared with a statistically insignificant return of \$151 for southern-born men educated in the South.⁵

The earnings returns to an additional point of occupational status are positive among the southern-educated men, negative among the native

⁴ We also estimated these structural equation models of the occupational attainment process with the inclusion of age and age-squared terms, and with a spline function specification of the education variable. In these models, the occupational returns to an additional year of graded schooling were 2.38 among the northern natives, 2.49 among child migrants to the North, and 0.77 among the adult migrants to the North. The occupational returns to each additional year of college are 10.02, 9.51, and 7.42, respectively. The results of the analysis thus remain the same when controls are included for age and a spline specification of education is used—the occupational status returns to an individual year of graded schooling or college are lower among men who grew up and were educated in the South.

⁵ These structural equation models of the earnings-attainment process were also estimated with a spline function specification of the education variable (see Featherman and Hauser [1978] for a discussion of spline functions). In these models, the earnings returns to an additional year of graded schooling were \$1,141 among the northern natives, \$1,051 among child migrants to the North, and \$121 among the adult migrants to the North. The earnings returns to each additional year of college are \$478, \$894, and \$374, respectively. The earnings returns to an additional year of college are in the correct direction but do not differ in a statistically significant way among the groups; the lower rate of earnings returns to an additional year of graded schooling is significant. These results are consistent with the hypothesis that the earnings returns to an additional year of schooling are lower among men who grew up and were educated in the South.

TABLE 3
REGRESSION ANALYSIS OF EARNINGS ATTAINMENT BY RESIDENCE GROUP, BLACK MEN
AGED 20-65, EXPERIENCED CIVILIAN LABOR FORCE, MILWAUKEE, MARCH 1973

INDEPENDENT VARIABLES ^a	RESIDENCE GROUP					
	Northern Born		Southern-born Child Migrants		Southern-born Adult Migrants	
	b ^b	SE(b)	b	SE(b)	b	SE(b)
Father's occupation....	6	26	11	33	12	20
Father's education.....	-175	114	-193	120	-69	74
Farm background.....	-770	1,859	-1,079	1,479	-7	471
Broken family.....	1,060	765	1,440	729	285	511
Number of siblings.....	244*	99	8	103	22	55
Education.....	934**	221	947**	243	151	111
Occupational status.....	-57*	22	7	19	37*	14
Weeks worked.....	138**	21	167**	21	157**	18
Experience.....	656**	122	296**	108	238**	78
Decay.....	-16**	3	-4	3	-4*	2
Intercept.....	-12,330	...	-12,240	...	-3,383	...
Adjusted R ²654618330	...
Error of estimate.....	2,615	...	2,824	...	3,197	...

^a An F-test for different regression slopes among the groups is statistically significant ($P < .01$).

^b Metric coefficients shown are in 1-dollar units.

* $P < .05$.

** $P < .01$.

northerners, and nil among the southern-born men educated in the North. At first this seems confusing, but we believe these findings reflect the true behavior of these groups. In other research, we have shown that black men who are northern natives place a greater emphasis on occupational status than on earnings attainments. They have a strong tendency to sacrifice earnings rewards in order to attain higher-status jobs. Southern-born men, on the other hand, emphasize earnings attainments in the achievement process. They display little tendency to achieve higher occupational status at the cost of sacrificing earnings attainments (Hogan and Pazul 1981).

The wage return to each additional week of work is about the same for the three residence groups. Since they share the same labor market, this comes as no surprise. The northern-born men enjoy a quicker rate of earnings growth during their early career, compared with southern-born men. The growth in earnings with additional years of job experience peaks earlier for the northern natives, however. The southern-born men continue to enjoy increased earnings returns each year until a later point in their lifetimes.

The earnings-attainment process, like the occupational attainment process, thus is more meritocratic among men educated in the North. This is reflected in levels of variances explained, adjusted for degrees of freedom, of more than 60% among the men educated in the North compared to 33% among the men educated in the South. The higher earnings returns accruing to a year of education among men educated in the North are the primary reason for this difference.

THE PROCESS OF ATTAINMENT AMONG YOUNG WORKERS

There have been intercohort increases in the effects of education on the occupational and earnings attainments of young black men (Featherman and Hauser 1978). These changes have been especially pronounced among men who are residing in the North (Hogan and Featherman 1977). The adult migrants to the North have a mean age of 39.0 years, compared with 30.4 years among the northern natives and 31.2 years among the child migrants. The intercohort increases in the effects of education on career attainments may account for the higher rates of return to education among the (younger) men who grew up in the North. Such an age-education interaction would not be adequately specified by regression models incorporating additive controls for age or work experience.

We therefore reestimated the structural equation models of the occupational and earnings attainment process (reported in tables 2 and 3) for black men aged 20-39 and in the experienced civilian labor force. As noted above, the differing social backgrounds of the residence groups make it

necessary to include controls for family background in these models. The additional age restriction further limits the number of sample cases to 330—80 northern natives, 83 child migrants to the North, and 167 adult migrants to the North.

The net occupational and earnings returns to an additional year of education are shown in table 4. Table 4 also shows the net occupational and earnings returns to an additional year of graded schooling and college that are obtained when a spline function for education is utilized. The occupational returns to schooling do not differ significantly among the residence groups, although the relative magnitudes of the coefficients tend to be in the appropriate order. The results using the spline specification of education are equivocal, and do not differ significantly among groups.

The earnings returns to education differ significantly among the groups, and in the expected direction. Each additional year of schooling increases earnings by more than \$1,000 among men raised in the North, but by less than \$300 among men who were raised in the South. The lower rate of earnings returns to additional years of schooling received by the adult migrants appears to operate at both the graded schooling and college level.

INTERPRETATIONS AND CONCLUSIONS

Previous research has noted that the rate of occupational and earnings returns to education among southern-born men residing in the North is lower than that among northern natives. This analysis has demonstrated that the lower rate of career returns to education among migrants to the North characterizes only those men who migrate as adults; the occupational and earnings-achievement processes of men who migrated to the North as children closely resemble those of men who are natives of the North.

Clearly, whatever factors cause migrants to have lower occupational and earnings returns to education than nonmigrants must apply to adult migrants but not to child migrants. In the introduction to this paper, we suggested three possible explanations for any observed differences in the stratification process. An exact test of these hypotheses would require life history residential, occupational, and earnings-attainment data, as well as data on the quality of education received. We do not have these data.⁶ A few speculations are possible, however, based on the data that are available.

It appears that the lower rate of occupational returns to education char-

⁶ An anonymous *AJS* reviewer suggested that measures of school quality could be obtained by matching state-level aggregate data on school quality in each year to the individual records of the sample respondents. While beyond the scope of the research reported here, this is a promising avenue for future research.

TABLE 4

REGRESSION ANALYSIS OF OCCUPATIONAL AND EARNINGS RETURNS TO EDUCATION AND
FIRST JOB BY RESIDENCE GROUP, BLACK MEN AGED 20-39, EXPERIENCED
CIVILIAN LABOR FORCE, MILWAUKEE, MARCH 1973

MODELS AND VARIABLES	RESIDENCE GROUP					
	Northern Born		Southern-born Child Migrants		Southern-born Adult Migrants	
	b	SE(b)	b	SE(b)	b	SE(b)
Occupational attainments: ^a						
Model 1: ^b						
Education.....	4.95**	1.48	7.02**	1.55	3.62**	.70
Model 2: ^c						
Graded schooling.....	1.63	1.81	2.94	2.98	1.39	.79
College.....	21.27**	5.85	8.98**	2.16	9.80**	1.44
Model 3: ^d						
Graded schooling.....	1.75	1.55	2.87	2.65	1.29	.78
College.....	13.17**	5.29	3.32	2.40	7.40**	2.04
First job.....	.55**	.13	.49**	.12	.25	.15
Earnings attainments: ^e						
Model 1: ^f						
Education.....	1,276**	243	1,070**	297	290	178
Model 2: ^g						
Graded schooling.....	1,341**	288	979*	468	338	199
College.....	988	981	1,096**	384	134	391

* Metric regression coefficients shown are net of the effects of the independent variables included in the regression analysis reported in table 2.

^b An *F*-test for different regression slopes among the residence groups is statistically insignificant ($P > .05$).

^c An *F*-test for different regression slopes among the residence groups is statistically insignificant ($P > .05$).

^d An *F*-test for different regression slopes among the residence groups is statistically insignificant ($P > .05$).

* Metric regression coefficients shown are in 1-dollar units and are net of the effects of the independent variables included in the regression analysis of table 3.

^f An *F*-test for different regression slopes among the residence groups is statistically significant ($P < .01$).

^g An *F*-test for different regression slopes among the residence groups is statistically significant ($P < .01$).

* $P < .05$.

** $P < .01$.

acteristic of the adult migrants is an artifact of the older average ages of the migrant men and the recent intercohort increases in occupational returns to education among black men. Since differing occupational-status returns to education are not found among men under 40 years of age, none of the three explanations advanced to explain anticipated differences between adult migrants and other men in the process of attainment apply in the case of occupational achievements.

In contrast, adult migrants have lower rates of earnings returns to each additional year of graded schooling and college than other men, if we restrict our attention to blacks under 40 years of age. Apparently, one or more of the three explanations advanced to explain anticipated differences between adult migrants and other men in the process of attainment do apply in the case of earnings attainments.

The mean age at first job of young black men in the experienced civilian labor force was 19.0 among the northern natives, 20.2 among the child migrants, and 19.3 among the adult migrants. The difference between age at school completion and age at first job is 1.0 among the northern natives, 1.5 among the child migrants, and 1.7 among the adult migrants. It does not appear that the transition into first, full-time job following the completion of schooling is unduly delayed among the adult migrants. We have no data on early career earnings, but the first-job status returns to graded schooling and college do not differ among the residence groups. These indirect sorts of evidence cause us to doubt that an interruption in the transition from school to work due to interregional migration is the cause of the lower returns to education among the adult migrants.

It is more difficult to discount either of the other hypotheses about the lower earnings returns to education received by adult migrants. In order to determine whether adult migration disrupts career trajectory, we regressed current occupational status on first-job status, years of graded schooling and college, and family background characteristics. The net regression coefficient of current occupational status on first job was .55 among northern natives, .49 among the child migrants, and .25 among the adult migrants (table 4). The career trajectory thus displays a much greater degree of continuity among the northern natives and child migrants, in support of this hypothesis. Although the total effects of graded schooling and college on occupational attainments do not vary, the direct effects of education, controlling for first-job status, are significant only for college education, and only among the northern natives and the adult migrants to the North. The research of Fredrickson and Hogan (1980) leads to the expectation that the direct effects of education on occupational attainment, controlling for earlier career status, will be greater among the migrants than among the nonmigrants. This does not appear to be the case among black adult migrants to the North. Migration from the South to

the North thus interrupts the career trajectories of black men who begin working prior to migration. Unlike white migrants, however, the black adult migrants to the North are not able to rely heavily on their educational credentials in obtaining a job commensurate with their qualifications, a fact which perhaps lessens their earning growth profiles.

The inability of the southern-born and southern-educated black men to convert their educational attainments into earnings attainments leads us to examine the educational quality hypothesis. Men residing in the North who were born and educated in the South receive low earnings returns to an additional year of schooling. Men born and educated in the South who remain in the South also receive low earnings returns to their schooling (Hogan and Featherman 1977). Men who were educated in the North, regardless of their place of birth, receive fairly high earnings returns to their schooling. This suggests that it is not something unique about migrants that causes their lower earnings returns to education. Rather, it appears that schooling received in the South is the critical factor distinguishing young black men who receive low earnings returns to schooling. Earlier, we reviewed the evidence that the education received by black men in the South was historically of lower quality than the education received by blacks in the North. We believe that this may be fairly widely recognized by employers who, in their hiring decisions, either discount or ignore schooling received in the South. To the extent that a good education is important in on-the-job performance, the southern-educated black men, with inferior education, may be less productive workers. A poor productivity record, in turn, probably would produce a slower rate of earnings growth. Thus, the earnings of southern-educated men with differing years of schooling completed would be roughly similar.⁷

This does not mean that men who were born and educated in the South and who migrate to the North as adults cannot earn relatively high wages. Such men are oriented toward jobs that pay well, even if they are of relatively low prestige. They work a greater number of hours per week and weeks per year. They display a commitment to doing relatively onerous work for pay—a commitment that is not shared by northern-educated men (Hogan and Pazul 1981). In short, men who were educated in the South

⁷ This argument implies that our structural equation models are misspecified owing to the exclusion of a measure of equality of education. If this omitted variable were hypothesized to have only additive effects on attainment, such a misspecification would produce a difference in the intercepts of the equations, not a difference in the regression slope of the education measure. But we do not believe the effects of school quality are simply additive. Rather, we believe that school quality interacts with years of schooling, with a poor quality of education reducing the distances between the educational attainments of men at adjacent levels of education. In this case, the omission of the school-quality measure would produce a downward bias in the regression coefficient for education, as observed in these models.

are more likely than northern-educated men to be in secondary labor market jobs where education is unimportant. In such jobs, they earn higher salaries than black men educated in the North, but forfeit the occupational prestige and earnings attainment advantages ordinarily associated with additional years of schooling.

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Intermarriage among Hispanic Ethnic Groups in New York City¹

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Intermarriage provides an excellent indicator of assimilation and of the social distance separating ethnic groups. Using 1975 marriage records this report describes the intermarriage patterns of five Hispanic groups in New York City. Particularly for the second generation, there are high rates of out-group marriage, both with other Hispanics and with non-Hispanics. Puerto Ricans provide a major exception to this pattern, having low rates of out-group marriage in both generations. A control for group size does not explain the low Puerto Rican second-generation rate. A comparison with 1949 and 1959 data indicates that an earlier trend toward Puerto Rican marital assimilation has reversed. Several possible explanations for the Puerto Rican pattern are suggested.

In a paper published well over a decade ago Fitzpatrick (1966) analyzed the intermarriage patterns of Puerto Ricans in New York City. By comparing the rates of marriage between first- and second-generation Puerto Ricans and non-Hispanics for 1949 and 1959 with the out-group marriage rates of New York City's foreign stock population in 1908 and 1912 (Drachler 1921), Fitzpatrick demonstrated that marital assimilation was proceeding in essentially the same manner among Puerto Ricans as it had among earlier immigrant groups. Low rates of out-group marriage for the first generation were followed by moderate rates for the second generation. Not only was the trend toward higher levels of intermarriage clearly delineated, but the levels were respectably high when compared with those in studies of other ethnic groups (Parkman and Sawyer 1967; Bugeleski 1961; Alba 1976; Abramson 1973; Mittelbach and Moore 1968).

Intermarriage represents a powerful indicator of assimilation. Gordon (1964) considers intermarriage, or marital assimilation, to be one of the more important dimensions of assimilation for a very good reason: high rates of intermarriage between groups can occur only when prior social

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processes associated with assimilation have progressed significantly. For example, some form of social proximity of large numbers of people from different ethnic backgrounds must have evolved into a common aspect of life. That proximity may be residential or occupational, or it may occur in other formal and informal settings. It must constitute a routine aspect of social life, or intermarriage will not occur at a high rate regardless of the similarity of the members of the ethnic groups in question. Of course, proximity constitutes only a necessary condition, not a sufficient one. Other barriers, such as racial prejudices, must have receded also. Just what sequence of events produces increases in proximity and reductions in other barriers remains largely unspecified.

Because a high rate of intermarriage can occur only when social proximity is common across many areas of social life, it provides an excellent indicator of interethnic social distance. Social distance refers to the orientation of members of one group toward members of other groups (Bogardus 1959; Laumann 1973; Gurak and Kritz 1978; Alba and Kessler 1979). The word "assimilation" often elicits negative responses because of its association with the disappearance of a group's culture into that of the dominant group. However, just marrying outside one's group does not automatically imply movement toward a core group. A marriage between a Dominican and a Puerto Rican, for example, may not imply such movement. Thus it is important to distinguish between out-group marriages among Hispanics and marriages between Hispanics and non-Hispanics.² Even out-group marriage between persons from two dissimilar cultural groups should not be interpreted as fostering the disappearance of one ethnic group into another, since social change could occur in each group's behavior and in the social distance separating groups. In this paper we shall apply a social distance perspective to the record of marital selectivity, in order to monitor intergroup relations of Hispanic ethnic groups in New York City.

Since the era covered by the Fitzpatrick analysis, the composition of New York's population has changed markedly. The Puerto Rican population increased from 612,574 in 1960 to 846,731 in 1970 (U.S. Bureau of the Census 1973). Perhaps of greater significance, the 1970 census found 1,278,593 individuals of Spanish language background; and the March 1974 Current Population Survey indicated that approximately 1,350,000 Hispanics resided in the city (Bondarin 1976). Several indicators confirm that the city's Puerto Rican population did not continue to grow during the 1970s or did so only at a very moderate rate (Fitz-

² The category "non-Hispanics" can only loosely be associated with a core social group. It consists of non-Hispanic individuals of foreign stock and native-born offspring of native-born parents. In both cases the marriage records provide no information on race. Consequently, little can be said about the nature of this group other than that it does not consist of Hispanics except for a very few, at least third-generation, Hispanics.

patrick and Gurak 1979, p. 16). Increasingly, "Hispanic" in New York City refers to Dominicans, Colombians, Ecuadorians, Cubans, and other immigrants and their descendants from South and Central American countries. In this context, two central questions will be asked: To what extent has the marital assimilation pattern observed for Puerto Ricans in 1949 and 1959 persisted through changes in the ethnic composition and general social and economic conditions, and what intermarriage patterns characterize the non-Puerto Rican Hispanic groups?

THE DATA: MARRIAGES IN NEW YORK CITY IN 1975

All 1975 marriage records for the five boroughs of the city that met at least one of the following conditions were coded: either (1) the bride or groom was born in Puerto Rico, Cuba, the Dominican Republic, Mexico, or a Spanish-speaking Central or South American country; or (2) one of the parents of the bride or the groom was born in one of those locations.

New York City records provide an excellent source of information on the current incidence of various forms of marriage. The records contain data on the birthplace of each spouse and of both parents of each spouse, thereby allowing a careful specification of the national origin of first- and second-generation residents of the United States. For Hispanics these data provide a comprehensive coverage because the recency of their immigration (since World War II) means that essentially no significant third-generation component exists among adults.

However, the marriage-record data are less than perfect with respect to population coverage. First, individuals residing in New York need not marry in the city: an unknown but small percentage of Hispanics who married in 1975 do not appear in the database. Second, in growing numbers, couples are living together without legally marrying. This is an increasingly common pattern throughout U.S. society (Glick and Norton 1977), and the tradition of consensual unions for Caribbean Hispanics may augment the tendency somewhat for the population under consideration. However, census data on marital status indicate that the marriage rate for Hispanics living in the United States is higher than that for the rest of the population (U.S. Bureau of the Census 1976). In fact, the problems associated with achieving legal permanent resident status may motivate a higher rate of legal marriage among Hispanics.

Using the above criteria, we coded 15,955 marriage records, representing all marriages in which at least one spouse was a first- or second-generation Hispanic. These records constituted 25.9% of all 1975 marriages in the city. Of these marriages, 47% involved Puerto Ricans (but no other Hispanics), 37% involved non-Puerto Rican Hispanics, and 16% involved both Puerto Ricans and other Hispanics. Of the 27,712 Hispanics included

in these marriages, 55.8% were Puerto Ricans, 18.8% were South Americans, 13.1% were Dominicans, 8.1% were Mexicans or Central Americans, and 4.2% were Cubans. These basic descriptive statistics clearly indicate the diversity of the broad category "Hispanics."

THE FINDINGS

The tendency for Hispanics to marry within their own national origin group varies considerably among the five Hispanic groups. Table 1 presents the exogamy rates for marriages and partners (controlling for sex and generation). Whereas only 29.5% of Puerto Ricans married non-Puerto Ricans, 56.1% of Central Americans and 63.4% of Cubans married outside their groups. Only Dominicans approach the low Puerto Rican exogamy level, but their rate (37.9%) is considerably higher. Because out-group marriage rates are generally higher in the second generation (Abramson 1973; Bugeleski 1961; Gurak and Kritz 1978) the low Puerto Rican out-group marriage rate is all the more surprising: it obtains despite the relatively large size of the Puerto Rican second generation. Although generational status is positively related to out-group marriage for four of the groups, it is unrelated to out-group marriage for Puerto Ricans. The second-generation out-group marriage rates for non-Puerto Rican Hispanics range from 75.7% for Dominican brides to 95.2% for Central American grooms, but those for Puerto Rican grooms and brides are 27.7% and 30.2%, respectively. Some of the higher out-group marriage rates of the non-Puerto Rican Hispanics are due to the small absolute size of these groups. However, as we shall see, the size factor does not completely account for the low Puerto Rican rates. Second-generation exogamy rates for the non-Puerto Rican Hispanic groups are high even when compared with those of ethnic groups of European origin, and those of Puerto Ricans are relatively low (Bugeleski 1961; Abramson 1973). Hispanic brides tend to have higher out-group marriage rates than males in both generations.

When Hispanics marry outside their own groups, whom do they marry? Table 2 presents a matrix of ethnicity-of-spouse percentages for the brides and grooms of the five Hispanic groups. The figures along the diagonal represent the in-group, or endogamy, rates. It can be observed that Puerto Rican and Dominican brides and grooms have the highest rates of endogamy and the lowest rates of intermarriage with third-generation Americans and other non-Hispanics. The rate of intermarriage with third-generation Americans is lowest for Dominicans. There is a high rate of intermarriage between Puerto Ricans and other Hispanics. This tendency is strongest for the brides of non-Puerto Rican Hispanic groups who evidence high rates of marriage with Puerto Rican grooms.

TABLE 1

HISPANIC OUTGROUP MARRIAGE RATES, BY ETHNIC GROUP, SEX,
AND GENERATION, NEW YORK CITY, 1975*

	Puerto Ricans	South Americans	Dominicans	Central Americans†	Cubans
Marriages.....	45.8 (10,051)	65.2 (3,880)	55.0 (2,515)	72.0 (1,751)	77.6 (951)
Individuals.....	29.5 (15,451)	48.4 (5,223)	37.9 (3,639)	56.1 (2,237)	63.4 (1,162)
Grooms (Total).....	27.3 (7,497)	49.5 (2,669)	38.0 (1,823)	54.3 (1,075)	62.5 (568)
First generation.....	(5,054)	(2,597)	(1,784)	(1,012)	(465)
Overall.....	27.2	48.3	37.1	51.8	56.6
With non-Hispanics.....	11.9	16.1	6.0	23.7	23.8
Second generation.....	(2,443)	(72)	(39)	(63)	(103)
Overall.....	27.7	91.7	76.9	95.2	91.3
With non-Hispanics.....	8.1	52.7	35.9	48.5	48.5
Brides (Total).....	31.5 (7,954)	47.2 (2,554)	37.7 (1,816)	57.7 (1,162)	64.1 (594)
First generation.....	(4,707)	(2,492)	(1,779)	(1,082)	(490)
Overall.....	32.4	46.3	36.9	55.2	57.8
With non-Hispanics.....	15.0	22.3	7.8	22.9	32.4
Second generation.....	(3,247)	(62)	(37)	(80)	(104)
Overall.....	30.2	80.6	75.7	92.5	94.2
With non-Hispanics.....	18.2	52.2	33.4	63.9	59.7

* Main figures are percentages of group specified by row and column headings. Numbers in parentheses represent number of cases used as the base in computation of percentage.

† Central American category includes Mexicans.

TABLE 2
DISTRIBUTION OF ETHNICITY OF SPOUSES FOR HISPANIC BRIDES AND GROOMS, NEW YORK CITY, 1975*

BRIDES								
GROOMS	Puerto Ricans	South Americans	Dominicans	Central Americans	Cubans	Third-Generation Americans	Non-Hispanic Foreign Stock	Totals
Puerto Ricans.....	72.7/68.5	6.1/17.9	4.8/19.8	3.4/22.2	1.1/13.5	8.6/...	3.4/...	100%/...
South Americans.....	23.3/7.8	50.5/52.8	4.0/5.8	3.1/7.2	2.9/13.1	9.4/...	6.7/...	100%/...
Dominicans.....	24.6/5.6	3.8/2.7	62.0/62.3	2.4/3.8	1.1/3.4	3.8/...	2.2/...	100%/...
Central Americans.....	20.4/2.8	4.7/2.0	4.5/2.6	45.7/42.3	1.0/1.9	14.8/...	8.9/...	100%/...
Cubans.....	21.1/1.5	10.0/2.2	5.1/1.6	3.3/1.6	37.5/35.9	15.0/...	7.8/...	100%/...
Third-generation Americans.....	.../8.2	.../9.2	.../4.4	.../10.8	.../13.5			
Non-Hispanic foreign stock...	.../6.8	.../13.1	.../3.4	.../12.1	.../18.9			
Totals.....	.../100%	.../100%	.../100%	.../100%	.../100%			

* The percentage distribution of brides' marriage choices is presented in the italicized figures, organized in columns. The figures for grooms, which are not italicized, are organized in rows situated to the left of the figures for brides. For example, 6.1% of Puerto Rican grooms married South American brides, and 7.8% of Puerto Rican brides married South American grooms. Data on third-generation Americans and "other ethnicity" are provided only when the individual is a spouse of a Hispanic; we do not have data on all marriage choices of non-Hispanics.

Intermarriage among Hispanics

Group size has a mathematical effect on intermarriage rates. Besanceney (1965) has pointed out that each additional intermarriage causes a larger increase in the intermarriage rate of a small group than of a large group. This is simply a function of the size of the percentage base. Similarly, whereas a majority group can never reach a 100% out-group marriage rate, it can occur quickly for small groups. Furthermore, small groups are more subject to "forced" exogamy—that is, intermarriage induced by imbalances in their age-sex structure.

Size differentials are also shaped by social factors such as the ability to sustain ethnic community activities on a scale that encompasses much of everyday life. For many Puerto Ricans in the South Bronx, it is not unusual to live in an area that is predominantly Puerto Rican, to work in a firm with many other Puerto Ricans, and to shop in Puerto Rican stores. For smaller groups, the probabilities dictate that interaction is more likely to occur with members of other ethnic groups, all other things being equal. Because these social mechanisms vary according to the culture of different groups and according to variations in the relations of particular groups to the dominant group, it is difficult to develop an appropriate control for group size although some adjustment is necessary (Besanceney 1965; Alba and Kessler 1979; Parkman and Sawyer 1967). Only when we eliminate the mathematical impact on intermarriage rates can these rates provide a meaningful indication of the social distance separating ethnic groups. The following analysis describes the social distances separating Hispanic groups from each other and from non-Hispanic groups.

Table 3 presents a social distance matrix for first- and second-generation Hispanics which was computed following the procedure used in Park-

TABLE 3
SOCIAL DISTANCE MATRIX BY GENERATIONAL
STATUS OF GROOM, NEW YORK CITY, 1975*

FIRST GENERATION	SECOND GENERATION					
	Puerto Ricans	South Americans	Dominicans	Central Americans	Cubans	Non- Hispanics
Puerto Ricans...	0	8	12	8	12	25
South Americans...	13	0	14	6	12	4
Dominicans.....	15	23	0	8	10	23
Central Americans..	16	22	24	0	12	11
Cubans.....	22	18	27	38	0	21
Non-Hispanics†....	29	29	40	29	33	0

* The figures for first-generation marriages are presented below the diagonal; those for the second-generation are presented above the diagonal.

† Here third-generation Americans and others are combined into non-Hispanics because we do not have data on intermarriage between these two groups or the total sizes for each group. This does not affect the social distance scores among Hispanic groups, but it probably produces a lower distance metric between each Hispanic group and the amalgamated non-Hispanic group than would be obtained for the Hispanic groups and third-generation Americans.

man and Sawyer's (1967) study of intermarriage in Hawaii. Their social distance index is computed as follows:

$$SD = 10 \times \log_{10} \left(\frac{N(A, A) \times N(B, B)}{\{\frac{1}{2}[N(A, B) + N(B, A)]\}^2} \right).$$

It is a ratio of within-group marriage to between-group marriage for a pair of groups. The use of products instead of sums eliminates the effect of differences in group sizes. The numerator is the product of in-group marriage frequencies, and the denominator is the squared average of between-group marriage frequencies.³ Parkman and Sawyer's procedure has a straightforward logic and permits comparison of Hispanic social distances with those of ethnic groups in Hawaii—a locale where intermarriage is common. The resulting metric—essentially a ratio of in-group to out-group marriages—estimates the social distance separating pairs of ethnic groups. This social distance metric is based on intermarriage rates but is independent of the mathematical influences of group size. Each group is at zero distance from itself. Higher scores indicate greater social distances. In practice, scores higher than 50 are unusual.

The social distance scores presented in table 3 indicate that to a high degree the very low Puerto Rican exogamy rates can be attributed to the relatively large size of that group. First-generation Puerto Ricans are closer to other Hispanic groups than those groups are to each other: the only exception to this pattern is Cubans and South Americans, whose social distance (18) is less than that of Cubans and Puerto Ricans (22). Perhaps more significant is the fact that although the social distance between Puerto Ricans and non-Hispanics is great (29), it is no greater than that of any other group and smaller than that of either Dominicans or Cubans.

The picture changes significantly for second-generation Hispanics. The social distance separating all Hispanics from each other declines significantly. Similarly, large declines occur for all groups except Puerto Ricans in the distances separating them from non-Hispanics. Central Americans and, even more, South Americans are indexed as very close to non-Hispanics. The distances for Dominicans and for Cubans are moderate, but lower than in the first generation by 17 and 12 points, respectively. The

³ The squared average is used in place of the product of between-group frequencies for technical reasons specified by Parkman and Sawyer (1967, p. 598). In practice the two techniques yield nearly identical results. When mate selection is random with respect to ethnicity, the ratio value is unity. The use of the log of ratio converts this zero-social-distance reference point to a value of zero. As intermarriage between individuals of a pair of groups becomes less frequent, the ratio value theoretically increases to an upper limit of infinity, but this will occur only when there are no between-group marriages. The use of logs keeps the range of values more reasonable. For example, an index of 20 is produced by a nonlog ratio value that is one-tenth the nonlog ratio for an index of 30.

Puerto Rican distance (25) is only slightly greater than the Dominican score, but it has declined only 4 points. The second-generation Puerto Rican score places that group in the most distant location with respect to non-Hispanics. Although differences in group size do exaggerate the non-assimilation pattern of Puerto Ricans, size cannot account for the relatively great social distance separating second-generation Puerto Ricans from non-Hispanics. In brief, not only are second-generation Puerto Ricans the most distant of all Hispanics from non-Hispanics, but their decrease (net of size effects) in social distance between generations is by far the smallest.

In order to provide a point of comparison for interpreting the social distance scores presented in table 3, we shall now summarize the scores determined for Hawaii by Parkman and Sawyer (1967). In Hawaii, for the period from 1929 to 1934, the social distances separating Caucasians from each of the seven other groups ranged from 24 to 39; for 1948 to 1953, the range decreased to 20–24 points. The unweighted average distance for all groups was 32 in the earlier period and 23 in the later period. These scores highlight the relative closeness of New York City's Hispanics to each other (an unweighted average distance of 10.2 for the second generation). For the second generation, the distance between Hispanics and non-Hispanics also appears low in comparison with the Hawaiian situation. Of course, social distances in Hawaii may have continued to decline after 1953.

THE PUERTO RICAN SITUATION: 1949–75

What of change over time? Regardless of the current situation of a group, accurate assessment requires trend analysis. Any attempt to construct a long-term time series for these groups is hampered by the problem of insufficient cases. Data are available on Puerto Rican marriages in New York City in 1949 and 1959 (Fitzpatrick 1966). Table 4 presents these data along with data for all New York City foreign stock marriages in the 1908–12 period, and the 1975 data. The original Fitzpatrick study was concerned only with the marriages of Puerto Ricans to non-Hispanics. Consequently, Hispanic intermarriages were excluded from both the numerator and the denominator of the exogamy rate. Although this procedure presents no problems for an analysis at one point in time, it does create biases over a period of time to the extent that the rate of intermarriage of Puerto Ricans with other Hispanics has changed. If it can be assumed that there has been an increase of intermarriage between Puerto Ricans and other Hispanics, the 1975 exogamy rates reported in table 4 will be biased upward in 1975. That is, the denominator will increase relatively more in 1975 than in earlier years with the inclusion of intermarriages

TABLE 4

COMPARISON OF 1975 PUERTO RICAN OUT-GROUP MARRIAGE RATES WITH
THOSE OF 1959 AND 1949 AND WITH ALL FOREIGN STOCK MARRIAGES
DURING 1908-12, BY GENERATION*

	FIRST GENERATION		SECOND GENERATION		INCREASE IN SECOND GENERA- TION† (%)
	%	N	%	N	
Brides:					
Puerto Rican (1975)....	12.9	3,606	20.7	2,943	7.8
Puerto Rican (1959)....	6.0	7,257	33.1	717	27.1
Puerto Rican (1949)....	8.5	3,077	30.0	523	21.5
1908-12.....	10.1	61,823	30.1	14,611	20.0
Grooms:					
Puerto Rican (1975)....	9.4	4,003	21.3	2,344	11.9
Puerto Rican (1959)....	3.6	7,078	27.4	638	23.8
Puerto Rican (1949)....	5.2	3,079	28.3	378	23.1
1908-12.....	10.4	64,577	32.4	12,184	22.0

SOURCE.—1975, New York City marriage records; 1949 and 1959, Fitzpatrick (1966); 1908-12, Drachler (1921).

* Marriages of Puerto Ricans to non-Puerto Rican Hispanics omitted in order to render the 1975 data comparable to the 1959 and 1949 data.

† Difference between third and first columns.

involving other Hispanics, although all numerators will remain the same.

The 1975 data provide reason to suspect that earlier assimilation patterns are not being replicated. Whereas the 1975 exogamy rates for first-generation Puerto Ricans are higher than those of previous eras, those for the second generation have declined from 27.4% in 1959 to 21.3% in 1975 for grooms, and from 33.1% to 20.7% for brides. Similarly, the generational differential has shrunk. These findings underestimate the trend reversal because the exclusion of Hispanic intermarriages from the denominator of the exogamy rate biases those rates upward more in 1975 than in earlier years. For example, the exogamy rate for non-Hispanics, taking all marriages as the denominator for second-generation grooms in 1975, is 8.1%: the upward bias has to be stronger in 1975 because of the recency of the arrival of most non-Puerto Rican Hispanics. The analysis, then, requires explanations for the second-generation Puerto Rican pattern.

DISCUSSION

Several themes emerge from this overview of Hispanic intermarriage. The results lead us to consider the considerable diversity which exists within the broad category of Hispanics. With the exception of Puerto Ricans and

first-generation Dominicans, intermarriage rates are high for Hispanics in New York City. Especially among the second generation, many of these intermarriages involve third-generation Americans and other non-Hispanics. There is a considerable amount of intermarriage among members of the various Hispanic ethnic groups. The most common manifestation of this phenomenon is the high rates of intermarriage between Puerto Ricans and other Hispanics. Indeed, when we control for differences in group size, the social distances between Puerto Ricans and other Hispanics are quite small.

Group size emerges as a major determinant of group differences in exogamy rates in general and intermarriage rates with non-Hispanics in particular. Controlling for group size, however, does not alter the basic conclusion that second-generation Puerto Rican marriage patterns with respect to non-Hispanics differ markedly from those of other Hispanic groups. The extension of a time series for Puerto Ricans reinforces this conclusion and indicates that previous trends toward marital assimilation of Puerto Ricans in New York City have reversed. Several investigators have noted low rates of intermarriage of Mexican Americans and non-Hispanics (Mittelbach and Moore 1968; Murguia and Frisbie 1975). In general the rates of intermarriage between Hispanics and non-Hispanics in New York far exceed those observed in studies of Mexican Americans in the Southwest. However, the rates for Puerto Ricans are low by this standard, too. For example, in 1963 in Los Angeles 23.3% of second-generation Mexican-American grooms and 25.6% of brides married non-Hispanics. The overall out-group marriage rate for males and females in Bernalillo County, New Mexico (Albuquerque) was 37% in 1971. The male exogamy rate in Bexar County, Texas (San Antonio), in 1971 was 13%. For second-generation Puerto Rican males in New York City in 1975 the intermarriage rate with non-Hispanics was 8.1%.

How can we explain the low rate of intermarriage among second-generation Puerto Ricans, and their relatively great social distance from non-Hispanics? As noted, relative group size and concentration contribute to this pattern, but cannot account for all of it. Several factors probably contribute to this phenomenon. The first is the possibility that second-generation Puerto Ricans may be experiencing a relatively greater emphasis on their ethnic identity than are members of the other Hispanic groups in New York. This emphasis would have to have increased over time to assist us in explaining the observed trend toward reduced levels of second-generation intermarriage among Puerto Ricans. The size of the Puerto Rican population in New York may contribute to the observed intermarriage patterns in any of several of the social nonmathematical ways mentioned above—such as the increased ability to provide institutional completeness

and thus increased insulation from other groups (Breton 1964). Puerto Ricans are distinct from the other Hispanic groups studied in that they are native-born citizens of the United States. This fact certainly makes travel back and forth to the homeland easier for Puerto Ricans than for other Hispanics, and may discourage the formation of commitments to permanent settlement (Gurak and Rogler 1980). This line of thought appears reasonable, but its validity and importance relative to other explanations cannot be assessed with existing data.

Two additional lines of analysis deserve serious consideration because of both their obvious relevance and the existence of supporting evidence. These explanations focus on residential and occupational segregation, and on the possible selectivity of the migratory patterns of Puerto Ricans. Recent studies indicate considerable residential segregation of Puerto Ricans in New York City. Massey (1979) found that the level of segregation between the Spanish-speaking population and the non-Spanish-speaking white population was higher in New York than in any of the other 28 large urbanized areas examined. Although the Spanish-speaking category includes non-Puerto Rican Hispanics, the large size of the Puerto Rican population of New York in 1970—the date of the census data used in the Massey analysis—indicates that the residential pattern for Puerto Ricans dominates the index. Furthermore, Rosenberg and Lake (1976) document some of the mechanisms of Puerto Rican residential segregation in New York, involving the simultaneous competition of poor Puerto Ricans with majority whites and with members of a larger minority—blacks. They argue that this three-way competition, along with the usual immigrant problems of low income and housing discrimination, strongly inhibits the assimilation of the Puerto Rican minority. Other investigators have noted the negative relationship between residential segregation and assimilation. Lieberman (1962), for example, used aggregate data to demonstrate that ethnic groups that are more segregated have lower rates of intermarriage and score lower on other indicators of assimilation.

Thus the residential segregation of Puerto Ricans, whether voluntary or forced, contributes to their low level of out-group marriage. Nevertheless, the relative importance of this factor cannot be specified, and it is unclear why this pattern is more pronounced among Puerto Ricans. Members of other Hispanic groups tend to be of higher occupational status (Fitzpatrick and Gurak 1979), and this factor may provide part of the explanation. First-generation Dominicans are even less likely than Puerto Ricans to marry non-Hispanics (table 1) and their occupational distribution is similar to that of Puerto Ricans: 55.4% of each are employed in low-status occupations, and 7.4% and 9.1%, respectively, are employed in high-status occupations. Out-group marriage for second-generation Do-

minicans greatly exceeds that of second-generation Puerto Ricans, and second-generation Dominicans tend to be employed in higher-status occupations. For example, while only 11.6% of second-generation Puerto Rican males were employed in high-status occupations, 20.5% of second-generation Dominicans were.⁴ Given the small size of the Dominican second generation in 1975, these observations should be interpreted cautiously. Should the pattern persist as the Dominican population grows, the search for explanations would have to focus on the reasons for the lower occupational status of Puerto Ricans.

The argument that economically better off and more assimilated Puerto Ricans are relatively more likely either to leave New York or to settle initially outside the city also receives some support. This line of reasoning is appealing because the various Hispanic groups have very different settlement patterns both within the city and at the national level. Given these differences and the implied differences in antecedent mechanisms, it should not be surprising to find differences in behavioral indicators such as intermarriage. New York City is the principal mainland settlement area for Puerto Ricans and Dominicans. While the other Hispanic groups are also large, the city constitutes a relatively minor settlement area for them. For example, South Americans are well dispersed in the metropolitan United States, and Cubans are far more concentrated in Florida and New Jersey (Jaffe, Cullen, and Boswell 1976). Residential dispersion is associated with higher rates of intermarriage (Fitzpatrick and Gurak 1979). In fact, the prevalence rate of out-group marriage for second-generation Puerto Ricans at the national level greatly exceeds the incidence rate observed for the second generation of Puerto Ricans in New York City (Gurak and Kritz 1978). Prevalence rates are usually much lower than current incidence rates. It should be noted that differences in intermarriage rates might result from contextual differences among sites as well as from selective migration (Murguía and Frisbie 1975). Wilber and Hagan (1975), for example, indicate that the economic situation of Puerto Ricans is worse in New York than in other metropolitan regions of the country. Whether this reflects selective migration or differential opportunities cannot be specified with existing data.

The speculations provided here represent only a few of the possible explanations for the low Puerto Rican intermarriage rates. Various combinations of factors may interact to produce the observed results. In any case, new investigations are needed to specify the social processes that are at work.

⁴ Low-status occupations include laborers, operatives, private household workers, and workers in similar occupations. High-status occupations include professional, technical, management, and administrative occupations (Fitzpatrick and Gurak 1979, pp. 50-51).

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Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

A COMMENT ON SCHOEN AND COHEN¹

The rapid development of mathematically intricate methods of data analysis over the past decade creates serious dilemmas for many sociologists. The use of such methods in any analysis appears to give the results the imprimatur of authority and leads some researchers to believe that they must attempt the most mathematically sophisticated treatments if their work is to be accepted. Those who lack the statistical and mathematical foundation to grasp the essential principles of these methods usually must resort to following the outlines of applications provided on the pages of the major journals.

Consequently, there is reason to be concerned about Schoen and Cohen's (*AJS* 86 [September 1980]: 359-366) reanalysis of data originally presented by Mittelbach and Moore (1968). The data concern ethnic endogamy among Mexican American grooms and at issue is the interpretation of a three-variable contingency table: type of marriage (endogamous or exogamous) by occupation (high, medium, or low) by generation in the United States (first, second, third and higher). A version of this table, giving percentages, a version not presented by Schoen and Cohen, is shown as table 1. In but one part of a broad article, Mittelbach and Moore interpret the

¹ I am grateful for discussions with Steve Rytina about issues raised in this comment.

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TABLE 1
PERCENT EXOGAMY AMONG MEXICAN AMERICAN GROOMS,
BY OCCUPATION AND GENERATION OF RESIDENCE,
LOS ANGELES COUNTY, CALIFORNIA, 1963

OCCUPATION	GENERATION		
	First	Second	Third or Higher
High.....	33.8 (74)	36.9 (187)	48.6 (138)
Middle.....	12.5 (353)	24.0 (766)	24.4 (870)
Low.....	11.3 (626)	19.6 (810)	34.4 (593)

SOURCE.—Calculated from Schoen and Cohen (table 1); derived originally from Mittelbach and Moore (1968, table 3).

NOTE.—Numbers in parentheses indicate total *N* grooms.

table, largely on the basis of a rank ordering of its percentages, as indicating that occupation is more important than generation in explaining intermarriage. Schoen and Cohen, however, employing log-linear analysis, conclude in "Ethnic Endogamy among Mexican American Grooms" that generation is the more important variable. A major point of their research note appears to be the superiority of log-linear models over earlier and simpler methods of interpreting contingency tables. They assert that their analysis provides "another example of the importance of rigor in the analysis of contingency tables, as earlier, less powerful methods are prone to error, especially when significant interactions are present" (p. 365).

I believe instead that Schoen and Cohen's article represents a species of methodological mystification. My quarrel is not with the use of log-linear methods in general but with the disturbing assumption that such methods are automatically superior to more commonsense approaches to a contingency table.

To begin with, Schoen and Cohen overstate the benefits of log-linear methods for the analysis of *this table*, and they neglect a serious disadvantage. In general, log-linear methods offer two advantages over simpler methods. One is that they provide a systematic way to test for the presence of interactions among variables. This is of some help in the analysis of table 1 for, as Schoen and Cohen demonstrate, a model without a three-variable term does not fit the data. Therefore, occupation and generation do not have independent effects on marriage type. There is indeed an interaction effect in the table, but the omnibus test afforded by the model fitting does not make its nature clear.

The other advantage of log-linear models fails to materialize here. It lies in their potential to summarize the information contained in many cells with a smaller number of parameters. This parsimony is achieved at a cost, however, because the parameters of a log-linear model are harder to inter-

pret than are probabilities or percentages, as Swafford (1980) argues. There are two versions in which the parameters may come. In one, they are interpreted additively as increments to the natural logarithm of a cell frequency or an odds. In the other, they are interpreted multiplicatively as factors in a product producing a frequency or an odds. Because neither of these versions is easy to grasp in substantive terms, one ordinarily would only want to use for interpretive purposes the parameters of an *unsaturated* log-linear model, one with fewer parameters than there are cells. But the *saturated* model, with as many parameters as cells, is the one used by Schoen and Cohen because no unsaturated model fits the data. Thus, all they have achieved is a rearrangement of the table into an equivalent but more opaque form, presented here as table 2. For most people, the patterns in the data will be more visible in table 1 than in table 2, even after the meaning of the numbers in the latter has been explained.

Moreover, the authors report incorrect estimates of the parameters. They intend to focus on a logit form, in which the logarithm of an odds depends on an additive function. However, the well-known computer programs for log-linear modeling (e.g., ECTA) report estimates for parameters in a different form, in which the logarithm of a cell frequency is the dependent quantity. Conversion of these estimates to those appropriate for the logit model is easy: one need only double the estimates associated with terms involving the dependent variable (Swafford 1980). But according to my analysis, Schoen and Cohen have not done so. (I have reported correct numbers in table 2). The failure to double does not affect the pattern of the additive parameters but would affect the derivation of multiplicative parameters (not presented by Schoen and Cohen)—a point worth noting because multiplicative parameters are probably preferable for interpretation (see Swafford's [1980] example).

That the log-linear formulation of the table is difficult to grasp is indicated by the authors' discussion of the interaction effect. They suggest "a

TABLE 2
ADDITIVE EFFECTS IN THE SATURATED LOGIT MODEL
FOR THE ODDS FAVORING ENDOGAMY

OCCUPATION	GENERATION			MAIN EFFECTS OF OCCUPATION
	First	Second	Third	
High	-.242	.150	.092	-.646
Middle.....	.046	-.224	.178	.344
Low.....	.194	.074	-.268	.302
Main effects of generation.....	.492	-.036	-.456	...

SOURCE.—Derived from Schoen and Cohen (table 3) by doubling the numbers they report. See text for an explanation of why this has been done.

weakening of occupational effects over successive generations" as the most plausible interpretation of the interaction, and go on to state that "the three negative effects [in the interaction] indicate that the interaction of first generation with high occupational level, second generation with middle occupational level, and third generation with low occupational level increases the likelihood of out-group marriage" (p. 365). But this last statement fails to rise above a literal and uninformative reading of table 2. The case for their interpretation seems clearer in table 1, as does its tenuousness. If *only* the low and high occupational categories are compared in table 1, the effect of occupation seems weaker over successive generations; and, correspondingly, the overall effect of generation seems weaker for the two higher occupational groups than for the lowest one (a point not noted by Schoen and Cohen). These statements, however, hinge on small differences between percentage differences.

Further, Schoen and Cohen miss a peculiarity in the table that provides another possible interpretation of the interaction, and in such a small table there is no reasonable way to determine which interpretation is "better." The peculiarity is the aberration in table 1 from the basic pattern of increasing intermarriage with each generational and occupational step—namely, the relatively low intermarriage rate of the third-generation members of the middle occupational group. Although visually striking among the percentages, this feature is lost in table 2.² It can be considered, however, part of the interaction, as one can demonstrate with the appropriate log-linear methodology. After the two appropriate cells are blanked out, the model lacking only the three-variable term just fits the cell frequencies: $\chi^2 = 6.69$ (df = 3), $p > .05$. The reasons for this low intermarriage rate are not clear, but it does call attention to the crudity of the occupational categories.

The key point is that one should not, as Schoen and Cohen do, stake too much on an interpretation of the three-variable table by itself, since the nature of the interaction effect it contains seems murky and the occupational categories too crude to assess fairly the effect of occupation.³ The need for tentativeness seems clearer from table 1 than from table 2, and Schoen and Cohen appear to be beguiled by a sense that the estimated parameters of a log-linear model are precise in a way that simple percentages are not. This sense is a delusion in this case.

² It was noted by Mittelbach and Moore (1968, p. 55).

³ I note in passing that for two reasons the issue of whether generation or occupation is the more important variable is not resolved by Schoen and Cohen's use of coefficients of partial determination. First, the interpretive status of these coefficients is problematic in general because log-linear models do not explain individual variation within cells (Swafford 1980, p. 669). Second, the comparison of the two coefficients in this case, .58 and .72, seems statistically meaningless. Is the difference between them large? small? statistically significant?

Finally, I take issue with Schoen and Cohen's use of the term "structural assimilation." Even though terminology is in principle arbitrary, consistency of usage has an obvious importance. The authors appear to base their usage on Gordon's (1964) famous discussion of assimilation, but they misinterpret the primary meaning of structural assimilation by using occupational status as an indicator of it. Gordon's emphasis was decidedly on integration in terms of primary relationships, such as friendship (e.g., pp. 70 and 80 in Gordon [1964])—not socioeconomic integration—and his emphasis has passed into the literature (see Alba [1976] and literature cited there). Hence, despite Schoen and Cohen's assertions to the contrary, there is a very close link between intermarriage and structural assimilation in the usual meaning of that term (Gordon 1964, p. 80; Alba 1976).

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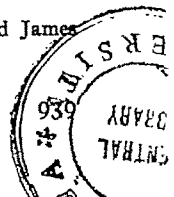
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THEORY AND METHOD IN THE STUDY OF ETHNIC ENDOGAMY AMONG MEXICAN AMERICAN GROOMS¹

The spread of mathematically sophisticated analytical techniques in sociology carries with it both the promise of increased analytical power and the danger of greater obfuscation. Researchers must always take care to balance the power against the peril. In the present case, we remain convinced that our paper is methodologically sound, substantively meaningful, and theoretically relevant. Alba's comment appears to be based on a misinterpretation of what we said and an overly restrictive view of the nature of log-linear analysis.

¹ Discussions with David Cantor, W. Parker Frisbie, Robert L. Kaufman, and James R. Kluegel were most helpful in the preparation of this reply.



We chose to reanalyze the Mittelbach and Moore (1968) contingency table of 1963 Los Angeles County Mexican American in- and out-group marriages, cross-classified by occupation and generation, because it was part of a first-rate study whose theoretical and substantive value has remained undiminished while new methodological tools have come into their own. In particular, we wished to reassess the validity of their claim (Mittelbach and Moore 1968, p. 57) that "occupation is more significant than generation in explaining outmarriage" for Mexican Americans because we felt, on theoretical grounds, that such was not likely to be the case.

Our theoretical approach derived from Gordon's valuable contribution in distinguishing among the various forms of assimilation and his observation that cultural assimilation is likely to be the first type to occur and may take place even in the absence of other forms (Gordon 1964, p. 77). We used the term "structural assimilation" in a somewhat different sense than Gordon did, hence we defined our meaning of the term (i.e., as reflecting incorporation into the social and economic structure of the society at all levels) immediately after introducing it. It is premature to say that the literature contains a single, generally accepted meaning for the term "structural assimilation," and our usage is by no means novel (e.g., it is essentially identical with the one found in Bean and Frisbie [1978], p. 3). Our analysis was motivated by the theoretical belief that when cultural assimilation is proceeding rapidly and structural assimilation is still quite limited—a situation which characterizes the lives of Mexican Americans—substantial levels of exogamy are a response to cultural more than to socioeconomic factors. While recognizing the imperfect nature of the variables at hand, we argued that the cultural variable, generation, should be more closely related to out-group marriage among Mexican Americans than the socioeconomic variable, occupation.

Log-linear methods are appropriate for the analysis because of the strong interaction effect between occupation and generation present in the table and the ability of log-linear models to *decompose* such effects. Rather than simply "summarize" a contingency table, "*a log-linear model describes the structure of a table*" (Bishop, Fienberg, and Holland [1975], p. 25; emphasis added). We take issue with Alba's claim that the presence of a three-variable interaction term means that "occupation and generation do not have independent effects on marriage type." The saturated log-linear model, with as many parameters as cells, separates out the main effects of occupation and generation from the occupation-generation interactions, so that those main effects can be compared apart from the "noise" of their interaction. Alba's remark that all we have achieved is a "rearrangement of the table into an equivalent but more opaque form" reflects a misunderstanding of that important point. Moreover, the need for a more

formal approach to the analysis of the table than visual inspection or "common sense" is reinforced by the "aberration" that Alba sees in the table—the relatively low outmarriage proportion among third-generation members of the middle occupational group. It is precisely because the table does not follow a simple, obvious pattern that simple, obvious approaches are likely to be inadequate.

Consistent with our stated goal of comparing the relative effects of generational and occupational factors, we presented two different measures of relative effect. The first was a comparison of coefficients of partial determination (CPDs), which allow us to determine the main effects of each independent variable when the other is held constant (Goodman 1972, pp. 1056–58). The CPDs for generation and occupation were .72 and .58, respectively, indicating that the main effect of generation was "relatively greater." No statistical procedure exists to test the significance of that difference, but the result is meaningful because it shows that a log-linear decomposition leads to results which contradict the conclusions of Mittelbach and Moore.

The second comparison was based on λ -effect parameters, which represent the additive effects of each category of each independent variable on the odds of out-group marriage. It was not our intention "to focus on a logit form" but to present comparable figures. The λ -parameters shown are quite appropriate for that purpose. Moreover, as Alba notes, doubling the λ -values to make them conform to a logit model would not affect their pattern. Thus we fail to see any basis for his claim that we "report incorrect estimates." It is at most a matter of taste, and surely, to argue that the relative sizes of 2, 4, and 6 are different from the relative sizes of 1, 2, and 3 does not contribute to ridding the literature of "methodological mystification."

The full matrix of interaction effects was presented for the sake of completeness, but we devoted little attention to the interactions because the focus of our interest was on the main effects. The interaction effects include both generational and occupational aspects, and there is no unique or customary way to disentangle them meaningfully. As we noted, and as Alba apparently agrees, these effects are open to a number of interpretations. The nature of the interaction effects, however, does *not* affect our conclusions concerning the *main* effects. It would appear that once again Alba does not appreciate the use of the saturated log-linear model as a means of decomposing effects in contingency tables.

Our log-linear reanalysis does not conclusively demonstrate that generational effects were greater than occupational effects in the outmarriage of Mexican Americans, nor have we made such a claim. We must object to Alba's dropping our qualifying language and then criticizing us for making

unqualified statements. We do claim—and the analysis supports the claim—that occupational effects were not greater than generational effects, a result consistent with our theoretical position and contrary to the conclusion previously reached.

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ENERGY, INFORMATION, AND SOCIOCULTURAL "ADVANCEMENT"¹

In "The Idea of 'Advancement' in Theories of Social Evolution and Development" (*AJS* 85 [November 1979]: 489–515), Granovetter claims to have demonstrated that a meaningful ranking of societies on a dimension of "advancement" is impossible. He bases this claim on the insurmountable difficulties he finds inherent in any attempt to rank and compare societies in terms of their relative "problem-solving capacity" ("efficiency" and "adaptability"), which he maintains underlies or is implicit in all evolutionary rankings of "advancement." However, if we examine some of the ranking systems actually employed by evolutionists and the supporting arguments advanced by Granovetter, we find that this claim and the assertions following from it are unfounded. Granovetter's characterization of evolutionists' criteria of ranking is inaccurate and misleading, and he has not convincingly demonstrated that the task of ranking and the task of explaining evolutionary change are *inherently* impossible. In fact, he has not shown

¹ I would like to thank Gerhard Lenski, Bruce Mayhew, and Robert Miller for their helpful comments on an earlier draft of this comment.

that the classification systems actually employed by some evolutionary theorists are seriously flawed either theoretically or methodologically.²

For instance, Leslie White clearly argued that the critical variable defining "advancement" was not some abstract notion of "problem-solving ability" but was instead the relative amount of energy harnessed by the technology of a society (1959, p. 145; [1949] 1969, pp. 375-76). "Efficiency" entered this determination only to the extent that it affected this amount ([1949] 1969, pp. 368-69), and the ideal measure of advancement from this perspective would be a direct measure of *energy*, not of problem solving or efficiency, and certainly not of the "values" or "utilities" of the members of the societies being ranked.³

In citing Sahlins and Service (1960), Granovetter focuses almost exclusively on "specific" sociocultural advance. Had he considered their notion of "general" sociocultural progress in more detail, he might have agreed with Segraves (1974, p. 532), who sees their definition of advancement in terms of the "absolute amount of energy harnessed by the sociocultural system." This definition of general sociocultural progress, in fact, provided the basis for Lenski's definition of general sociocultural advance: "*the raising of the upper limit of the capacity of human societies to mobilize energy and information in the adaptive process*" (1970, p. 70; emphasis in original). The reader will notice that this definition differs from the composite one which Granovetter (p. 492) attributes to Lenski (1970) and Lenski and Lenski (1974) and might conclude that the definition cited here was a preliminary one which Lenski later discarded in a more refined version of his theory. Actually the reverse is true. Granovetter takes the "efficiency" part of the definition he cites from an earlier work, *Power and Privilege* (Lenski 1966, p. 93), but attributes it to the second edition of *Human Societies* (Lenski and Lenski 1974, p. 46).⁴

Granovetter is certainly correct to point out that in many cases "advancement" in terms of Lenski's criteria has entailed major inefficiencies in energy consumption and resource use, but it must be pointed out also that it was the amount of information accumulated or energy harnessed or

² Many of Granovetter's criticisms apply with full force to Parsons's self-labeled "evolutionary" theory; but since Granovetter claims to be demonstrating that *all* evolutionary rankings and *all* evolutionary theories are critically flawed, our attention will focus on their relevance for other theorists' work.

³ Severe space limitations prevent full citation of the relevant texts and discussion of the issues involved; however, a longer version of this comment including full citations and discussions is available from me.

⁴ The splicing of passages from two separate works would be misleading even if the sources were correctly identified, and one can only wonder why Granovetter found it necessary to construct his own definition of "advancement" when he was citing an introductory textbook with explicit definitions of terms.

both that provided the basis for ranking on advancement—not the efficiency of its use or production.

Furthermore, in addition to misrepresenting Lenski's criteria of ranking, Granovetter misrepresents his arguments concerning the generation of a "surplus" in societies by claiming that Lenski's theory and ranking system require the assumption that all societies will produce a "maximum possible surplus." Without such an assumption, Granovetter argues (p. 492), "other factors . . . would have to be considered." This is an ironic misrepresentation, given that Lenski has always considered the production of *any* surplus to be highly problematic and has been very much concerned with identifying and explaining these "other factors." At several points in his works, Lenski explicitly argues that horticultural and other more "advanced" technologies do not necessitate or automatically produce a surplus, they only make one possible (e.g., Lenski 1970, p. 235). Without the development of surplus-exacting and surplus-extracting institutions, surpluses cannot be assumed or expected to develop, and the assumption of a surplus is not necessary in order to rank different societies in terms of the relative amounts of energy and information mobilized by their respective technologies.

Some additional mischief is introduced into the question of ranking when we are informed by Granovetter that any ranking system must consider such individual factors as "choices," "cravings," "consumer preferences," and "valuations" in determining the relative efficiency and hence advancement of a society. In fact, if people in different societies want different things and pursue different ends, Granovetter would maintain that their technologies cannot be meaningfully compared because there is no common base or metric of comparison. This problem vanishes, however, if energy or information provides the basis of ranking. Energy and information can be quantitatively compared even if the means for producing them, their specific form, and their application vary (e.g., kilograms coal equivalent). In fact, it was this cross-level, cross-system comparability that made energy and information so attractive to evolutionists and general systems theorists in the first place.⁵

In his section on adaptive capacity, Granovetter shifts attention away from efficiency, the ability to solve present problems, to the capacity of a system to respond to future largely unanticipated problems of survival. Some evolutionists have argued that, *ceteris paribus*, the greater the store of accumulated information and the greater the capacity of the system to harness energy (not only in greater quantities but also from a wider variety of sources), the greater the chances a system has to survive a disturbance in its biophysical or sociocultural environment and hence the greater its overall chances of surviving in a changing environment (e.g., Segraves

⁵ This is not meant to imply that all problems of measurement are solved or solvable.

1974; Lenski 1970).⁶ To discredit this probabilistic generalization, Granovetter argues that the "advance" from subsistence to commercial agriculture actually *lowered* the adaptive capacity of societies in many cases because it increased the incidence of starvation in regions of the systems that adopted it. Although he is supposedly discussing factors that affect system survival, the evidence he marshals to support his argument involves individuals and subgroups, not the systems they constitute. He has not addressed the issue of system survival, and it is only by shifting levels of analysis that this evidence is made to appear to contradict the evolutionary proposition.

It is difficult to say whether or not, protests to the contrary, Granovetter's discussion of problems associated with nonlinearities is merely a sophisticated way of saying that macrosocial generalizations are not possible because macrosocial phenomena are too complex (see White [1949] 1969). What is clear, though, is that much of the "complexity" that Granovetter finds in making evolutionary generalizations results from his own particular characterization of the problem. His discussion of problems he has confronted in making predictions about "situations of collective behavior, where preferences are distributed in a nonlinear way" (p. 504), suggests that the problems of complexity may not be inherent in all attempts to make generalizations but only in attempts to make macrosystem predictions and generalizations by aggregating individual choices, wants, values, preferences, and the like. This problem is, therefore, not one of evolutionary theorizing per se but of a particular (reductionist, subjectivist, individualizing) approach to the task. Granovetter can certainly approach the task in this way and face these difficulties, but he cannot claim that all approaches are necessarily plagued by these problems of complexity. It is also ironic that he sees the introduction of intersystem contact and interaction as a problem or complication for evolutionary theorizing when it has been the starting point and cornerstone of many of the major theories of sociocultural evolution! It is hard to conceive of seriously raising this as a challenge to most evolutionary theories (e.g., Lenski 1970) when it is, itself, the linchpin of those theories. Intersystem contact and competition are major complications only if one assumes that change is immanent, or that any explanation of macrosystem change must proceed by aggregating the choices or behavior of the individuals that constitute the systems.

In his concluding section, Granovetter speculates that, while little will be lost, much will be gained by abandoning the idea of advancement and the pursuit of evolutionary explanations. It will shift attention away from

⁶ Granovetter does not cite Segraves (1974), but a careful reading of her paper would have answered many of his questions and criticisms. Segraves also offers an interesting macrostructural theory of sociocultural evolution.

static taxonomies and eliminate what Granovetter sees as a "deterministic" tendency in evolutionary approaches and a (mistaken or inadvertent) tendency to find that the variable used to classify societies on advancement is the most important variable explaining advancement. This appears to be the result of specious reasoning, since the importance of any variable in any explanatory schema is not determined a priori but by its relative success in accounting for variation in other variables of interest. In fact, a taxonomy based on a particular variable (e.g., technology) can clearly demonstrate the limits of that variable in accounting for variation in other variables. If some variable of sociological concern (e.g., stratification) evidenced considerable intracategory variation, it would indicate that the variable defining the categories was not a very powerful predictor of that phenomenon. It is, therefore, unfair and unfounded to claim that a theory is necessarily deterministic and monocausal simply because it employs a particular variable for classifying and ordering observations.

In conclusion, many of Granovetter's arguments appear irrelevant to the actual criteria used by many persons to rank societies on advancement and to their efforts to explain evolutionary change. By reformulating the basis of ranking to "problem-solving ability," and thereby ignoring the actual classification systems employed by some evolutionists, Granovetter concludes falsely that a meaningful ranking is impossible. What he has done, in large measure, is reject his own characterization of evolutionary rankings and evolutionary explanations. He has not demonstrated that the task of ranking is inherently impossible. One can agree that if the reductionist, individualistic, intracranial approach that Granovetter discusses at points in criticizing evolutionary theories were accepted, evolutionary rankings and evolutionary theories would be impossible, but under these same assumptions social science itself might well be impossible.

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REPLY TO NOLAN

Nolan offers two main criticisms of the arguments in my paper: (1) that by construing "advancement" in terms of societal problem solving, I distort the meaning of most evolutionist theorists; and (2) that my conception of problem solving is unduly reductionist. I will respond to these in turn.

Nolan's first and central argument is that I have set up and knocked down a straw man. I assert that if evolutionary advance is defined either by a system's ability to solve current problems or by its capacity to meet future ones, no rank ordering of societies along this dimension can be meaningful. Nolan believes, however, that the main line of evolutionist thinking does not define advancement in this way. If it did, he would apparently be much more sympathetic to my position, since he admits that "many of Granovetter's criticisms apply with full force to Parsons's self-labeled 'evolutionary' theory . . ." (his n. 2). Much of our dispute, then, turns on what "genuine" evolutionary theory is. In particular, he points out that the levels of energy and information generated by a society have been the main criteria of evolutionary advancement of such writers as Leslie White, Sahlins and Service, and Lenski.

But this strikes me as a quibble. Theorists use energy and information as measures of advancement because they are so certain of the connection of these quantities to societal adaptation to the environment. (I myself believe this connection to be tenuous, but space does not permit or logic require that I pursue this point here.) In my paper I have called such adaptation "problem solving." To do so is hardly out of line with the main body of evolutionary thought. The Lenskis (1978), for example, have recently defined "adaptation" as "the process of adjusting to, or changing, environmental conditions; hence, broadly, problem-solving" (p. 482). A theory which was interested in energy or information independent of its contribution to such adaptation could hardly be called "evolutionary"; the analogy to Darwinian ideas would be so strained as to be absurd.

In the works of White or Sahlins and Service, it is easy to find evidence that interest in energy results from a conviction that it is a convenient measure of how well various environmental problems have been solved. Culture, says White (1969), "is a mechanism for providing man with subsistence, protection, offense and defense, social regulation, cosmic adjustment, and recreation. But to serve these needs of man energy is required" (p. 367). That energy is the means, not the ultimate measure, is clear in the statement that "the degree of cultural development, measured in terms of amount of human need-serving goods and services produced per capita, is determined by the amount of energy harnessed per capita, and by the efficiency of the technological means with which it is put to work" (1969,

p. 368); also see White (1959, pp. 41, 49). Similarly, Sahlins (in Sahlins and Service 1960) defines "progress" as the "total transformation of energy involved in the creation and perpetuation of a cultural organization. A culture harnesses and delivers energy; it extracts energy from nature and transforms it into people, material goods and work, into political systems and the generation of ideas, into social customs and into adherence to them" (p. 35).¹

My arguments against the usefulness of the idea of "advance" are logically independent of the *means* by which adaptation is achieved or problems solved. It is for this reason that I did not discuss questions of energy or information in any detail. Establishing this does not prove that my arguments are correct—only that this part of Nolan's attack on them is misconceived.

Even if Nolan concedes that evolutionary theory may properly be said to be about problem solving, he might still contend that my notion of what "problems" societies face is unduly reductionist. He objects to my introduction of individuals' choices, values, and preferences into a determination of what problems societies actually have. But to avoid such considerations I see only two options: (1) Imagine that each member of any society has exactly the same assessment of what are important societal problems as each other member; then the society's problems may be taken as an undifferentiated unit. This is, of course, highly unlikely, and among other difficulties, eliminates the possibility that different members of or groups in a society may have different interests which conflict. (See Granovetter 1981.) Moreover, even this extreme assumption does not obviate my arguments, as it merely moves the interpersonal comparison-of-utility difficulty to the societal level (Granovetter 1979, pp. 497–98). (2) Imagine that every society's problems are identical and are objectively given by the environment. This seems to be Nolan's tack, as he refers several times to the centrality of the problem of "system survival." With so minimal a version of evolutionary theory it may be that my arguments about current problem solving

¹ These comments refer to Sahlins and Service's notion of "general" sociocultural progress. Contrary to Nolan's assertion that I focus on "specific" evolution, my comments are almost exclusively directed against this notion of "general" advance. There is a good reason for this: specific cultural evolution, as formulated by Sahlins and Service, is equivalent to my notion of "current problem solving" and, for reasons consistent with mine, they do not attempt to define a hierarchy of advancement for this aspect of evolution. They argue instead that "adaptive advance is relative to the adaptive problem. . . . Viewed specifically, the adaptive modifications occurring in different historical circumstances are incomparable; each is adequate in its own way, given the adaptive problems confronted and the available ways of meeting them" (1960, p. 26). Only with regard to general evolution, and "adaptability," do Sahlins and Service believe that societies can be ranked-ordered; thus my arguments against them concern only this portion of the theory (Granovetter 1979, pp. 498–508).

can be made irrelevant. But the cost is to confine attention to a level of analysis of such generality as to be uninteresting.

Nor does this seem to be the usual line of evolutionist thought. White was cited above as defining cultural development in terms of the output of goods and services per capita, and Sahlins's notion of "progress" involves the transformation of energy into material goods and into social, political, and ideological systems. But once such goods and systems are admitted into the collection of items considered to be environmental adaptation or problem solving, it is hard to imagine how one can neglect divergence of priorities among parts of a system's population on where the system's energy ought to be directed. It is certainly true that the difficulties of aggregating individual preferences are severe; but they will not go away because they are denounced. And Nolan's assertion that considering such difficulties will make social science impossible seems premature, as the subdiscipline of welfare economics, devoted mainly to such issues, seems lively and thriving. To my knowledge, its efforts have not to date been attacked as being "intracranial."

The charge of reductionism applies in any case only to the first part of my argument, which rests on difficulties imposed by interpersonal comparisons of utility.¹ The second part—that ranking based on flexibility in meeting future problems is made impossible by the inability to predict what such problems will be—does not require these problems to be defined by individual wants. As I point out (p. 499), even if such problems were uniform for the entire society and somehow "objectively given," these predictability issues would remain. Thus, the accusation of reductionism is irrelevant in this context.

In addition to his disagreements with my theoretical arguments, Nolan asserts that I have misrepresented the ideas of Gerhard Lenski and Jean Lenski on social evolution. Here I must plead partially guilty—in the sense that anyone who tried to get from my paper alone a rounded picture of the Lenskis' arguments (and their development over time) would be misled. My intent was not to give a rounded and developmental account. Instead, I referred to the Lenskis' work as part of a tradition, leading from Veblen, through Forde, Harskovits, Childe, and White, and in many ways culminating in Lenski's 1966 work *Power and Privilege*. This tradition is characterized by a near technological determinism based on the notion that technology determines level of surplus which in turn determines level of social advancement or development.

Nolan believes that my treatment is misleading because Lenski has "always considered the production of *any* surplus to be highly problematic." This is an exaggeration. There has been some evolution in Lenski's thought on this question. In *Power and Privilege*, the overwhelming emphasis is on

a strong connection between technological advance and increase in surplus. There is a disclaimer that "the existence of a surplus is not a function of technological advance alone" (p. 46), but so far as I can tell, the only treatment here of other factors which might generate or inhibit a surplus is contained in a footnote which asserts that had there been no elite, no surplus would have been produced, since "population gains would have kept pace with gains in productivity" (p. 64n.). Successive editions of the Lenskis' *Human Societies* devote increasing attention to this problem. In the 1978 edition, the main emphasis is laid on the role of religion and ideology in motivating the production of a surplus, when one becomes technologically possible (pp. 175, 243). This is a departure from the earlier tradition, and to that extent it may be misleading to classify the Lenskis with it. It is also true, however, that even in the 1978 edition, little attention is given to the conditions under which relevant ideologies successfully compel that production which supports political or religious specialists. The lion's share of analysis still concerns technological factors in production.²

In any case, whether or not I have properly classified the Lenskis' theories on surplus and social evolution is marginally if at all related to the validity of the arguments in my paper. Nolan's assertions about advancement as problem-solving and about the pitfalls of reductionism constitute his main attacks on my ideas; I believe I have shown these attacks to be narrowly based and unconvincing.

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² As Nolan points out, I have mistakenly attributed a 1966 quotation of G. Lenski to the 1974 edition of *Human Societies*. I was alerted to this error by Professor Lenski shortly after publication of the paper and offered my apologies at the time. Nolan's idea that this error fundamentally misrepresents the development of Lenski's thought seems mistaken to me, however.

Review Essay: Fair Science?

Fair Science: Women in the Scientific Community. By Jonathan R. Cole. New York: Free Press, 1979. Pp. xv+336. \$17.95.

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No major shift in social institution is ever judged by the code in place when it starts. No shift ever succeeds except with asseveration that it fully lives up to that existing code and always will. These are two lemmas which, it seems to me, have proved themselves over and over again during the past century in the United States. In *Fair Science: Women in the Scientific Community*, Jonathan Cole is making the classic (and probably unsuccessful) counterattack against a shift in process: holding it up against the existing code, here a work ethic.

That is one important perspective on the book under review. There is another: Who owns the null? The embarrassing truth is that this question identifies social science verdicts on real social changes. There is not enough power either in theory or technique to overturn the null in complex situations. What would be needed is the kind of calibration and control achieved, with great effort and enormous experience, by engineers (see, e.g., the lucid new work of Bennett [1979]). Jonathan Cole, as in all his works as a patriotic citizen of science, takes as his null hypothesis that there is no (unfair) discrimination in science.

Two caveats are in order. The jazzy title catches your interest but is misleading: this book is about the professoriat. Cole has only glancing remarks to make about industrial, or about commercial, or about direct government science, or about contract science for government. The book is about science only if you share Cole's (unstated) conviction that (now, in the United States) science rests almost entirely on the professoriat. And over half the full-time faculty in universities are in fields which Cole includes within science. The fate of science is tied up with that of higher pedagogy. Second, law is in charge now on this topic; as Cole makes very clear from his table of contents on, law is the audience to be wooed and won. I do not know whether Cole has done well, by his or any other lights, for that real audience.

Jonathan Cole has, I think, earned the high encomium that this is a proper *book*. It is not just another research report, thesis, or monograph. You can disappear into it for periods of time, bemused by a byway found in some note and then able, because it is a book, to wander around finding other connecting paths. It is a creature of differing moods and tongues,

not a cabinet neatly crafted together. There is scholarship here, not merely science; he has dug back into a variety of woodchuck holes (both entrances) in a complex web that could only have come from years' worth of poking and note taking. Yet this book is also science and research report and monograph: you can check and cross-check (and fume that he left out *N*'s in this table or that); you are carefully led through subtle teasings out of possible causal chains, chains more subtle than the data can fully sustain; there are fresh, large, systematic and satisfying surveys, surveys which include some ingenious measures.

Jonathan Cole also has earned harsh criticism—perhaps that is another aspect of having authored a proper book. He is self-anointed poet laureate of science. I need only quote: "If science is distinguished by its level of meritocracy, it is also unusual in the extent of its inequality. . . . the simple fact is that there are relatively few scientists who are talented enough to make extraordinary discoveries. . . . Because of the skewed distribution of talent and the universalistic reward system, we get both an approximation of meritocracy and, simultaneously, extraordinary inequality in science" (pp. 6-7). You do see what I mean? Begging the question rather on the grand scale, is it not? (see p. 156 for Cole's authorities).

Perhaps you think me harsh. Surely the passage quoted was just thoughtless boilerplate. No. He goes on: "I asked: Could an institution that so nearly approximates the ideals of a meritocracy fail to approximate such high standards in dealing with women of science? . . . Are there sets of social conditions under which particularistic standards predominate over universalistic ones in science?" (p. 9).

Chapter 6, a qualitative overview of the past half-century's fluctuations, an overview which follows complex earlier multivariate analyses, is a good place to spot the central theme which Cole hammers in again and again. The theme is woven in among a fascinating variety of other speculations in this chapter: "A major theme of this book maintains that sex differences in scientific productivity are a key interpretative factor in understanding the set of relationships between gender and forms of recognition" (p. 239). "In analyzing current data on honorific recognition, I found that scientific productivity completely accounted for the sex differences . . . in each historical period. The modest associations between sex status and honorific recognition are reduced to insignificance. . . . It does suggest, however, that for academe treated as a single community there were few honored men or women, and that gender fails to explain those differences in rewards that did obtain between men and women scientists and scholars" (p. 246). In sharp contrast are his emphatic assertions here and elsewhere that unquestionably there has been discrimination against women (even after all reasonable "controls") in the rank and other features of academic jobs. (Except for salary: "To close the circle, to the extent that salary, especially with the upper ranks, was dependent on research productivity, women were bound to lose out in the race with men" [p. 236].)

For the moment, suppose, as Cole does, that his findings are incontrovertible in this and the main data analyses earlier. I do not think that he has yet taken the time to mull over their significance for institutional change. A note (p. 243) on another study points out that only 15% of women with academic appointments in universities state that they are primarily interested in research but does not point out that the larger percentage of such men so stating (39%) is itself a minority. Surely, current science's ethic of research productivity will be hard put to maintain its place in academe, and that is doubly true if it has been or has been seen as being inappropriately applied to women (and other groups). Women *will* enter academe in greater force: does Cole think that they will permit maintenance of an ethic which in fact would continue to lower their standing—an ethic, which is, at best, hard put to justify itself in view of the *prima facie* purpose of colleges, namely pedagogy?

Cole announces that deep-rooted cultural (p. 284) and social-psychological (p. 130) mechanisms are to blame for lower productivity and that these may weaken in a century or so! Cole is determined to push the gender problem away from the fortified lines of the science institution: "In the final analysis, the real battle for improving the situation for women in science will be fought out before women ever reach the borders of the scientific community" (p. 298, and see p. 81, top). He does not seem to realize that current American science's institutional fort is just another of history's palisades that may no longer be on the main line of march if it refuses to reach out positively to overcome substantive problems. American science, in my opinion, needs shaking up very much more than it needs poet laureates.

Now on to details. The heart of this meaty book is on pages 116–27. These are the main payoffs of the analysis in chapter 4 of how 1,100 scientists in three fields (psychology, biology, and sociology) each assess 60 colleagues (in a rotated panel array so that 600 named scientists are rated altogether), about whom Cole has assembled an impressively complete set of hard information on personal and professional statuses as well as on how often their known stream of articles is cited in others' published work. To my mind, he does a good thorough job of establishing (given his overall "metanull" about science) that women's handicaps in science are (even in numerical extent) much like the handicaps of persons who trained for the Ph.D. away from the major centers: the substantial disadvantages are refracted through the main productivity-reputation mechanism of science as an institution. Cole is soothed by the fact of refraction: I doubt that either Arkansas Ph.D.'s or women are.

Cole's heart is really with elites (see, e.g., p. 285), and I think he is insightful and helpful about the particular problems women may have at the elite level (see, e.g., pp. 131–35). I hope he comes back to do more, for in his own account of science this is the core problematic (which he also emphasizes at what one might call the upper-middle-class level in discussing magnified differences at the tail of the distribution [p. 120]). But

the logic of survey research, his central tool in the main chapters, 3, 4, and 5, impels him to say: "My aim is to probe beyond the scientific elite to see whether women throughout the social system of science are less visible and less esteemed than men in comparable locations" (p. 106).

Detailed criticism there can and will be of his main specific findings. But in fairness to Cole, I think it should be said that, however such re-analyses turn out, he has carried out an unusually varied, open-minded, and thorough analysis. The topic is intrinsically slippery, as are most dissections of institutional physiology. One aspect of Cole's problem is the underidentified triad of cohort-age-period which bedevils demography. Table 4.5 and, in more detail, table 4.8 contain his key assertion about causal mechanism. These tables make a strong case for influences being funneled to an unusually clear and sharp extent through research performance (a normalized average of counts of articles and counts of citations). But, as Cole says in footnotes, there is never an end to such questions as what a priori order variables should take (an excellent and witty recent diatribe on these questions is Leamer [1978]).

For substantive policy purposes, which are important to Cole and account for part of his careful exegeses of tables in the text, I think the real difficulty is how to choose among frames of presentation for the data. This is a problem of almost visual metaphor to which Roger Shepard's smallest-space essays have contributed much. Take as just one example table 4.8, this time as compared with table 6.8. Table 4.8 says that research performance has noticeable impact, even net of certain other causes, upon rank of men's current department (β of 0.15), but none for women's (β of 0.02): a pretty extreme finding that women are not converting conceded contribution into tangible recognition. Turn to table 6.8, which, for a different data set describing three (decade) cohorts, chooses a different representation of the same issue. The correlation of sex with departmental prestige is shown together with reduced value (β coefficient) when research productivity (citations are not available for this population) is controlled. The reductions here are not nearly so dramatic as that in table 4.8 (and it cannot very well be because of omitting citations, since women tend to do better on citations than on volume of production). But there is not inconsistency, even aside from the changes in populations and concrete measures. Table 6.8 gives, for each cohort, the ρ and β of department rank at each of five seniorities; the main point is that in all 14 instances the effect on women is negative and is only somewhat reduced.

One of the most interesting of Cole's accounts gives effects on various cohorts of men as well as women Ph.D.'s—effects on the central measure of productivity—not only of marriage but also of having children. The results (p. 252) sound hopeful: neither marriage nor children hurt men (they may even help them), and they hurt women only slightly. (This finding is featured in the Free Press's news release on the book.) However, Cole has not followed his own intelligent advice (pp. 272–75 in the chapter on affirmative action) to lawyers to watch out for "attenuated

variance." Even in the most recent of his Ph.D. cohorts fewer than half the women (in comparison with more than 90% of the men) are married. What he may have found is that this self-selected set of women, some of them superwomen and some who found superhusbands or marital situations; were able—surely by prodigious efforts—to push along as well as comparable men scientists despite domestic responsibilities.

I shall insert some side criticisms of subsidiary themes before returning to the main perspectives. There are some parts I could do without: only IQ aficionados will be able to take chapter 5 seriously. Surely no one will love Cole's exercise in stereotyping scientists into "prominent," "depraved" (oops—actually "notorious"), "invisible," "esteemed." Someday I hope he or his brother (to whom this book on women's place in science is dedicated) will have the patience for yet a further look at citationing, in connection with these data sets. They could see whether the notable differences among fields in the use of first names versus mere initials of authors could play a role in citations of women's work.

The law should be allowed to shine better than Cole considers it to in his chapter 7 on affirmative action (especially in his devastating commentary [pp. 265–80] on the assumptions made by Ronald Dworkin in an essay on the Bakke case). From Cole's own account, it seems to me the law is much more subtly and accurately attuned to my earlier question—Who owns the null?—than are scientists or Cole in particular. There is a lack of self-skepticism on the part of science-as-an-institution which I think disturbing. Use as template Jonathan Cole's skepticism about the presumed beneficial impact of heterogeneity of intellectual environment (e.g., on p. 271): the very *a priori* quality, the vagueness, and the tie to ideology which he scornfully points to are surely often present in self-accounts of science. The academic machine that is science in the United States in Cole's book (quite realistically) is centered on graduate (and undergraduate) education syllabi whose vociferously enforced sequence and structure have as much tough-minded scientific support as Uncle Ezekiel's claims for brambleberry nostrum. Or take Cole's elaborate development (pp. 74–78) of a "conditional hypothesis of discrimination" to the effect that women's gender is activated detrimentally only when they produce little or no research; on page 119, in the later central chapter on reputation, Cole blandly offers (hypothesis 1) the reverse idea. My point is not that there is direct contradiction (the dependent variables are different, as are the study populations [p. 100, p. 53]) but that there is not enough sensitivity to the importance of the choice of preferred, null hypothesis.

I just wonder whether Cole, despite or perhaps because of his balance and liberal mindedness, quite sees the depth and intensity of the problem. Start with a minor point. He cites (p. 102, n. 38) nursing as one of the few fields where men do not dominate as occupational stars. Close reading of Ida Simpson's recent book (1979) would convince him that even this field is no exception. Now turn to the next page for a most chilling fact.

In one of the many underreported data sets from his three major surveys, Cole finds that half the women psychologists and sociologists included at least one woman among the five persons they named as contributing most to their field in the past 10 years, whereas in those fields only 17% of the men respondents did so. And this is not a by-product of the existence of an agreed tiny elite of men to whom the men confined their naming; 3,000–4,000 different scientists were generated in the choices of five by each of the 300–400 respondents for each science. So it is not just women boosting women, it is men ignoring women. (It would be nice to know whether this effect is attenuated in Cole's other, unreported choices of the three who most contributed to "your specialty.") Cole pays amazingly little attention to characteristics of the 1,000 raters (and the 1,000 non-respondents), and those might affect their ratings—one has to hunt through footnotes to find that only a tenth of the raters are women. We are back to the weary null: if you already *know* as Cole does (aside from occasional pro forma doubts [e.g., p. 165]) that science is a straight shooter, his data are convincing. If not, surely one wonders whether science is not just another big smooth machine designed to be so consistent that it cannot be found out. A person of a certain gender might determine that, to the extent one can, one will leave the burden of proof to science and do one's best meanwhile, despite its vaunted universalistic ethic, to turn to an ethic more suited to the concrete academic task of pedagogy.

Dark thoughts like these stimulate a further escapist thought: could Cole have started his report in just the wrong basic frame of mind? All these fancy path analyses and controlled this and that analyze how women are being kept down fairly, but could the main message be that in absolute terms they are hardly being kept down at all? For example, the average perceived quality of all the women scientists was 4.16 out of 6.00 maximum, whereas the overall figure for men is 4.49. The answer is no. I think Cole did well to refrain from saying "only" 4.49. Either on quality or on the visibility scores the "small" differences between women and men are very close to the gain men make on average between age 35 or so and age 45. This difference seems to me precisely the crucial gain to which people devote the most effort in a career. I see no need for Cole, early in *Fair Science* (pp. 81–83), to explain away colleagues' "common sense understanding."

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Review Essay: Statistical Methods for Categorical Data

Analyzing Qualitative/Categorical Data: Log-linear Models and Latent-Structure Analysis. By Leo A. Goodman. Edited by Jay Magidson. With contributions by James A. Davis and Jay Magidson. Cambridge, Mass.: Abt Associates, Inc., 1978. Pp. viii+471. \$25.00.

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Analyzing Qualitative/Categorical Data assembles 10 articles published by Leo Goodman between 1970 and 1975. Seven of them are on log-linear (including logit) models, two deal with latent-structure analysis, and one concerns the scaling of responses. Three of the papers first appeared in this *Journal* and one in *American Sociological Review*. The remaining six came out in statistical journals. Nevertheless, the style of exposition is fairly homogeneous across all 10 papers, and each of them makes unique contributions even though there is some overlap of content. Readers hitherto acquainted with only some of the articles will, therefore, find the collection most helpful in extending their knowledge. Inasmuch as the papers are reproduced from the original printings, there have been no editorial changes of a substantive kind. However, a nontrivial error in the original version of table 5, chapter 1, has been corrected.

Some readers will have seen the highly critical review of Goodman's volume by Rosenthal (1980) stating a series of purported limitations on the methods and models in the book. I do not address the issues raised by Rosenthal's polemic because it has been rebutted effectively and in detail by Clogg (1980). Unfortunately, owing to a hiatus in editorial responsibility for *Contemporary Sociology*, Clogg's discussion remains unpublished, but I understand that he will supply a copy to any interested reader.

I shall first offer some remarks on the significance of the intellectual development represented by Goodman's material and then make some observations about the pedagogical utility of the book. In the latter discussion there will be brief reference to the two chapters written by other authors—Jay Magidson, who edited the volume, and James A. Davis, whose article from *Sociological Methodology 1973-1974* was the first secondary exposition of Goodman's work on log-linear models.

To appreciate how the stage was set for the introduction of log-linear models into sociological inquiry, the reader would do well to review Lazarsfeld (1968) on the analysis of attribute data in survey research. In that article he provided a parameterization of the saturated model for the three-

way classification of three dichotomous variables. (The saturated model is the one that uses all the available degrees of freedom and thus fits the data perfectly.) The idea was to have three parameters for the three univariate proportions, another three parameters for the cross products (departures from independence in the three two-way marginal tables), and one "homogeneous, symmetric parameter of third level" (p. 423). These, together with the sample size N , would permit the "system" to be "completely summarized by a *new fundamental set* of eight data" (p. 424). The development of this parameterization served Lazarsfeld well as a basis for his conceptual discussion of the "logic of explanation and inference in contemporary survey analysis" (p. 428). But we must note that for Lazarsfeld "inference" meant causal inference, not statistical inference. He put the latter problem aside. Had he tried to grapple with it, he might have seen that his "algebra of dichotomous systems" is not suited to the specification and the statistical testing and estimation of unsaturated models (e.g., those using fewer than the 8 df in a 2^3 table). It is beyond the scope of this review to discuss why Lazarsfeld's parameterization is not a productive one. But we may note that there are usually many different ways to parameterize a given model, so that algebraic consistency of a parameterization does not insure its statistical usefulness.

Goodman's log-linear approach, in contrast to Lazarsfeld's "dichotomous algebra," can be used not only to parameterize the saturated model but also to specify unsaturated models. And Goodman provides methods for assessing the fit of a proposed model and for testing whether the particular parameters included in it contribute significantly to its fit. In comparing alternative models for a multiway contingency table in the fashion suggested by Goodman, the analyst in effect tests all the hypotheses generated by Lazarsfeld's strategy of "elaboration." Indeed, it turns out that there are important hypotheses to be tested whose very existence was not suspected by the exponents of "contemporary survey analysis" prior to 1970. (See especially the section on "Partitioning of Conditional Independence" in chapter 4.) But the new statistical methods have not rendered obsolete Lazarsfeld's discussion of the substantive or conceptual issues in causal interpretation of survey data. On the contrary, they have (at last!) made it possible for a research worker to present adequate statistical support for such interpretation.

The penultimate sentence of Lazarsfeld (1968) was written in good time. Speaking of the comparison of odds ratios suggested by Goodman (1965)—an article I wish had been included in the collection under review—Lazarsfeld stated, "This leads to interesting comparisons between two stratified tables but has not been generalized to the more complex systems that come up in actual survey analysis" (p. 428). By 1970 the generalization was in print. Since 1972 there has been an increasing flow of cogent analyses of "more complex systems" that were simply not possible before.

Goodman's contributions to latent-structure analysis and scaling stand

in a slightly different relationship to prior work. He has taken the latent-structure approach of Lazarsfeld, extended it in various ways, and provided efficient statistical methods for estimating and testing latent-structure models. The importance of the latter contribution is suggested by the remark of Lazarsfeld and Henry (1968, p. 121): "A great deal of imaginative thinking and sophisticated programming is still needed before latent class analysis can be routinely applied to a set of data." Goodman's algorithm is the basis for Clifford Clogg's computer program *MLLSA* (Maximum Likelihood Latent Structure Analysis), a note concerning which appears on page 468. With this program, "routine" application of latent-class analysis is indeed possible. Of wider utility, in my opinion, are the nonroutine variations on the basic latent-class model demonstrated in the several constrained or restricted models discussed by Goodman. (These models likewise can be estimated with *MLLSA*.) Upon reflection it is easy to see that the key assumption of local independence in the latent-class model—that item responses are mutually independent within each latent class—is quite a strong one. With most survey questions there are likely to be several sources of interitem association in addition to the dependence of all items on one or more latent variables. Realistic models must make this structure explicit. Goodman's extensions of Lazarsfeld's work on this topic provide help in contriving such models.

With respect to scaling, Goodman's main suggestion is to introduce an explicit stochastic mechanism into the model proposed by Louis Guttman (1944). As has been noted by critics of Guttman scaling (e.g., Nunnally 1967, p. 66), his model is deterministic and therefore unrealistic for most psychological measurement. Consequently, much attention has been given to the calculation and disposition of "error" in constructing instruments intended to resemble a Guttman scale. Goodman finesses the problem by postulating a latent class of respondents who are "intrinsically unscalable." Their responses are random (in a carefully specified sense), whereas the intrinsically scalable individuals respond deterministically with one of the scale-type response patterns. Heuristically, this model is of great interest. My hunch, however, is that not many sets of questions appearing in surveys are scalable even in the weaker sense of Goodman. His methods provide a formal test of the model, as contrasted with the rules of thumb hitherto applied to assess scalability. With adequate sample size (far larger than the $N = 100$ originally suggested by Guttman), the hypothesis of scalability will usually be rejected unless great care has been taken to define a universe of scalable content and to construct appropriate questions. Exercising such care would be entirely in accord with Guttman's statements on the strategy of scale construction. Even when a set of data is consistent with the hypothesis of scalability, however, the proportion of intrinsically unscalable persons estimated under the model may be disconcertingly high. Perhaps those analysts who recognize the possibility of nonattitudes would not be disconcerted, however. For a comparison of Goodman's work on scaling with other approaches to "error" in a Guttman scale, see Clogg and Sawyer (1981).

When Goodman's methods are applied to old data—as in all his empirical examples—many insights are gained that eluded the original investigators, and often the original conclusions are seriously challenged. Moreover, Goodman takes pains to avoid overlooking plausible alternatives to an attractive model. Although in chapter 5 he gives stepwise procedures for selecting a model that is parsimonious and fits the data well, he warns of the possibility that alternative stepwise procedures or other search strategies may not lead to the same model. Furthermore, inasmuch as there is an ineluctable trade-off between parsimony and goodness of fit, there is often no useful meaning for the term “best model” in regard to a particular set of data. For this reason, I surmise, Goodman never uses that term. (Even so, I find it almost impossible to prevent my students from doing so.) Substantive as well as statistical considerations are relevant to model selection, and no single statistical criterion is decisive by itself. Early users of Goodman's methods were not always sensitive to the nuances of his approach.

The presentation of alternative analyses in these papers does, of course, serve a didactic purpose. For one of the best ways to appreciate the difference between two models is to fit them to the same data. But Goodman often seems to be saying that even the analyst concerned solely with substantive issues would do well to entertain a variety of models, since each of them may yield insights into the structure of the data not forthcoming from the others. In working with “more complex systems” one is indeed often impressed by the subtlety of nature and the poverty of the sociological imagination. In table 4 of chapter 4 Goodman lists no fewer than 57 nonelementary hierarchical hypotheses that can be stated for a four-way table. (Nonelementary hypotheses concern three-way and higher-order interactions; hierarchical models are those that can be specified in terms of the one-way and multiway marginals fitted under the model. Most of the chapters on log-linear models consider hierarchical models almost exclusively.) It takes a great deal of practice in working with these models to gain a feel for their meanings. I suspect that a new style of theorizing will have to be invented before our theorists will be able to make nontrivial but correct predictions of which of the 57 models will be appropriate under stipulated conditions.

A much-vexed topic in sociological methodology that might be reconsidered in relation to log-linear models is the so-called level of measurement. As the title of Goodman's book makes clear, the data to be analyzed pertain to qualitative or categorical variables (attributes, in Lazarsfeld's nomenclature). Almost all the models presented in the book disregard the ordering (if any) of the categories of polytomous variables. (However, chap. 5 is instructive on the use of orthogonal polynomials to distinguish linear from quadratic effects involving an ordered trichotomy.) In one sense, therefore, they assume only the “nominal” level of measurement. But in another sense, what is actually analyzed is a set of counts or sample frequencies. And counting involves use of an “absolute” scale of

measurement, so that in analyzing counts one may claim to be working at the supposedly most exalted level of measurement. More to the point, a log-linear model can be interpreted in terms of the statements it implies about various odds and odds ratios. Thus it becomes meaningful to assert, for example, that the parameters of a certain model imply that the odds on a specified outcome in Population I are, say, 3.7 times as great as in Population II. Or, if A and B are dichotomous variables, one might find that the magnitude of their association increased by a factor of 1.3 between two waves of a panel study. If we think of measurement of association (or, more generally, interaction of variables) rather than measurement of variables as the ulterior objective of empirical research, then work with log-linear models actually involves a scale on which ratios are meaningful and need not be presented with apologies for a primitive level of measurement.

There is, of course, much more to be said about the matter mentioned in the preceding paragraph. And I should emphasize that Goodman's text provides no example of the kind of loose, unguarded assertions I am making in these comments. My main aim is to suggest that quantitative sociology has entered a new era in the last decade, although we are only beginning to glimpse the possibilities for novel and genuinely basic research that have been opened up by the work under review and the much larger body of work on related topics not treated in this particular book. If my conjecture has any basis, we shall have to raise our level of aspiration and standards for acceptable research. One of the clear implications of Goodman's examples is that models with radically different conceptual interpretations may be about equally attractive on statistical grounds, so far as one may judge from a particular sample. Thus, on pages 211 and 294 Goodman gives path diagrams for two models fitted to Coleman's two-wave, two-variable panel data on membership in and attitude toward the "leading crowd" for 3,398 high school boys. In one model there are five (out of the possible six) direct associations linking pairs of observed variables; but in the other model, the observed variables have no direct associations at all, being linked indirectly by way of two latent variables. Both models fit well. Since neither model implies the other, one cannot make an explicit comparison of chi-square values in the manner prescribed for models that are hierarchically related. Nonetheless, both models cannot be "true." It just happens that they give closely similar estimates of expected frequencies in the particular population under study. It would require a much larger sample than 3,398 from that population to achieve rejection of one or both models. Or, one might decide between the models (or in favor of still another model) on the basis of comparative analyses in a variety of populations. The more ingenious we become in devising sophisticated models to reflect fine points of conceptualization, the more often we must find ourselves with this kind of an embarrassment of riches. The classic "need for additional research" will become increasingly urgent. Perhaps we will be led to a rather different allocation of resources among

lines of inquiry such that the really critical hypotheses in a given state of knowledge receive an adequate test. This seldom happens today. A model-rich environment is not necessarily a comfortable one.

There is one major limitation of the examples given by Goodman. He analyzes 10 multiway tables in all. (Five of them appear in more than one chapter, and four chapters give more than one illustration.) Eight are four-way and two are five-way tables. One of the four-way tables has one trichotomous variable; but with this exception all the variables in the illustrative data are dichotomous. It should not be supposed that Goodman advocates the dichotomization of variables in the fashion recommended by some writers on survey analysis. In fact, he was recently moved (Goodman 1979a) to disavow any such view. (His statement also corrects other misconceptions that had arisen concerning the methods treated in this book; it is, therefore, strongly recommended.) The fact remains, however, that the book tells us little about the new issues that arise when polytomous variables are studied. Most of the models presented do generalize readily to this case, and the computer program *ECTA* (Everyman's Contingency Table Analyzer) mentioned on page 468 handles polytomies with no difficulty. But one must go to other literature to learn techniques for deciding whether and how to collapse a polytomous variable, for imposing conceptually interesting constraints on the contrasts between categories of a polytomy, and for exploiting the information concerning order in the case of a polytomy with ordered categories. Three important papers (Goodman 1968, 1972, 1979b) provide foundations for much future work on these topics.

Less consequential is the omission of any discussion of conventions of presentation. Some authors have followed Goodman closely in regard to style and detail of tables. There is nothing wrong with this except for the problem of bulk. After all, when Goodman is teaching us how to use models, he must present much more information than is needed in communications between experienced users. I suspect that the long tables of chi-square values for alternative models will come to be drastically abridged in journal articles. But such material probably should be made available to referees when papers are submitted for publication. The raw data (sample counts) should be published whenever one or a few tables are the focus of the analysis. Some form of archiving is mandatory when there are too many primary tables to publish. Goodman's use of previously published data sets is perhaps the strongest witness I have seen to the fecundity of secondary analysis and the fallibility of primary analysis.

Who will study this volume? My guess is that it will be most useful (1) to advanced graduate students with a commitment to and considerable prior training in quantitative sociology and (2) to mature scholars, including teachers of required courses in statistical methods for sociological research. To use it as a text in such a course, one would have to spend quite a lot of time interpreting it for students. The difficulty is not in the level but in the detail of the presentation. Goodman does not, in general,

include mathematical derivations and proofs. So the problem here is not quite the same as that confronted by the student of mathematical statistics. It is, rather, that most topics are considered from several points of view and each viewpoint is exploited thoroughly. The papers constituting this volume are all original contributions to the literature of statistical methods—not reviews or expositions of material published earlier. Hence they were not written with pedagogical considerations foremost in mind.

Moreover, various kinds of supplementation are needed to round out the presentation of a self-contained unit of study. A qualified teacher can perhaps take care of this in lectures. By way of prerequisites (perhaps needing some review at the beginning of a course) the student should have been exposed to ideas about probability, sampling, and statistical inference, and should have some experience with the binomial, normal, and chi-square distributions, including especially the use of the latter in testing the hypothesis of independence in a two-way contingency table. Any knowledge of regression and analysis of variance will be helpful, though perhaps not strictly essential. Early in a course many students will need instruction concerning the concepts of proportion, odds, and odds ratio; the relationships among these concepts; and the simpler statistical methods for working with them—material such as that covered by Goodman (1965) or more fully by Fleiss (1973, chaps. 1–6). (For various reasons, Davis's chapter, which constitutes part 3 of this volume, does not quite meet this need. It is not free of errors, it introduces a notation not used elsewhere in or outside of the book, and it confuses the definitions of odds and odds ratio. Magidson's chapter, however, could be of benefit to the student who is well versed in ANOVA and regression.)

If, at an early stage of a course on statistical methods for categorical data, there is a treatment of the two-way contingency table, a logical sequel would be an intensive discussion of the three-way table. At this point the instructor could rely heavily on the material in section 3 of chapter 4, being careful to relate to it the important discussion in section 6.1 of that chapter. But in view of the paradigmatic status of the three-way table in survey analysis, some way must be found to relate the statistical models to the conceptual structure developed by, for example, Lazarsfeld (1968) or Rosenberg (1968). To some extent, this can be done by exercises, wherein students may fit the models listed in Goodman's table 3 (p. 119) to some of the illustrative three-way tables given by Rosenberg and then reconsider Rosenberg's interpretations in light of the results.

In the course I teach (to a not very highly selected group of first-year graduates taking it as the second term of required statistics) perhaps a third of the term has gone by before students are ready to study the four-way table, making use of chapter 1 or 3 of Goodman's volume. A course taught at such a pace has no chance of covering all of that volume in a semester. An instructor in my situation who needs a term-sized textbook should consider the one by Fienberg (1980). Haberman's two volumes

(1978-79) are most valuable for the advanced student and research worker. They also provide an introduction to the subject that should be accessible to the statistically mature student without previous exposure to log-linear models, but that student is a rarity in most sociology departments. Unfortunately, their level of intellectual quality prevents a favorable recommendation for any of several other texts that attempt an introductory treatment of this branch of statistical methods.

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Book Reviews

Who Gets Ahead? Determinants of Economic Success in America. By Christopher Jencks et al. New York: Basic Books, 1979. Pp. xv+397. \$17.50.

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Suppose that Christopher Jencks and his associates had not published *Inequality* in 1972 (C. Jencks et al., *Inequality* [New York: Basic, 1972]). Then *Who Gets Ahead?* would be saluted now as a significant advance on our knowledge of the processes of social selection in America. Its objective is to describe the determinants of economic success. What are the relative effects of family background, cognitive skills, length and type of schooling, race, and personality on subsequent occupational status and earnings? The description of these determinants is more accurate than any provided by earlier works for three reasons: a broader data base, more successful reduction of measurement error, and more sophisticated use of multiple regression and path analysis to reveal indirect as well as direct effects. This gives us a better understanding of mechanisms or channels through which the various elements of an individual's inheritance and upbringing combine to place him more or less advantageously in the labor market.

Of course, it is neither a complete nor a completely accurate description. The data exclude women and deal only with men aged 25-65 who were not in schools, armies, prisons, or hospitals or unemployed. The data from five national and six special purpose surveys have limitations and inconsistencies which defy the most painstaking reconciliations of secondary analysis. Nevertheless, the measured associations are convincing, and what is measured is more comprehensive than anything previously published. For example, the use of data on brothers, pioneered by Blau and Duncan and also used in our comparable British study (A. H. Halsey, A. F. Heath, and J. M. Ridge, *Origins and Destinations: Family, Class, and Education in Modern Britain* [Oxford: Clarendon Press, 1980]) enables the variable "family background" to refer not only to "demographic" or "material circumstance" variables, but also to those dimensions of family climate which the French have in mind when they speak of "*la famille educogène*."

But *Inequality* did appear in 1972, and was much acclaimed and much criticized. It was subtitled *A Reassessment of the Effects of Family and Schooling in America*. Essentially, *Who Gets Ahead?* is a reassessment of that reassessment which results in a closer resemblance to our previously familiar picture of the social determinants of occupational success. It turns

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out, in short, that family of origin, schooling, measured intelligence, and personality put a heavier, and "luck" a lighter, stamp on a man's economic prospects than readers of *Inequality* had been invited to believe.

Cynics in the modern antisociological fashion will doubtless express ironical satisfaction that the \$400,000 (Jencks et al.'s estimate) spent on this new analysis has purchased a restoration of the plain man's picture of the universe we know and do not necessarily love. But sociologists will do well to resist this overreaction, as they resisted when previously encouraged toward nihilistic despair about the possibility of educational paths to equality. The more sensible judgment is that the Jencks team is to be commended for taking criticisms of *Inequality* seriously, for defining more rigorously the variables of family, personality, and schooling, for using these definitions in a herculean effort of reanalysis, and for adding to the value of the answers by refining the questions. That way lies the progress in social science to which the profession aspires.

The restoration is, in any case, by no means complete. The old figure for the percentage of variance in occupational status explained by family background was 32; the new figure is 48. The old figure for schooling was 42%; the new is 55%. Combining the variables of family background, test scores, years of schooling, and personality traits, it now appears that the characteristics which people take into the market on first entry explain 55%–60% of variance in adult occupational status and 33%–41% of variance in annual earnings.

Thus the general thrust of the *Inequality* argument is not blocked by recalculation. For example, whereas in *Inequality* the expected difference between the occupational status of brothers was 82% of the expected difference between pairs of unrelated men, the new percentage is 72. Clearly the revised figure does not afford dramatically enlarged scope to the social engineers.

Yet readers are likely to notice a marked change of tone between the two publications. This is partly because *Who Gets Ahead?* has a lot less to say about public policy. More fundamentally, it is due to a shift in statistical presentation: the earlier book focused on within-group differences (variance); this one focuses on between-group differences (means).

Inequality attacked the utopian hopes of the Great Society for widespread reform through educational engineering. The dramatic effect of that essay in demolition, however, rested largely on its demonstration of the limits of social action. People with similar family backgrounds, test scores, and schooling subsequently scattered themselves over the range of occupational statuses and incomes to about three-quarters the extent of the scatter of people in general. In that sense American society was an open lottery. But by the same token, the scope for social engineering on behalf of a principled allocation of life chances was woefully small. If schooling explained only 12% of the variance in men's incomes, then complete equalization of schooling would at best reduce income inequality by only 12%.

The critics rightly complained that to assume it is possible to change the value of one variable without changing the totality of relations between

variables in a system of plural causation is statistically convenient but sociologically invalid. If America gave everyone the same schooling, it would, in the process, completely change the class structure, the labor market, and indeed its whole social self.

In *Who Gets Ahead?* these objections are accepted. Moreover, Jencks et al. also accept criticism of the undue importance they previously attributed to luck, narrow their definition of the variables listed above, and discuss the implications for the labor market of a radically equalized distribution of human capital. The old conclusion, nevertheless, unhappily remains. Past efforts to equalize through education have been ineffective. The authors might also have mentioned the arguments adduced by Lester Thurow (*Generating Inequality* [London: Macmillan, 1976]) that these policies have been expensive and that such effects as they have had will be arrested in the 1980s. The problem of equality in America will therefore turn increasingly on the distribution of market incomes. And that is what Jencks and his colleagues have been arguing all along.

Origins and Destinations: Family, Class, and Education in Modern Britain. By A. H. Halsey, A. F. Heath, and J. M. Ridge. Oxford: Clarendon Press, 1980. Pp. ix+240. \$29.50 (cloth); \$12.95 (paper).

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This monograph is one in a series published by the Oxford Social Mobility Project on the social origins and educational destinations of males in England and Wales during the course of this century. *Origins and Destinations* builds on and extends the earlier work of D. V. Glass (*Social Mobility in Britain* [London: Routledge & Kegan Paul, 1954]) at the London School of Economics by incorporating American approaches to occupational mobility initiated by Peter M. Blau and Otis Dudley Duncan (*The American Occupational Structure* [New York: Wiley, 1967]). The subsample analyzed consists of 8,529 men, aged 20-60 in 1972, who were resident in England or Wales at the age of 14 and at the time of the survey.

This book deals exclusively with educational attainment and equality of opportunity in British schools. The major questions concern changes in access to and the distribution of schooling during the 20th century and the determinants of educational achievement in terms of class background, intelligence, and "cultural capital." A. H. Halsey et al. document the expansion of educational access throughout the period, showing that each successive age cohort profited from a higher probability of entering and completing the various levels of education. This expansion is accounted for by two factors: the steady increase in the amount of schooling available, and the fluctuations in the size of cohorts that affected the demand for education. Only the youngest cohort, those born between 1943 and 1952, experienced a leveling or a decline in the probability of entry to selective school-

ing, due to the very large number of baby boom children competing for a limited number of places. For this cohort, selective schooling became more difficult to obtain, and the general trend of increasing educational opportunity for all social classes halted.

The educational experiences of individuals are portrayed as a set of pathways, or routes, that fork and sometimes reconnect at later points. The selection of a specific path is highly consequential for later educational opportunities. The authors endeavor to map these paths at four critical junctures. The points selected are: (1) attendance at a private or state primary school, (2) transfer to a secondary school at age 11 or persistence toward the terminal primary degree, (3) the type of secondary school chosen and the decision to remain in school beyond the minimum school-leaving age of 14 or 15, and (4) the decision to continue into some form of postsecondary education. In a brief review, it is impossible to do justice to the variety and richness of the analyses reported. Each juncture is fully explored, a number of alternative hypotheses drawn from both the British and American literature are tested, and comparisons are drawn. We will focus on two of the major themes running through the book that seem particularly significant now: the role of the independent (or private) school in perpetuating inequality, and the effect of educational reform on access to postsecondary schooling.

The authors are understandably concerned with British independent schools, which Richard H. Tawney has termed an "educational monstrosity" that perpetuates "the division of this nation into classes of which one is almost unintelligible to the other" (*Equality* [London: Allen & Unwin, 1931], p. 145). This "elite" path of private schooling is, however, chosen by very few Britons. The proportion of males attending private primary schools never exceeded 7.2% for any one cohort, and for the sample as a whole the proportion was less than 5.8%. The proportion of the sample attending private secondary schools was 6.5%. This figure is considerably below the proportion of children attending private schools in America, which is estimated at 10%. Until recently, the American private school has been ignored in the literature, with very few authors arguing that it could play an important role in transmitting inequality. Curiously, the British seem not to have harbored such illusions. As Halsey et al. point out, the independent schools constitute a significant educational alternative for upper-class parents desirous of maintaining status intergenerationally.

The major determinant of attendance in independent schools is having a parent who attended an independent school. However, given the expansion of secondary schooling and the existence of prestigious grammar schools, the majority of students attending a selective secondary school in every cohort were from families in which neither parent had previously attended. Among the boys attending technical schools 80% came from families with no tradition of formal academic training, as did two-thirds of those at grammar schools; in universities, 88% of the students were from families in which neither parent was a university graduate. The chances of success for these first-generation entrants were much like those for students from

more educated families, despite the lack of parentally transmitted "cultural capital"—a term used by Pierre Bourdieu in "Cultural Reproduction and Social Reproduction" (in *Power and Ideology in Education*, ed. Jerome Karabel and A. H. Halsey [New York: Oxford University Press, 1977]). The authors argue quite persuasively that family factors reflecting Bourdieu's notion of cultural capital influence selection but not retention or successful completion once students enter a level of schooling.

The Education Act of 1944 established universal free secondary education in Britain and replaced the Special Place examination with the 11-plus. These legislative reforms were designed to increase access to secondary schools and to decrease class privilege; a tripartite state system of secondary education consisting of elite grammar schools, secondary modern and technical schools, was set in place, in contrast to the comprehensive high school developed in the United States and advocated by the Labour party. Independent schools, although widely criticized, were not affected by the reforms.

How did these legislated changes affect the social composition of secondary schools and the opportunity structure for children from different backgrounds? Not very much, argue the authors. The number of grammar schools increased enormously, but not at the expense of the independent schools. Children from professional or service families were not excluded from secondary education in favor of working-class children. Access was clearly increased, and the educational chances of working-class children correspondingly improved. However, the determinants of attendance at more selective secondary schools did not change perceptibly. Whether this reflects long-term commitments to a meritocratic system or the robust stability of class differences in Britain is less clear. Test scores and family background both predict educational success in Britain, as in the United States; similarly, educational reform does not seem to dent the inequality of outcomes in either country.

This study is not, properly, a replication of either Glass's work or that of Blau and Duncan, although it shares features with both. In the first place, it does not deal with occupational mobility or other outcomes after the completion of schooling. Before concluding that educational reform in Britain has had little impact on the stratification system, one would like to examine other outcomes. Second, the sample is restricted to white males, so an analysis of differences by sex or race is ruled out. It is not clear whether data on racial background were collected, although race was not tabulated in this report. A comprehensive assessment of inequality of opportunity should surely include the variables of sex and race. Finally, it is difficult to compare specific inferences or conclusions with those drawn in the American literature. Class differences are coded and tabulated in non-comparable ways, and the path analyses do not present unstandardized coefficients. It would be useful to rectify these matters in articles or forthcoming publications, since such additions would heighten interest. However, as it stands the book represents a considerable achievement and a lucid contribution to our knowledge about schooling and stratification.

Class Structure and Income Determination. By Erik Olin Wright. New York: Academic Press, 1979. Pp. xxvi+271. \$21.00.

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Recent sociological research on stratification is characterized by an emphasis on the structural determinants of economic inequality and by a revival of interest in Marxist analysis as a way of conceptualizing these structures. Erik Olin Wright is concerned with both of these issues, and *Class Structure and Income Determination* has a dual purpose: for Marxists, this research represents an investigation of the nature and economic consequences of positions in the social relations of production in advanced capitalist societies; for non-Marxists, this study demonstrates that a particular operationalization of class is important for explaining income inequality. In view of the timeliness and importance of these issues, this book, portions of which have appeared in various journal articles, is likely to generate considerable debate and research regarding the links between structural positions and inequality.

The book is divided into two parts. Part 1 (chaps. 1-4) presents Wright's theoretical perspective on class and how class is related to income inequality. In chapter 1 ("What Is Class?"), he distinguishes the Marxist conceptualization of class from that rooted in other theories, defining class as "common positions within the social relations of production, where production is analyzed above all as a system of exploitation" (p. 17). Chapter 2 provides a conceptual map of classes in advanced capitalist societies, based on a Marxist framework. In chapter 3, Wright attempts to reconstruct a Marxist theory of income determination from the logic of Marxist methodology and to contrast this theory with human-capital and status-attainment approaches, which ignore social relations of production. This discussion leads to the formulation of a variety of hypotheses in chapter 4. His central thesis is that class location mediates the income-determination process—that is, the determinants of income interact with class position.

The second part of the book (chaps. 5-9) presents empirical analyses designed to test some of these hypotheses; for the most part, these analyses are based on individual survey data collected by the Institute for Social Research at the University of Michigan. Chapter 5 ("Class and Occupation") attempts to convince non-Marxists that class is worth studying by showing that even a crude measure of class has a strong effect on income that is independent of occupational status. Chapter 6 ("Class and Income") examines how class structurally mediates the income determination process. Two main findings emerge from this analysis. First, employers and managers receive greater incomes than workers, even when a variety of individual characteristics are controlled. Wright interprets these income gaps between classes as reflecting consequences of exploitation. Second, the returns to education are greater for some classes (e.g., managers) than for

others (e.g., workers). These education-income interactions provide evidence that the processes of income determination are indeed "different" for dissimilar classes; these interactions are furthermore assumed to reflect structural properties of the classes themselves, and not simply attributes of individuals. Chapter 7 elaborates on the key manager-worker differences using data from the Hierarchy in Organizations study. The final chapters of analysis ("Race and Class," "Sex and Class") examine black-white and male-female income differences within classes. A major finding here is that group differences in the income returns to education are substantially reduced within class positions.

This book makes a number of significant contributions. Wright's theoretical discussions of the Marxist view of class, the types of class positions in capitalist societies, and the Marxist theory of income determination are well written and extremely useful. Further, his use of conventional econometric techniques for assessing the relationship of class to income inequality contributes considerably to bridging the gap between the Marxist theoretical perspective and the vast body of quantitative research on stratification. The book demonstrates convincingly that class "matters" for income inequality; thus, even if one does not adopt Wright's Marxist theoretical framework, it is clear that one must incorporate measures of position in the social relations of production in adequately specified models of income determination. For Marxists, this book also outlines a research agenda for class analyses of inequality. Wright suggests some needed extensions: the need for better measures of class position, historical and comparative analyses of class structures, and analysis of the relations between class structure and the forms of organization of capitalist production.

Nevertheless, one wonders just how much a strictly Marxist interpretation of inequality can contribute to the development of a comprehensive structural theory of income determination. Wright's adherence to a Marxist class perspective often leads him to ignore how other perspectives may complement his theory. The status-attainment approach, for example, attempts to model, or describe empirically, how people are allocated to unequal jobs. This model is compatible with various theoretical perspectives, including Marxist theory, and is complementary to Wright's concerns about why jobs are unequally rewarded. In addition, Wright argues that human-capital theory is inadequate because it emphasizes individual characteristics as determinants of income inequality. But neoclassical economic theory is not reducible to human-capital theory, and concepts such as productivity and supply and demand need to be considered as well. Also, Wright largely ignores the now-vast literature on economic and labor market segmentation, which also seeks to explain how and why jobs are unequally rewarded. This omission is especially unfortunate in that much of this literature also derives its inspiration from Marxist theory, particularly the work of radical economists on dual economy and dual labor markets.

There is furthermore a considerable gap between the theory Wright proposes in the first part of the book and his empirical analysis in the second

part. Much of the problem here centers on the notion of "exploitation" as the explanatory mechanism for why managers, for example, obtain greater incomes than workers. His findings are also compatible with the view that managers exhibit greater marginal productivity and receive greater rewards on that basis. The same problem applies to his interpretation of the greater income returns to education for managers than for workers. Wright regards this as due to a "social control imperative" on the part of employers with respect to managers. Yet this is also consistent with arguments that employers must set up hierarchies for other organizational and technological reasons. Wright's interpretations may well be correct; the point is that he does not provide compelling evidence for his exploitation argument, due partly to his inattention to plausible alternative hypotheses. In addition, Wright's near-exclusive use of individual survey data does not permit a strong test of his exploitation argument. More appropriate to his theoretical concerns would be data obtained from firms, which constitute the basic units that create surplus in capitalist societies. While a sample of firms would not necessarily permit one to adjudicate between social control and other organizational/technological arguments, it would provide a better test than individual survey data.

In sum, this book is important for a number of reasons, especially for its presentation of the Marxist theory of income determination. It will undoubtedly generate considerable debate regarding the correctness of the argument and the appropriate kinds of data that are required to test Marxist theory. However, it is doubtful that Marxist theory alone is sufficient to account satisfactorily for structural determinants of stratification; insights must also be incorporated from non-Marxist organizational theories and work in institutional economics. This is a difficult theoretical task, as it requires the incorporation of concepts from both class and organizational paradigms, as well as insights from work on individuals' socioeconomic achievement. Despite its limitations, Wright's book will no doubt play an important part in the synthetic effort required to develop such a comprehensive structural theory.

Life Chances: Approaches to Social and Political Theory. By Ralf Dahrendorf. Chicago: University of Chicago Press, 1980. Pp. ix+181. \$15.00.

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By his own admission, Ralf Dahrendorf has been relatively silent in theoretical social science since the publication of *Class and Class Conflict* (Stanford, Calif.: Stanford University Press, 1959). With *Life Chances*, which is actually an updated and expanded collection of essays and lectures, Dahrendorf once again bursts onto the theoretical scene.

As the title indicated, the book is about a theory of life chances. For Dahrendorf, life chances are a function of "options" and "ligatures." By options, Dahrendorf means opportunities for choice available to individuals. Ligatures refer to the attachments or bonds that link individuals to society. Dahrendorf argues that we need to include both options and ligatures in the life-chances concept, since options without ligatures are meaningless while ligatures without options are oppressive.

In preindustrial societies, people generally have strong social linkages but few opportunities for individual choice. According to Dahrendorf, these conditions tend to produce oppressive forms of group conformity. Industrial societies, in contrast, offer many opportunities for individual choice but few opportunities for individuals to develop strong social bonds. These conditions tend to foster widespread alienation and anomie as individuals are set adrift in a sea of meaningless choices. Ligatures and options can vary independently, although the tendency in modern industrial societies has been to expand the horizon of individual choice at the expense of social bonds.

Dahrendorf sees the social structure as arrangements of life chances. The individual confronts this structure as a series of constrained options that form a kind of life-cycle trajectory. Where you are at any given time depends on where you started plus what choices you made in the past. The key point is that the options are not open but are systematically constrained.

The ligatures part of this life-chances hybrid is more problematic. Ligatures are the stuff of social cohesion. They are the connecting threads that bind individuals to society. Dahrendorf offers forefathers, history, home, faith, country, and community as examples. According to Dahrendorf, people become linked to society by virtue of occupying positions and roles. Once linked, ligatures "give meaning to the place which the individual occupies" (p. 31).

It is not clear from Dahrendorf's discussion exactly what it is that individuals are linked to. It is not roles and positions; rather, roles and positions provide the opportunities from which linkages may emerge. Dahrendorf wants to make a distinction here between opportunities for meaning and the experience of having meaning, but he has no theory about the mechanisms which translate the potential into the reality. Nor does he have a theory about the process by which potential meaning is stored in roles and positions. This is an important, yet underdeveloped, part of the life-chances concept. It raises other unresolved issues, such as the construction and internalization of meaning, meaning conflict, and the role of ideology in the legitimation of a structure of individual choices.

Dahrendorf admits that ligatures would be difficult to measure. He acknowledges that some general estimate of how well a person is linked to society would have to include both the number and intensity of ligatures. Presumably, ligatures could also vary by type. Individuals, for instance, could develop linkages through close interaction with others (e.g., family), from activities in which they engage (e.g., job), or from ideas which they hold (e.g., faith). Used in this way, the notion of ligatures covers a multi-

tude of sins. Although intuitively appealing, it is probably too broad a concept to be of much practical value.

Dahrendorf claims a great deal for the life-chances concept. He asserts that it can provide (a) a way to make sense of history, (b) a way to link the substantive and the theoretical in the social theory of change and conflict, and (c) a way to connect theory and praxis.

The strongest argument in the book proposes the use of the life-chances concept as a way to make sense of history. Dahrendorf agrees with Karl Popper that history has no inherent meaning. But he maintains that it is possible and even desirable to give history meaning, and he proposes the life-chances concept for this task. For social scientists, the problem in doing history is to discern the direction of social processes and to indicate what this means for people. The concept of life chances does this while avoiding teleological assumptions about the inevitability of progress. Dahrendorf makes one normative caveat here: having more life chances is better than having fewer life chances. Progress is then defined as growth that comes from the realization of life chances. Dahrendorf does not argue that life chances will produce growth; rather, he suggests that growth is more probable where more life chances are available to more people. History, then, involves the ebb and flow of changing life chances and is not merely the unfolding of new combinations of old elements.

Dahrendorf's argument that the life-chances concept may be the key to understanding social change and class conflict is less convincing. In this regard, he asserts that "social conflicts are about life chances" (p. 61). The problem with this is that conflicts develop over resources and meaning, not over options and ligatures. It would probably be more accurate to state that options and ligatures define the conditions under which conflict over resources and meaning takes place. While Dahrendorf is correct in criticizing Marx for developing a teleological theory of history, the traditional Marxian categories are still more useful for understanding the dynamics of group conflict than the somewhat more individualized notion of life chances.

The last part of the book concerns the relation between theory and praxis, broadly defined. For Dahrendorf, the ideal of praxis is liberty. Dahrendorf defines liberty as life chances plus civil rights, combined with a philosophy that places the individual first and a humility that recognizes that no one individual has all the answers.

According to Dahrendorf, the end of a period of social-democratic consensus has been reached. The consensus has died because the grand liberal program of extending citizenship rights and decentralizing power has largely been achieved and no new program has surfaced to replace it. As a result the political left has become anomic, demoralized, and fragmented.

Dahrendorf sees the danger facing the political left as that of taking its program of change too far. For example, segments of the left have gone from demanding equality of opportunity to demanding equality of condition. Dahrendorf argues against this envisioned socialist utopia on the grounds that inequality is necessary to present those at the bottom with a

concrete vision of what could be (i.e., hope) and that hope is necessary for progress, which itself is the only hope for "escape from the condition of oppressive drabness and drab oppression" (p. 123). Similarly, Dahrendorf wants to place limits on participatory democracy. He sees the participation of "all in everything" (p. 132) as meaningless. The issue is the right of participation, not the requirement of participation. Dahrendorf's program for praxis, then, is to construct a liberal society that promotes the greatest number of life-chances for the greatest number of people, while maintaining at all times the integrity of the individual and his right (within limits) to be different.

There will be those who take issue with Dahrendorf on the extent to which the liberal program has, in fact, been achieved in the industrialized West. Others will surely object to Dahrendorf's critique of revolutionary socialism. Still others may object to the life-chances concept itself. Nevertheless, the book succeeds as an intriguing theoretical reflection of the human condition. It deserves wide readership and serious discussion.

The Literacy Myth: Literacy and Social Structure in the Nineteenth-Century City. By Harvey J. Graff. New York: Academic Press, 1979. Pp. xxii+352. \$27.50.

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Fifty years ago, the authors of *1066 and All That* warned us that history is not what we thought but what we can remember. Now in *The Literacy Myth*, Harvey J. Graff reveals that literacy is not what we thought either. In this subtle and densely argued exercise in social history, he effectively demolishes the "myth" of literacy in two senses: "literacy," he contends, is not a unitary property that the individual enjoys or lacks, but is rather a shifting, complex pattern of characteristics and processes; second, and more important, Graff demonstrates that literacy as such has not been (as it is often purported to be) the magic key to individual success in life or to societal modernization and civil harmony. Literacy is shown to be intricately entangled with other features of the social environment and personal circumstance, many of them more potent than sheer mastery of the three R's in shaping individual fate and the contours of social order.

A firm empirical base for the book's revisionist thrust is found in detailed records of mid-19th-century life in three Ontario cities: Hamilton, London, and Kingston. The author meticulously examines the evidence for achieved literacy in these Canadian populations and explores the manifold ways in which literacy and illiteracy are related to other determinants and consequences of life experience. In his careful attention to what actually

happened to individual men and women, he anchors his broader theses about literacy's true meaning in the social structure. To render the matter perhaps too baldly, literacy wasn't and isn't what it's cracked up to be. Without denying that the ability to read and write and enumerate can influence life chances and social organization, Graff shows clearly that the claims by educators and other guardians of community virtue for the efficacy of literacy were stunningly overwrought. In the society of Upper Canada in 1850-70, there is persuasive evidence that structural features outweigh literacy or its lack in determining life chances; ascriptive characteristics, such as father's status, ethnicity, and race, consistently exert more influence than does literacy. In summary: "Social thought and social ideals have, for the past two centuries, stressed the preemption of ascription by achievement as the basis of success and mobility, and the importance of education and literacy in overcoming disadvantages deriving from social origins. In the three cities, in 1861, however, ascription remained dominant. Only rarely was the achievement of literacy sufficient to counteract the depressing effects of inherited characteristics, of ethnicity, race, and sex. . . . Literacy, overall, did not have an independent impact on the social structure" (p. 114). And if literacy is no guarantor of occupational attainment, wealth, or secure social status, so illiteracy in the Canada of the day is not utterly damaging. We find illiterate individuals coping well: "Generally assumed to be disorganized, unstable, irrational, and threatening to social order, without schooling their plight was assured. Illiterates in the three cities, contrary to the stereotypical expectations, proved themselves to be far more adaptive, integrated, and resourceful in confronting the urban environment . . ." (p. 115).

To complement the fine-grained examination of literacy's role in the individual life course, Graff offers a more general analysis of schooling in relation to social structure. Here again the discussion convincingly subverts received theoretical assumptions and conventional wisdom. Notably, the salience of education in industrialization and modernization is questioned; the relationships are shown to be altogether more complex and ambiguous than has usually been assumed. Literacy is not a prerequisite for industrialization in any direct linear fashion. Indeed, the history of industrialization in Britain supports the reverse interpretation, with industrial development preceding the achievement of widespread literacy. Where the growth of primary education can be construed as smoothing the way for the factory system, as in North America, Graff asserts that its importance lay more in its functions as an agency of social control and moral discipline than in its strictly cognitive tutelage: "The provision of mass schooling; the working class' acceptance of it, though a questioning one; and universal public education all served this direction: promoting discipline, morality, and the 'training in being trained' that mattered most in the creation and preparation of a modern industrial and urban work force" (pp. 232-33).

Throughout the 19th century, literacy was viewed as an adjunct to morality, to obedient citizenship and subordinate routinized workmanship.

Cognitive skills were not highly valued (and were often suspect) in isolation from these other social imperatives. And surely many observers would agree that contemporary education retains much the same guise.

In a splendid final chapter, "Literacy: Quantity and Quality," Graff poses significant issues about the nature of education in life history and social structure. He urges that we conceptualize literacy more rigorously; that we distinguish among its several levels and modalities; that we separate its influence on individuals from its influence on social order. Perhaps the most telling indication of his achievement is that any reader of this book will be forced to think about literacy and education in new ways.

Growing Up American: Schooling and the Survival of Community. By Alan Peshkin. Chicago: University of Chicago Press, 1978. Pp. viii+256. \$12.95.

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Growing Up American is an eloquent book. In it, Alan Peshkin has captured the meaning of schooling in and for a small rural midwestern town to which he gives the pseudonym of Mansfield. Mansfield High School becomes the central actor in the drama. Its ninety-second graduation ceremony, according to Peshkin, is symbolic: "By annually repeating this educational exit ritual, Mansfield High School demonstrates its capacity to respond to the schooling needs of its support community. Indeed, the ritual also reconfirms the community's capacity to empower the school to respond to its needs. Only viable communities continue to meet this test of potency in a ceremony that symbolizes the success of both school and community" (p. 192). Leading to that interpretation are accounts of the community which accent the values of belonging, nurturance, and intimacy. The school board contains men who went to the Mansfield schools, whose children attended those schools, and who now sit as educational guardians of the community. In turn, over the years they have been careful to hire superintendents who are "country." The superintendent, sharing the values of the community, recruits and hires teachers with small-town backgrounds who share Mansfield's beliefs and values and who effortlessly give the community what it wants. The youngsters—in class, in extracurricular activities, and in their out-of-school life—move through the process, take part in it, value it, and ultimately (sometimes while still in high school) marry and have children who will be part of the same scheme.

Methodologically, Peshkin proceeded as a community anthropologist. He went looking for an "American village"—small, but with a high school, rural, agricultural and without industry, and agreeable to his conducting a study. He and several assistants lived "on and off" for two years in Mans-

field. His intent was neither evaluation nor reform. Although he was interested in the relationship between the high school and the community, the initial focus was not tightly constrained: "In any event, without an overriding central issue or perspective to guide the study, I cast my net widely, committing myself to explore all aspects of the community and its school slowly, patiently, and exhaustively" (p. 4). Teachers and board members are presented as "composite persons." Participant observation, documents, questionnaires, formal interviews, and an appendix with 32 tables operationalize Peshkin's anthropological methods. In fact, all of these methodological tools are problematic and their uses under debate in the methodological literature; Peshkin's practical resolution of such dilemmas represents a refreshing eclecticism.

Careful description and commonsense themes presented in ordinary language comprise the results. Peshkin does not reach for theory in the grand or formal sense. Malinowski, Lévi-Strauss, Geertz, and their critics do not appear in the book's references. The sociologists do a little better—here he cites Homans, MacIver, Nisbet, Vidich, and Bensman. The educationists cited—Barker and Gump, Cronin, Cusick, Holt—represent diverse theoretical positions; most of them focus on high school and adolescence. The topics, chapter by chapter, cover the community, the school board, the teachers, the students. Throughout, Peshkin describes events we all know about; he isolates and highlights them. Football games entice over a third of the town; they are a major social event. The curriculum differentiates into "book subjects" and "practical courses." Teachers with small-town backgrounds occupy Mansfield classrooms. Classroom and extracurricular activities have an ideological overlay which is in some instances more important than their content. Examples are a Teens for Christ group and a Future Farmers of America club that meet after school in the high school. Implicitly, the analysis has a holistic, almost functionalistic, quality. Evaluations by the North Central Association of Colleges and Schools, school board membership, recruiting and training of teachers, educational programs, etc., all seem interdependent and congruent with community values.

For this reviewer, the interpretative high point of the monograph is the analysis of conflicting values presented in the last chapter, "Paradise Maintained?" Peshkin puts Mansfield on the horns of a series of value dilemmas: personal identity versus intellectuality, intergenerational stability versus maximizing academic achievement, school-community harmony versus national ideals, and a balance of academics, athletics, and extracurricular activities versus school consolidation. Localism and sense of community are writ large. Finally, he poses like dilemmas for all American communities—fundamentalist Kanawha County, white South Boston, black and Hispanic New York City, and Amish Wisconsin—and for the larger population surrounding these communities. What kind of society does America want? What kind of schools should serve the "communities" of America? Mansfield and its high school become a vivid, eloquent illustration around which discussion, debate, and choice can occur.

Tasks and Social Relationships in Classrooms: A Study of Instructional Organization and Its Consequences. By Steven T. Bossert. Cambridge and New York: Cambridge University Press, 1979. Pp. xi+119. \$14.95 (cloth); \$4.95 (paper).

Learning Lessons: Social Organization in the Classroom. By Hugh Mehan. Cambridge, Mass.: Harvard University Press, 1979. Pp. xvii+227. \$15.00.

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Doing research on classrooms may be just too easy. These accessible, familiar settings, with their relatively simple roles and manageable size, give the researcher a confident sense of fully knowing a little social world. But they also create a problem. It becomes very hard to say anything new. Those who would do exciting research on classroom life need sharp theoretical questions which can cut through the layer of familiarity to make us see freshly what we seem to know so well.

Both books under review strive for a new theoretical angle on classroom life. Hugh Mehan's *Learning Lessons*, grounded in ethnomethodology and sociolinguistics, offers a line-by-line, gesture-by-gesture analysis of the interactional organization of classroom lessons. Steven T. Bossert's *Tasks and Social Relationships in Classrooms* provides a new take on the old problem of the effects of differing teaching styles. Drawing on the sociology of work, Bossert asks how the task organization of a classroom affects the relations of teachers to their students and of students to one another. Yet despite the advantages of distinctive theoretical vantage points and observational approaches that focus on the nitty-gritty of classroom life, both books suffer from a kind of flatness, as if the search for a new angle of vision had eclipsed a view of the complex dynamics of classroom life.

Tasks and Social Relationships in Classrooms is based on field observation of four classrooms. A year of observing two third-grade classrooms whose teachers seemed to differ in teaching style led Bossert to spend a second year observing the two original classrooms and two fourth-grade classrooms which seemed to differ along the same dimension. Bossert identifies the contrast between the two kinds of classrooms as a difference in the tasks students undertake and the patterns of evaluation and authority the tasks imply. He describes three kinds of task organization. "Recitation" constrains students to work simultaneously at the same activity and makes the teacher the center of classroom order. Children's performances are necessarily "very public" and "can be easily compared" (p. 44). Recitation requires uniformity and discipline, and it leads teachers (and children themselves) to emphasize differences in achievement among children. "Class tasks" (e.g., workbook assignments) also require all children to do the same work, but "because the tasks are done independently or in small groups, neither all class members nor the teacher can constantly observe each other

while they are working," and performance is thus "less public than in recitation" (p. 44). During "multitask activities" (independent reading, individual or small group projects, arts and crafts), different children work on different tasks. Such tasks are more likely to be freely chosen by the children, and during multitask activities it is difficult for the teacher and other students to "observe the task performance of every pupil" (p. 45) or to compare the work of different children.

Bossert argues that the task organization of a classroom has important consequences. Recitation leads teachers to rely more heavily on formal authority than on informal rewards and sanctions. More interestingly, when teachers use recitation they give disproportionate attention and help to high-achieving pupils—those pupils who help them get through the lesson by supplying right answers or serving as examples for other students to emulate. Teachers using multitask activities, on the other hand, give more help to the less proficient students.

For Bossert the worst consequence of recitation is that it creates a competitive hierarchy among students which shifts prestige and privilege to the top-performing students. "What developed in the recitation-dominated classrooms was a teacher-organized academic hierarchy based on performance. . . . [S]pecial privileges, such as working in small groups and special projects, and teacher assistance were allocated in terms of a child's relative performance within the classroom." In contrast, the teachers whose classrooms were organized around multitask activities saw different students as good at different kinds of work or insisted that all their students did "academically sound" work (p. 61). Bossert furthermore shows that this difference in task organization has substantial effects on students' friendship patterns. The classrooms in which recitation is central lead children to form stable friendship groups sharply stratified by academic achievement. In the multitask classrooms friendship groups remain fluid and extremely heterogeneous academically (pp. 64–67).

Bossert's scheme of interpretation sounds novel at first, but all too quickly we recognize our old acquaintances—by now the enemies of effective research on teaching—the good, warm, liberal, supportive, open-style teacher; and the rigid, authoritarian, discipline-oriented traditional teacher. Bossert studied two teachers who relied primarily on recitation. They also favored rigid discipline, made some children into teacher's pets, patrolled their classrooms to enforce rules, and so forth. Two other teachers spent substantially more time in multitask activities and were friendly, supportive, encouraging, and democratic in their teaching styles. Task organization and overall teaching style may well go together in just this predictable way, but then Bossert has not really introduced a new dimension of classroom organization. He has relabeled (and perhaps extended) a dimension we know all too well, and with much too little effect. When Bossert compares a specific teacher behavior ("desists per 100 minutes") across tasks and teachers, he finds that task organization has an independent effect, but the teacher effect is stronger than the task effect (p. 49).

Tasks and Social Relationships in Classrooms is also peculiarly lacking in a sense of the dynamics and difficulties of teaching—the trade-offs teachers face between the advantages of a friendly, relaxed teaching style and the potential desirability of being able to control students. For Bossert, multitask activities, and the fluid classroom organization they permit, create an ideal learning environment. He does not examine the effects of task organization on learning directly, but he hypothesizes that, since the recitation-dominated classrooms “inhibit the development of strong affective bonds” and decrease “empathic relationships” between teachers and students, the “multitask organization . . . may be more conducive to pupil achievement than recitation” (pp. 97–98). He thus ignores the widely reported findings from studies of alternative schools that more relaxed, less structured classrooms enhance student morale but do not enhance academic achievement. Even from Bossert’s data, it is evident that the teachers who used multitask organization spent a good deal of time on arts and crafts activity, in contrast to the more consistent stress on academic tasks by the teachers who used recitation. Furthermore, the total amount of task activity (vs. “free time”) was substantially greater in the recitation-dominated classrooms (p. 46). Finally, Bossert ignores the real difficulties of pulling off the organization of a multitask classroom. As Mary Metz has shown (*Classrooms and Corridors: The Crisis of Authority in Desegregated Secondary Schools* [Berkeley and Los Angeles: University of California Press, 1978]), “developmentally-oriented” teaching requires extraordinary energy, stamina, and expertise. And even with all these, there may simply be a trade-off between creating a pleasant environment in which children work and play at what interests them and creating an environment that effectively fosters academic learning.

Like Bossert, Hugh Mehan ignores conflict and competing interests in classroom life, but for him this is a matter of principle. His goal is to uncover the normative rules through which orderly classroom life is produced by those who participate in it and, collectively, construct it. He austere limits himself to close analysis of what happens within classroom interactions themselves, rejecting consideration of precisely such issues as the motives and interests of the participants, their personalities, social class backgrounds, and so forth (pp. 120–21). Instead, he provides a detailed analysis of nine lessons extracted from 13 hours of videotaped classroom activity (spread over several months) in one first- through fourth-grade classroom.

Mehan describes his theoretical approach as “constitutive ethnography,” an account of the “structuring activities” through which the “social facts” of classroom life are produced (p. 17). To understand how lessons are constructed, Mehan begins by identifying the basic units of teacher-student interaction, which he calls “elicitation sequences,” three-part interactions in which the teacher first asks a question or otherwise elicits a response from students, a student replies, and the teacher evaluates the response. These initiation-reply-evaluation sequences may range from requests for a

specific answer ("What is a word that starts with 'M'?"), to requests for simple agreement or disagreement, to requests for more complex opinions or interpretations. Perhaps unsurprisingly, teachers repeat their "elicitations" until they get the answer they are looking for. (In Mehan's typical language: "Once an instructional sequence has been initiated, interaction continues until the symmetry between initiation and reply acts has been obtained" [p. 52].)

Elicitation sequences are in turn organized into "topically related sets." The teacher marks the beginning of a topically related set by gesturing, positioning herself, and "establishing an orientational position vis à vis the students and instructional materials" (p. 65). Topically related sets are in turn the constituent units of lessons. A lesson begins with overall direction from the teacher about how the lesson will proceed; its body consists of sets of topically related elicitation sequences; and it ends with a closing phase in which the teacher again offers directions and information (p. 75).

The second set of rules that organize classroom lessons concerns turn-taking, or rather the procedure by which the teacher allocates the floor to students. The teacher may "nominate" a particular student, calling on the student by name or by directing a question to that student by nonverbal means, invite "bids" by asking students to raise their hands, or invite replies directly by addressing a general question to the group of students. Teachers again enforce "symmetry" between the kinds of turns they invite students to take and students' responses. If a student speaks out of turn or otherwise violates the turn allocation procedure the teacher will negatively sanction the violation (p. 98).

Readers who come to Mehan's book to understand education, rather than to pursue the special agendas of ethnomethodology or sociolinguistics, will probably find *Learning Lessons* unsatisfying. First, Mehan's interest in the constitutive rules which generate classroom interactions forbids any direct analysis of either the content of lessons or of the motivations or roles of classroom participants. Not what gets done in classrooms, but how it gets done, in a formal sense, is Mehan's concern. Furthermore, constitutive ethnography's aim is to find the underlying set of rules that account for observed patterns of interaction, and thus Mehan's "goal is to construct a model that accounts for *each and every instance* of teacher-student interaction in the nine lessons in the corpus" (p. 20, emphasis added). Such an approach necessarily commits him to "accounting for" interaction at a very abstract level of formal regularity.

Indirectly, however, Mehan does end up telling us something about what teachers and students are doing during lessons (aside from performing, once again, the miracle of interacting with one another). Mehan notes, for example, that school talk is different from many other kinds of talk. "Evaluation" plays a "significant role in instructional discourse," while it "seldom occurs in everyday discourse" (p. 64). In a similar way, turn-taking in classroom interactions is distinctive in that the floor always reverts back to the teacher at the conclusion of a given interaction. These hints cannot,

of course, be explored within Mehan's framework, since he does not directly compare classroom lessons to other forms of interaction or compare patterns of interaction across teachers or across classroom tasks. A fusion of Bossert's interests with Mehan's methods might, however, have produced a very interesting book. Used with substantive questions in mind, Mehan's method might reveal the fine-grained patterns that constitute teacher authority, make one classroom different from another, or differentiate interaction in schools from that in other organizations.

Education's Lasting Influence on Values. By Herbert H. Hyman and Charles R. Wright. Chicago: University of Chicago Press, 1979. Pp. ix+161. \$12.50.

John P. Robinson
University of Maryland

Herbert H. Hyman and Charles R. Wright's 1975 work on *The Enduring Effects of Education* (Chicago: University of Chicago Press, 1975) represented one of the most exhaustive secondary analyses of survey data ever conducted. That analysis provided remarkably consistent evidence on the role of formal education in inculcating individuals with a drive for accruing information and knowledge that persisted well after a person left school. However, some critics, as well as Hyman and Wright themselves, expressed concern over the narrowness of their cognitive dependent variables, given the wide-ranging lessons about attitudes and values that also take place during formal education.

Education's Lasting Influence on Values, which is about half the size of the earlier volume, is addressed to this unfinished business. It examines the relation between education and values from 151 discrete instances when relevant value questions were asked in NORC or Gallup surveys between 1949 and 1975. The values examined include those implicit in child-rearing priorities (honesty, responsibility, etc.), tolerance of nonconformity (à la Samuel Stouffer), freedom of expression, equality of opportunity, and those embodied in support for legalization of abortion or opposition to capital punishment.

The analytic techniques applied to these data by the authors are much the same as those employed in their earlier work: percentage differences across education categories (elementary, high school, and college) controlled for various third variables. Age is the major third variable examined (Appendix C), although controls on the possible effect of sex, religion, residence, occupation, and social origin are introduced in Appendix D. No multiple-regression analyses are used to control for the effects of several of these variables simultaneously.

As was the case for the conclusion about information accrual presented in the earlier work, the authors find that these 50 pages of tabular material

can be boiled down to a surprisingly simple "main effect." Again the message of that main effect is almost unequivocally positive: "... education produces large and lasting good effects in the realm of values" (p. 60). The main effect persists no matter when the person's formal education was obtained, where the person's formal education stopped, or whether the value questions asked were phrased in concrete or abstract terms.

Despite the impressive array of situations in which the authors find this basic relation to exist, their case for demonstrating "effects" cannot be airtight. No panel survey data on the same individuals are presented to test the alternative hypothesis of selective recruitment of potentially open-minded, empathic, or humanitarian individuals by educational institutions. Hyman and Wright recognize the problem and discuss it at some length, offering for example the argument that whatever selectivity is operating should be minimized in more recent generations when advanced education is available to practically anyone. They argue that their main effect is also found at the college level, despite the large variation in values held by incoming freshmen at various institutions.

Yet this very line of argument can be turned the other way. Educational institutions do vary in the egalitarian/humanitarian lessons they emphasize in their instruction, and the authors' case could be strengthened by showing that institutions that minimize this emphasis have a minimal effect on liberating values and vice versa. Evidence of similar interaction effects is found in Appendix D, where education's effects on values are found to be stronger for Protestants than for Catholics and among white-collar workers than among blue-collar workers. These religious differences may have interesting implications for the values inculcated by Catholic schools, while the class differences may indicate the relative ineffectiveness of a middle-class institution on working-class orientations.

The authors' case for their main effect is enhanced by examining their findings in the context of the existing literature. Their general conclusions seem consistent with those of Stouffer's classic study, *Communism, Conformity and Civil Liberties* (New York: Doubleday, 1955), and the comprehensive reviews of Howard Bowen (*Investment in Learning: The Individual and Social Value of American Higher Education* [San Francisco: Jossey-Bass, 1977]), and of Kenneth Feldman and Theodore Newcomb (*The Impact of College on Students* [San Francisco: Jossey-Bass, 1969]); the latter works review the type of panel study data that would have strengthened the present volume.

Reading the book could have been enlivened if Hyman and Wright had identified and quoted the various educational critics whose arguments these authors' data are intended to refute. Perhaps these critics had a wider range of values (materialism, individualism, etc.) in mind than the mainly humanitarian ones examined here. Potential critics of *Education's Lasting Influence on Values* might also have been more persuaded by behavioral manifestations of values than by verbal recognition of "the right answers" in the survey setting; at the same time, Hyman and Wright do show that this

recognition persists across an impressive variety of survey circumstances.

Thus, it is hard to diminish the monumental task these patriarchs of secondary analysis have achieved in once again bringing complex and fascinating data to light in their highly accessible style. The magnitude of the educational differences identified by the authors may not constitute the proof that the purist might seek, but they seem unlikely to fade into insignificance even if the effects of selective recruitment could somehow be properly controlled. The implications of their main effect should not be disregarded at a time when various dismantlings of our basic education system are being contemplated.

Psychoanalytic Politics: Freud's French Revolution. By Sherry Turkle. New York: Basic Books, 1978. Pp. x+278. \$12.50.

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Everyone remembers the social upheavals which occurred in France in the spring of 1968. Not as well known is the role of psychoanalytic theory and practice in catalyzing this revolution. Before the late 1960s, French psychiatric, intellectual, and political life was united in staunch opposition to Freud's psychoanalysis; but after the disorders of 1968, French culture reversed its viewpoint and enthusiastically embraced psychoanalytic theory, most notably in the writings of Jacques Lacan. *Psychoanalytic Politics* is an account of this transition.

In a readable and nuanced fashion, Sherry Turkle describes the social conditions and political attitudes of French culture prior to its assimilation of psychoanalysis, with particular attention to existentialism and Marxism. She clearly shows the crucial place of Freud's ideas in the thinking of those psychoanalysts, students, and patients who made the 1968 upheaval a reality. Then she goes on to document the way French culture paradoxically accommodated psychoanalytic ideas in its Marxism, existentialism, feminism, and antipsychiatry, concluding with a convincing description of the new, commanding role of the "French Freud" in current French popular culture. Her several discussions of Lacan's leading ideas are especially illuminating, in the light of social context. The book is a pleasure to read and is especially informative for Americans who may have wondered how the highly politicized French could ever have made Freud their own. American readers are also in for a jolt when they see the contempt with which French psychoanalytic writers view our trivialized and watered-down interpretations of Freud.

Turkle's sense of what France's reinvention of Freud means is as simple—indeed, alarmingly simple—as her descriptive account is nuanced and complex. As the anchors of traditional society gradually give way, and as bureaucratic structures become increasingly cold and controlling, strain is

placed on the inner, private sector of personal life. This strain creates a demand for new ideas—psychological ideas—which will structure and organize an individual life which is becoming increasingly autonomous and separate from institutional norms. In this, Turkle likens the French situation to the conditions accompanying the American acceptance of psychoanalysis, for she believes that both cultures contain the same social dynamics. But American psychoanalysis was interpreted in the context of the typical emphases upon adaptation, autonomy, the authority of the medical doctor, and the primacy of *curé*. The French context is political: it presupposes enduring conflict between the self and the social order. This conflict is central to the writings of Lacan, who is accordingly unremittingly hostile to American ego psychology, with its presupposition that a portion of the drive-regulating function of the mind is, from the beginning, conflict free. Hence the French Freud could never have appeared in America, and the medical-adaptive American Freud has never succeeded on the Continent.

Turkle's good and lively social descriptions raise deeper substantive questions: To what tradition of inquiry, if any, does this book belong? What, in fact, is her approach? Is it empirical sociological analysis, or the interpretive sociology of subjective meanings, or a blend of these? Put more simply, To what other books might this one be likened? On these points Turkle has, as I have indicated, little to say, and what she does say is ambiguous. She makes deliberate though only passing reference to Philip Rieff and Peter Berger as the social theorists to whom she is most indebted. But the works of Rieff and Berger share important affinities with what Edward Shils has called the theory of mass society, which has its roots in the writings of Mannheim, Ortega, and Scheler. This theory is enormously sophisticated intellectually, as Rieff's and Berger's powerful analyses of the rise of a psychological society indicate. But it is also empirically quite clumsy. In fact, it is doubtful that it has any intention of making empirical claims at all.

Turkle's book in effect carries forward one central insight of this theory (that institutional weakness produces psychological thinking), by way of Rieff and Berger, into the more empirical realm. As such, her work is as much interpretation as it is explanation: she interprets meanings as much as she explains causes. This may account for the ambiguity Turkle herself displays in describing her work: first as the sociology of science, then as the sociology of knowledge, but also as ethnography (she refers to her "informants" a number of times). It is also, I think, a sociology of culture, a history of the psychoanalytic movement in France, and, further, an account of the rise of psychological ideals in a posttraditional, modern society. This accent on interpretation may also account for the fact that, while her book, strictly speaking, probably belongs to that amorphous, emerging genre of studies which Allan Buss has called the sociology of psychological knowledge (*Psychology in Social Context* [New York: Irvington, 1979]), it also bears considerable affinity to the work of Fred Weinstein and Gerald Platt (*The Wish to be Free* [Berkeley and Los Angeles: University of California

Press, 1973]), of Marthe Robert (*From Oedipus to Moses: Freud's Jewish Identity* [New York: Doubleday, 1976]), and of Dorothy Ross (*G. Stanley Hall: The Psychologist as Prophet* [Chicago: University of Chicago Press, 1972]). All are highly interpretive studies of the genesis of psychological ideas.

But, as I said, Turkle lifts out of the theory of mass society only a single part, that dealing with the contemporary tension between self and society. Rieff and Berger grounded their analyses of this tension in an elaborate global theory of the secularization of Western culture. For them, the rise of a psychological culture is dialectically related to the progressive decline of Western institutional life as this life was energized and structured by a religious orientation to the world. Turkle does not mention religion, understood as a declining historical force, at all.

In either case—whether one reads this book as good description or, at a deeper level, as interpretation—there is much to be learned not only about psychoanalysis in France in the 1960s and 1970s but also—if only by implication—about the rise of a psychological view of the world in modern societies.

The Existential Sociology of Jean-Paul Sartre. By Gila J. Hayim. Amherst: University of Massachusetts Press, 1980. Pp. xviii+157. \$13.50.

Edith Kurzweil
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From the time of the publication of his philosophical novel *La Nausée* in 1938, Jean-Paul Sartre remained at the center of French intellectual life and struggled against being monumentalized and deified, as well as against having his ideas objectified and simplified. But "existential freedom," expressed in human action in "material situations," not only lent itself to philosophical interpretation, but also to the quick "descent" from Kierkegaardian or Hegelian heights to the streets of Paris—in the songs of Juliet Greco, and in various forms of left-wing politics. Although Sartre wrote fiction and autobiography and commented on literature, art, society, people, criticism, and politics, his philosophy was always part of everything else, transforming the *pour-soi* (undefined and free subjectivity) into the *en-soi* (reified objectivity through the eye of others) in whatever medium he used.

In *The Existential Sociology of Jean-Paul Sartre*, Gila J. Hayim looks to Sartre's philosophy alone and, in particular, to his method of social inquiry in *Being and Nothingness* (1943) and in *Critique of Dialectical Reason* (1960). She documents how the notion of future-oriented action—that is, of freedom—in the earlier work deals with the themes of individual and collective behavior stressed in the later one. And she locates Sartre's sociology in these themes, ignoring his own generally loose definition of sociology and dissociating herself from what is usually known as "existential soci-

ology," with its emphasis on feelings, and from interpretations that exaggerate Sartre's concern with "the absurd," "the obscure," or "the irrational." Instead, she sets out to extract a sociology of action, connecting the individual actor to his social group and to what he or she does, and to show how Sartre, like Max Weber, is concerned with temporality, choice, values, and goals, rather than with hidden essences of secret subjectivity (p. 3)—the more customary philosophical foci. Hayim anchors Sartre's existentialism in the philosophical tradition from Socrates to the present—in the inextricable connections between knowledge and existence, in the dualist theories of mind/body or exterior/interior, and in questions of precedence of materiality versus consciousness. But she avoids philosophical arguments: by addressing "social" and "personal" content alone, she can tell us how Sartre's philosophy can be useful for sociology.

Hayim starts by tracing similarities between Weber and Sartre: matter and consciousness as well as things and human values constitute both their worlds; they both see meaningful action as conscious and goal directed; and they both posit that consciousness, when in good faith, resists determination. Further on, Hayim finds "convergence" between George Herbert Mead's "I" and the Sartrean "*pour-soi*," and between the former's "Me" and the latter's "*en-soi*." And she demonstrates, plausibly, how Durkheim's emphasis on structures and institutions negates the core of Sartre's ideas, namely, individual freedom and the privacy of human activity and goals.

After showing how Sartre deals, for instance, with will and passions (means to carry out a project), with being illuminated through nothingness, with Freudian psychoanalysis (a means of recognition of a conscious situation), with the unconscious (a manifestation of bad faith), or with individual death (a prey for the living), Hayim moves to the ethical implications of these premises—that is, to the self-conscious situation and the socially responsible person who is "the *being by whom values exist*" (p. 67).

This focus on the individual, at the same time, appears to account for the fact that Hayim plays down Sartre's Marxism. But she is justified in this position, insofar as the *Critique* itself underlines what had been wrong with Marxist praxis and does not deal directly with Sartre's own politics. Hayim contrasts Sartre's analyses of institutions in vivo—his reader is engaged in a constant movement between freedom and submission, between emotions over anticipated action and unintended consequences—to the "artificiality and massification" of our customary sociological language (p. 75), which is said to lack "phenomenological reciprocity." Reciprocity, however, connects all levels of existence, so that the person as *pour-soi* allegedly becomes meaningless and incomplete, unless engaged in voluntary activity. How Sartrean praxis functions in different types of groups, how interaction with Others can become antihuman, and how, paradoxically, the sense of being human, when part of reflexive praxis, can come from action on matter is excellently clarified by Hayim. She always keeps her eye on the sociological arguments as she addresses questions of group needs, group unity, group formation, regrouping, etc., on the micro level, and notions of power, prohibitions, or potential violence on the macro level, and focuses on the in-

teraction between them. Yet Sartre's own concern with process constantly remains central, as groups form, gain in importance, receive pledges from individuals, and then induce them to relinquish their freedom. (I was surprised, only, not to find references to Georg Simmel.) Anyone who has read Sartre's *Critique* will appreciate the lucidity Hayim brings to these discussions, as she explains, for example, the links between concepts of leadership and authority and Sartre's common praxis and as she shows how he applies David Reisman's notion of other-directedness to individuals' political behavior and, later on, to the "merger between State and bureaucracy" (p. 135).

Like Sartre's *Critique*, Hayim's book ends with the emphasis on human freedom, on the struggle which moves "from inertia to rebellion and back to inertia" (p. 143). Moments of hope, tragedy, and contradiction combat alienation and make us experience reality. This is the Sartrean legacy Hayim wants sociologists to share. For when she concludes that *Being and Nothingness* and the *Critique* "aim at making the experience of reality intelligible" (p. 148) and thereby help us reflect on, and regain command of, our lives, she suggests that sociology and its language be humanized and that our customary categories be dereified. Her message—which, incidentally, in spite of her controlled language is typically French—ought to be heeded, especially by those sociologists who tend to forget the larger cultural issues.

The Age of Structuralism: Lévi-Strauss to Foucault. By Edith Kurzweil. New York: Columbia University Press, 1980. Pp. xi+256. \$20.00 (cloth); \$5.95 (paper).

David Goddard

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When Claude Lévi-Strauss began to write structural analyses of kinship, totemic classification, and mythology in the 1940s and 1950s his audience was quite limited. A few Marxists called attention to the ahistorical nature of his investigations; the younger generation of English anthropologists at Oxford and Cambridge was fascinated by his 1949 treatise, *The Elementary Structures of Kinship* (Boston: Beacon, 1969), of which Rodney Needham wrote a superb explanation and defense in *Structure and Sentiment* (Chicago: University of Chicago Press, 1962). *Tristes tropiques* (1955), while widely read, only hinted at the intellectual direction Lévi-Strauss was taking. In a series of essays published as *Anthropologie structurale* (1958), the earliest of which dated from 1943, it did become evident that he was attempting to chart a new direction for anthropological studies that would challenge the prevailing functionalist orthodoxy, but there was no particular suggestion that he was unveiling a new intellectual doctrine at odds with the dominant currents of French thought at the time. *Le Totémisme aujourd'hui*

(1962) was similarly directed to an anthropological audience and again attacked functionalism, although a broader epistemological theme emerged at the end. But in the same year *La Pensée sauvage* appeared, and almost overnight structuralism became internationally fashionable. The reason for this lay in the criticisms Lévi-Strauss directed at Jean-Paul Sartre's newly developed conceptions of subjectivity, history, and dialectics, the synthesis of existentialism and Marxism which had appeared in 1960 and which contained a few unkind remarks about Lévi-Strauss's remote and austere objectivism. As Paul Ricoeur was later to note in "*Structure et herméneutique*" (*L'Esprit*, Nov. 1963), the main thrust of Lévi-Strauss's work was toward a "reduction of meaning" to the level of apparently formal relations between elements of cultural material in an attempt to disclose "an inventory of mental constraints." It was, Ricoeur said, a Kantianism without a transcendental subject; or, as Lévi-Strauss himself put it in *Le Cru et le cuit* (1964), he was interested in the human mind, the identity of whose "occasional bearers" was essentially irrelevant to the analytic purpose. It was this, rather than a structuralist method derived from Saussurean linguistics, that set Lévi-Strauss apart from the subjectivist and historicist mainstream, but it is unfortunately given no real emphasis in Edith Kurzweil's ambitious book, *The Age of Structuralism*.

Kurzweil has, however, provided the American reader with a very adequate survey of the work of the principal writers of the 1960s, most of whom were influenced by the concepts and methods of Saussurean linguistics and tantalized, if that is the word, by the possibility of discovering some order of underlying and unconscious structures in historical, literary, psychological, or cultural reality. But what situates these writers in "an age of structuralism" is, to say the least, tenuous. Resonant themes from linguistics were incorporated into the analysis of a variety of cultural, social, and intellectual phenomena without it ever becoming clear that there was such a thing as a structuralist method, a common set of presuppositions, or any kind of theoretical framework. Barthes, Derrida, Foucault, Althusser, and Lacan shared no common outlook, hewed to their own particular subject matters, and could not in any way be said to belong to the same school—or, indeed, said to be structuralists. Kurzweil is partly aware of this and takes pains to show that the work of Lefebvre, Touraine, and Ricoeur (which she examines along with that of Lacan, Althusser, Barthes, and Foucault) was not structuralist and that the work of the latter four was not oriented toward any common methodology or set of agreed-upon structuralist categories. But nowhere, and this is perhaps the weakest point of the book, is the nature of structuralism established in any clear fashion. Its clearest expression is in Lévi-Strauss's earlier essays where he seeks to extrapolate a method from Jakobson and Trubetzkoy, as well as from Saussure, as a theoretical rationale for the study of kinship. By the time he embarked on the four volumes of *Mythologiques* in the 1960s his interest had come to be simply sets of relations, the model for which might just as easily be mathematics or physics or music. Paralleling Marx, he might well have said that he was not a structuralist.

Moreover, the other writers considered are or were far too iconoclastic to be grouped together even loosely. Marx and Freud, for instance, were important as sustaining influences for some (e.g., Althusser, Lacan, and Ricoeur) but were generally unimportant for Lévi-Strauss, Barthes, and Foucault. Lefebvre and Touraine had little interest in structuralism except to attack it. In the final analysis, perhaps all that can be said about structuralism in the brief period before 1968 when history caught up with it is, as Lefebvre put it (in "*Lévi-Strauss et le nouvel élitisme*," *L'Homme et la Société*, nos. 1-2 [1966]: 36), that it was "a language of the immobile" inconsistent with the idea (and also the reality) of historical transformation in the "hot" societies of the West.

But what remains of structuralism as a lasting contribution to the human sciences is the anthropology of Lévi-Strauss, a body of work almost excruciatingly painstaking in its attention to detail and both far humbler in its ambitions and far wiser in its philosophical clarity than the intellectual pyrotechnics all too frequently put out in the name of structuralism, or as borrowing from structuralism, in the heady years of the 1960s. As Lévi-Strauss remarked in a different context, "They have the word but not the thing." Or, in the words of the late A. L. Kroeber, the term "structure" is like "streamlining": "during its vogue [it] tends to be applied indiscriminately because of the pleasurable connotations of its sound" (*Anthropology* [New York: Macmillan, 1958] p. 325).

Notwithstanding, Kurzweil's book is a useful survey, and for the reader who is looking for a fairly painless entry into this tortuous chapter of French intellectual history, these sketches of the main protagonists are valuable. In particular, the chapters on Ricoeur, Touraine, and Lefebvre (the nonstructuralists, incidentally) are well worth reading.

The Technological Conscience: Survival and Dignity in an Age of Expertise. By Manfred Stanley. New York: Free Press, 1978. Pp. xix+281. \$15.95 (cloth); Chicago: University of Chicago Press, Phoenix Books, 1981. \$9.95 (paper).

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The central thesis of *The Technological Conscience* is that the illegitimate extension of scientific and technological reasoning into all spheres of human action has corrupted the language of politics and social science and poses a grave danger to the dignity of the individual. Manfred Stanley pursues this hardly novel idea with considerable philosophical and sociological erudition, focusing on the realm of language. *The Technological Conscience* offers some fine descriptions of linguistic corruption, but is less adequate in providing a social and historical explanation of this phenomenon.

The book falls into three parts. In the first, Stanley states that his inten-

tion is to offer a sober, rational option to what he calls the "optimist" and "pessimist" positions concerning the impact of modern science and technology. While pessimists such as Arendt, Heidegger, Mumford, and Marcuse appeal to his humanist sympathies, Stanley wants to rescue their views from the "blind alley" of antimodernism. This rescue effort involves shifting the discussion away from technology and science per se to "technicism . . . the metaphorical misapplications of some of the assumptions, imagery and linguistic habits of science and technology to areas of discourse in which such mistakes obscure the free and responsible nature of human action" (p. xiii). His goal is "a criticism of technicism that stops short of a total rejection of the very idea of science (and social science)" (p. 6). The conceptual and linguistic imperialism of technicist theory and practice rests on "ignorance regarding the nuances and the implications for action of language itself" (p. 7). Technicism presents self-regulating objective processes and eclipses the vocabularies of human intention and agency. Not satisfied with what he sees as the privatized communalism of the counterculture, Stanley seeks a renewal of the ideal of the public as it was expressed in the now-eroded liberal and Marxist traditions. Such a renewal, in turn, demands a revival of "dignity talk," speech about the nature of individuals which sets clear limits on potential technological manipulation. Stanley bases his view of the dignity of individuals in language because it is through language that persons "in however small a way . . . construct and alter the social world" (p. 73) and thus become responsible for it.

In part 2, Stanley seeks to demonstrate the impact of technicism on social science through a discussion and critique of prevalent analyses of legitimacy, power, and scarcity. He criticizes Stinchcombe, among others, for separating observed behavior and subjective intentionality and in so doing enhancing the possible "technicist utilization" of the social sciences for purposes of social control. Stanley devotes a chapter to cybernetic theorists such as Ludwig Bertalanffy, Karl Deutsch, and Ervin Laszlo, as well as American and Soviet defense planners, and criticizes the "general tendency" of cybernetic theory "to reduce ends, goals, data, values, decision and language itself to instrumental-operational terms" (p. 136). Although he does not reject cybernetic thinking completely—he believes it can contribute to understanding the connection between social complexity and spontaneity and between society and the environment—Stanley does think it fosters an ersatz pluralism based on an erosion of distinctions between opinion and judgment, information and propaganda, and that it furthers a tendency to obscure the importance of human agency in social action by projecting agency onto cybernetic symbols. The author argues that an alternative "nontechnicist approach to social theory" should draw heavily on Schutz and Garfinkel, as well as develop a philosophy that views persons as producers and consumers of symbolic interpretations. He believes that a social science sensitive to meanings and symbolic consciousness "is surely less susceptible to technicist reductions than any other" (p. 130).

In part 3, "Toward the Liberation of Practical Reason: The Problem of

Countertechnicist Social Practice," Stanley looks to education for protection of public language from "technicist corruptions." He discusses educational policy literature from the social sciences and argues that it has incorporated a utilitarian, technocratic language which itself undermines a major purpose of education—that is, the fostering of "civic literacy" necessary to close the gap between expert and nonexpert discourses and thus to defend democracy. He is critical of what he sees as the potential for elitism and authoritarianism in Paolo Freire's philosophy of education and advocates instead a humanist ideal of education as "cultivation of the person as a critical, reflective agent" (p. 213). He concludes by appealing to conservatives, liberals, and radicals to unite in a common program of preservation and restoration of the "linguistic, cognitive and social competences" (p. 250) of the citizenry in the face of the mystification of technical vocabularies, irrational specialization, and "unlimited professionalism."

The major problem with *The Technological Conscience*, one which it shares with much of the current discussion of language and/or positivism in social science, is that it either connects linguistic corruption inadequately to social relationships and institutions or connects them on such a high level of abstraction—such as seeing the roots of the problem in a general "crisis of modernism"—that it becomes very difficult to move beyond description to explanation. The question why this particular form of linguistic corruption becomes pervasive when and where it does ought, in my view, to be posed in the context of the problem of reification. Here it would be in order to pay greater attention to Husserl, Adorno, and Marcuse. Stanley does not do so, partly because he exaggerates the inadequacies of what he calls the "pessimist" position. Mumford and Marcuse, for example, were far more aware of the connections between technology, language, and social relations than the author allows. Perhaps some sacrifice of the complexity of their views is unavoidable, given Stanley's construction of the optimist and pessimist positions against which he wants to present himself as the rational alternative.

Another problem is Stanley's recurrent idea that antitechnological pessimism laid the cultural groundwork for National Socialism. This notion, once a commonplace in scholarship on the Nazi era, has come under recent challenge. Heidegger and Spengler, among many others, were quite enraptured with technology and its spirit. The real outpouring of German anti-technological pessimism emerged from the rubble of 1945. The demonization of the evil machine was a convenient reification in the period of the Nuremberg war crimes trials. In the 1920s and 1930s, "technicist" language and political irrationalism were more reconcilable than Stanley's dichotomies would suggest.

Despite these shortcomings, Stanley's humanist, phenomenological, and linguistically sensitive approach will be welcomed by those social scientists who favor an ongoing dialogue with history, philosophy, and literature and who are concerned that the language of their own disciplines threatens the goals of democracy and individual dignity which they profess.

The Myths of Information: Technology and Postindustrial Culture. Edited by Kathleen Woodward. Madison, Wis.: Coda Press, 1980. Pp. xxvi+250. \$6.95 (paper).

Mark Poster

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The Center for Twentieth Century Studies at the University of Wisconsin—Milwaukee has established a project on the relation of technology and culture. A main hypothesis of this project is that technological innovations should be studied not as simple contributions to material well-being, but as interacting profoundly with the existing culture. The first fruits of the endeavor are two volumes: *The Technological Imagination: Theories and Fictions* and *The Myths of Information: Technology and Postindustrial Culture*. The latter volume is a product of two symposia: one was held at the Milwaukee center in 1977, the other occurred in Nice the following year under the auspices of the Centre du 20ème Siècle. Volumes such as this can suffer from unevenness of quality and incoherence, but that is not the case here. The contributions do vary in quality, but in general the level is high. More remarkably, the whole stands together: each contribution, whatever its perspective, confronts the problematic at stake in the project. And this is so even though the collection is genuinely international: the contributors teach in France, Germany, Canada, and the United States. No doubt a good deal of the credit for the success of the book must go to the editor, Kathleen Woodward.

Interest in the volume, however, inheres in the topic, information and society, and the light brought to bear upon it. Even with the recent burgeoning of departments of communications and the growing corpus of literature devoted to the "information sciences," one has the sense that the social sciences have yet adequately to come to grips with the astonishing transformation in the forms and qualities of communicative practice that has occurred in large part since the end of World War II. An argument can be made that our society bears little resemblance to that theorized by the founders of social science—Marx, Weber, and Durkheim; that the generation and dissemination of information so pervades our institutions and practices that traditional conceptual strategies are either outmoded or in need of basic revision. With few exceptions, the work of the social sciences has failed to register, much less confront, this possibility.

A short review cannot do justice to the diversity of argumentation in *The Myths of Information*. Suffice it to say that perhaps nowhere else can one find a similar combination of strategies to confront the issue of information and society. One closes this volume with a sense of the enormity of the task of constructing an adequate theory of information and a corresponding sense of the wealth of empirical investigations that this topic affords. The book presents a panorama of the options, from the grim outlook of Jacques Ellul to the sanguine rigor of Oskar Negt.

One of the more interesting pieces is by Jean Baudrillard, a sociologist at Nanterre, who has been working on a theory of the media for a dozen years. Baudrillard is concerned with the effects on society of the media's dissemination of information. He takes exception to the dominant views about this relation: (1) that the spread of information through the media is a social good because it increases meaning, providing the population with valuable knowledge; and (2) that, properly speaking, information has nothing to do with "meaning," but is a value-free tool, as Claude Shannon suggests. Against these arguments Baudrillard posits a third position: that the spread of information via the media destroys "meaning" or, more precisely, social relationships.

With considerable insight into the deficiencies of the media as vehicles of human communication, Baudrillard demonstrates how every step taken by them, which appears to make important knowledge available, may in fact be a ruse, that functions in an opposite way, to prevent communication or to produce simulations of communication. Thus we have the staged interviews, the phony dialogues, the on-the-spot questionnaires that play at immediacy and authenticity but in fact produce rigged outcomes, answers structured by the embarrassment of momentary stardom. The consequence is, according to Baudrillard, entropic, emptying the social field of the type of symbolic exchange that increases relational complexity.

For all the power attributed to the media, Baudrillard contends that the masses remain impervious to their influence. In a splendid reversal of perspectives he writes, "Is it the media that cast the spell of fascination on the masses, or the masses who reduce the media to spectacle?" (pp. 146-47). It may be, he speculates, that the masses absorb information transmitted by the media, but without effect, neutralizing its impact through a kind of passivity.

These remarks illustrate the type of creative theorizing that characterizes many of the contributions to this volume, which is worthy of the attention of social scientists concerned with the issue of information and culture.

Television and Human Behavior. By George Comstock, Steven Chaffee, Natan Katzman, Maxwell McCombs, and Donald Roberts. New York: Columbia University Press, 1978. Pp. xviii+581. \$16.95 (cloth); \$9.95 (paper).

Mark R. Levy

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Understanding the effects of television on human behavior is one of the most difficult, but also one of the most important, problems facing the social science community today. While many scholars and commentators have views about the "tube's" impact, comparatively few have been able to demonstrate its effects in a scientifically acceptable fashion. Our theories of

television's effects are surprisingly weak and our methodological armamentarium is depressingly bare. Yet despite these enormous impediments, some tentative conclusions can be drawn. In *Television and Human Behavior*, George Comstock and his colleagues have produced a wide-ranging and informative synthesis—an intellectual stocktaking-*cum*-critique. Based on a three-year review of more than 2,500 studies, this substantial volume is chock-full of high-quality empirical generalizations; important, if limited, attempts at theory building; and provocative suggestions for future research, both basic and applied.

After a brief and well-written overview, the first substantive chapter examines the content of television. Entertainment programming, "kid-vid," news, and even commercials are discussed in terms of recurrent themes, bias, and presentation of social reality. A particularly interesting section deals with the so-called violence index, explaining its construction and utility and summarizing the methodological criticisms which recently have been leveled against it. Although most of the major forms of television content are discussed, here as elsewhere the authors are restricted by the "literature." Few analyses exist of "trivial" but heavily watched programs such as game or talk shows, and the number of cross-genre or cross-cultural studies too is limited. All would be important for a full understanding of television's messages.

A chapter on the television audience summarizes all major scholarly investigations of viewing patterns and augments those findings with some special analyses of commercial ratings data. As thorough as this chapter is, it suffers because most of the findings are presented in terms of traditional audience demographics like sex, age, and income. Recent research, which should have been available for this project, has supplemented this with a more psychographic, life-style, or uses and gratifications approach to audience measurement—a far more illuminating way to dissect the mass of viewers.

In a chapter titled "Living with Television," the authors present a quick but thorough discussion of the place of television in the daily life of the audience: what else people do when they watch television; the place of television viewing in total leisure time and the way it displaces other activities; and the "truth" which television programming executives so long have known—namely, that people do not watch television programs, they simply watch television.

The longest chapter in the book provides an especially valuable summary of television's effects on that most studied of all populations—children. The scientific quagmire of television violence is skillfully negotiated, as the authors cautiously conclude that exposure to television violence increases the likelihood of subsequent aggression in children who watch it. Also considered in this chapter are television as a learning experience, television as an agent of socialization, and television's prosocial impact.

Four additional subgroups—women, blacks, the poor, and the elderly—are the focus of a brief chapter. In addition to examining studies of tele-

vision's effects on each of these groups, the authors also discuss the ways in which television portrays and stereotypes these avid viewers.

In the following chapter, two types of choice behavior—voting and buying—are considered together, since, as Comstock et al. acknowledge, the behaviors have similar underlying psychologies. Although the effects of persuasive message campaigns have long been studied, the authors conclude that few exact relationships have been firmly established.

In an attempt at middle-range theorizing, Stephen Chaffee (assisted by Albert Tims) puts forth a psychological model of behavioral effects which attempts to explain divergent communication situations. Presented as a flowchart, the model builds on such variables as image salience, the arousal ability of messages, patterns of constraint recognition, and existing repertoires of behavior. While Chaffee makes a compelling case for the model, its ultimate fruitfulness has not yet been tested.

The book concludes with a sensitive look at the future, outlining a research agenda which gives highest priority to studies of television as an agent of socialization and in the political process. The never-ending struggle to make television research policy-relevant and policy-acceptable is reviewed with clarity and sophistication.

In short, *Television and Human Behavior* represents a mature summing up of our knowledge about mass communication, roughly at decade's end. This book is as strong—and as weak—as the research it reports. Like much contemporary mass communication research, it is excessively psychological. The impact of television on social groups, organizations, and society is largely ignored—not because the authors of this thoroughly commendable volume suffer from a reductionistic bias, but simply because so much of that work remains to be done.

Hearth and Home: Images of Women in the Mass Media. Edited by Gaye Tuchman, Arlene Kaplan Daniels, and James Benet. New York: Oxford University Press, 1978. Pp. xi+333. \$15.95 (cloth); \$3.50 (paper).

Helen Baehr

Polytechnic of Central London

Since the late 1960s the women's movement has responded critically, often angrily, to what it has rather loosely called "sexism in the media." In her introduction to *Hearth and Home* Gaye Tuchman describes the media's treatment of women more specially—as "a national social problem" (p. 5). Her plea to readers (and presumably to the American establishment) is: "How can we free women from the tyranny of media messages limiting their lives to hearth and home?" (p. 38).

A book about sex-role stereotypes in the mass media is to be welcomed by all who struggle to change sexist images for progressive ones. But this book's overall approach reveals another tyranny—the ever-present assumption and strain of American media research toward empirical effects.

The book is organized around the themes of the symbolic annihilation and trivialization of women in the mass media. It is divided into four parts, on television, women's magazines, newspapers and women's pages, and the effects of television on children and youth. A concluding chapter deals with the policy implications of the material of the book.

Tuchman's introductory chapter argues that "the media, with some cultural lag, reflect dominant attitudes and values in a society and act as agents of socialization, teaching youngsters *how to behave*" (p. 37, emphasis added). Larry Gross and Suzanne Jeffries-Fox, in their essay on "Television's Effect on Children and Youth" (chap. 14), set out to "consider how the *effects* of television on viewers might be identified and measured" (p. 240, emphasis added).

The evidence and arguments put forward in this collection are organized, uncritically, around concepts of the "mass," "effect," and "function" and around that dominant paradigm of mass communication research which makes simplistic, unidirectional, and reductive connections between media presentations and behavior. The research assumptions are behavioristic—that is, they argue that women's behavior and attitudes result from exposure to the mass media, thus obeying laws of conditioning and reinforcement. Tuchman writes: "Watching a lot of television leads children and adolescents to believe in traditional sex roles" (p. 37). The relationship between the media and audiences is reduced to a causal one. But it is not the media in themselves that determine what women are. Women's roles are constructed outside the media as well, and it is women's marginality in culture generally *and* in the media which contributes to their subordinated position. We must look beyond media content to examine the relationship between women's subordination in terms of their economic place in patriarchal relations and the representation of those relations in the ideological domain.

George Gerbner, in "The Dynamics of Cultural Resistance" (chap. 1), is one of the few contributors to resist the empirical temptation of content analysis. He probes some of the connections between the media's role and the established structure of social relations. He argues that it is the main function of culture to cultivate resistance to change, stating that when women, or other groups denied access to power, are shown as independent, adventurous, and powerful, they are portrayed (on television) as "enforcing rather than challenging the laws that oppress them" (p. 50). They play policewomen, detectives, or soldiers. I would argue that the dominant culture does not merely resist change, as Gerbner suggests, but that it incorporates and works to produce and determine new definitions. This can be seen at its most simple level in the construction of a new media stereotype, the "liberated woman." E. Barbara Phillips's essay points to this progress when she asks, "Is 'Ms.' just another member of the 'Family Circle'?" (chap. 6).

A useful idea is explored in Carol Lopate's analysis of Jacqueline Kennedy Onassis's treatment in 12 women's magazines (chap. 7). Lopate

argues that the magazines are integrated into the political and economic world in the very way in which they present women and women's needs. "Jackie" is treated in ways that allow the magazines to project images of women which define work outside the home negatively and associate happiness and emotional fulfillment not with money but with children and the home. "With this definition, women remain the group that can be pushed in or pulled out of the labour force, depending on the economy's needs" (p. 140).

Gaye Tuchman (chap. 11) outlines the role of the women's page of the *New York Times* in constituting the women's movement as a news item. In the early days of consciousness raising and "issues," the movement did not fit the professional definition of "news" which prefers events to issues. Sunday, weekend, and night meetings often took place too late for coverage of them to meet copy deadlines. Tuchman raises the important question of whether women's pages ghettoize issues and decrease the likelihood of women's news appearing elsewhere.

The themes of *Hearth and Home*, the ideas and concerns it seeks to investigate, are central to the feminist struggle against sexist representation. But the book as a whole falls short by dint of its uncritical reliance on and acceptance of empirical content analysis and reflection models of the media. As a method, content analysis is an impoverished means of discovering the aesthetic structures and media practices which produce an ideological effect in the material they organize.

Chapter 9, by G. William Domhoff, is by far the worst offender. Domhoff argues that the women's page of the *San Francisco Chronicle* works as a "window on the ruling class" (p. 161). With its coverage of high-society weddings, parties, luncheons, and functions, the women's page is "the only part of the newspaper that tells us each and every day there is a ruling class in America" (p. 175). I cannot help feeling that a better understanding of the crucial relationship between the ruling class and the mass media (i.e., of the relations between the social and economic conditions of patriarchy and cultural production) would have been reached had Domhoff focused his investigation on the Hearst family's press empire instead.

The Economic Crisis and American Society. By Manuel Castells. Princeton, N.J.: Princeton University Press, 1980. Pp. xiv+285. \$20.00 (cloth); \$7.50 (paper).

Roberta Garner
DePaul University

"The nation is in the worst economic mess since the Depression," the president tells us, and Manuel Castells tells us why. In a densely textured book, Castells examines the causes and the unfolding of the crisis. The major

strength of his approach is the dialectical emphasis, the insistence that crisis must be understood as a process involving tendencies, countertendencies, and the crisis-generating effects of the countertendencies themselves. Castells identifies these economic tendencies and countertendencies and analyzes their sources in the social structure of the United States. Economic crisis is a social process.

The Economic Crisis and American Society begins with a chapter on the theory of crisis in advanced capitalism. The chapter is a difficult one that presupposes an acquaintance with Marx's theory of crisis and, specifically, with his concepts of the extraction of surplus value, the organic composition of capital, and the falling rate of profit as the organic composition rises. Castells considers the falling rate of profit to be the key mechanism of the long-term decline of capitalism, and in this chapter he challenges the critics of this theory.

Castells discusses the difficulties in interpreting the empirical evidence concerning trends in the composition of capital and the rate of profit. He introduces some ingenious defenses of the theory: one is the suggestion that the organic composition of capital has indeed risen at the level of the whole society (if not at the level of individual firms) because of the growth of necessary but unproductive expenses for transferring value to products—"an increasing number of people are employed as 'dead labor' " (p. 52). A second defense of the theory is the cogent argument that the fall of the rate of profit must be understood within a system that includes a number of countertendencies, especially state intervention and the internationalization of capital; the falling rate of profit becomes visible only as these countertendencies are exhausted.

In chapter 2, Castells examines the process of crisis since World War II. In a remarkably compact and lucid chapter, he identifies the major contradictions of capitalist accumulation in this period and discusses three countertendencies: the internationalization of capital in search of markets and lower wage rates and accompanying efforts to establish U.S. political, ideological, and military hegemony; the development of a debt economy with the expansion of consumer credit and the growing power of financial institutions; and the massive multifaceted role of the state. As we now see all too clearly, these countertendencies to crisis have in the long run contributed to the crisis.

Chapter 3 returns to the theme that crisis is not a mysterious vibration of the economy but a product of a social system in which classes are the actors in the mode of production and their relationships, conflicts, and actions give rise (intentionally and unintentionally) to the tendencies and countertendencies of crisis. Here Castells examines the class structure of the United States, the structure of corporate capital, the emergence of a service economy and its accompanying strata, the problems of un- and underemployment, the division of the economy into monopoly and competitive sectors, and the related problem of "minorities" (i.e., groups that have disproportionately borne the burdens of high rates of unemployment

and low wages in the dual economy). He concludes the chapter with a consideration of the "urban crisis," since the city is the spatial and political locus where many of the structural contradictions are most painfully manifested. This chapter is the least satisfactory of the book; Castells cannot resist the temptation to cover too much material, and thus he occasionally fails to clarify linkages between class relationships and the systemic processes addressed in the first two chapters.

In chapter 4 Castells examines how class structure is translated into political action concerning economic and social policies: here the links between economic processes and class structure are more clearly explicated than in the third chapter. Classes, the elements of the mode of production, are locked into "now open, now hidden" conflict with each other, and often these conflicts are fought out in the arena of the state. Measures that provoke crisis and measures to resolve it are expressions of class interest—often but not exclusively those of the capitalist class. Castells analyzes this process, citing especially the reforms of the 1960s, the way in which these reforms threatened the limits acceptable to capital, and the reversal of reforms in the 1970s. He concludes the chapter with a discussion of pressures for "austerity" and government cutbacks and plans for the reassertion of U.S. hegemony abroad.

The book ends with a look toward the future. Castells is no Marxist Chicken Little who hails every crisis as the final collapse of capitalism. The present crisis may be a stage in the decline of capitalism, but collapse is not imminent. Castells believes that there are two ways of resolving the crisis within the capitalist system: a step forward toward economy and social democracy, or a move toward what he calls overexploitation (reduction of jobs, reduction of government expenditure, work speedups, reduced wages, and efforts to maintain control abroad). But Castells believes that the solution of overexploitation is not viable in the long run, nor can it be sustained internationally (since corporate capital does not have the near-total hegemony abroad that it seems to have in the United States). Europe may become the vanguard region in the supersession of capitalism.

In short, Castells has written a comprehensive, timely, and masterful overview of the present crisis of capitalism. Before ending, I want to urge that *The Economic Crisis and American Society* reach the widest possible audience. Everyone should become more aware of the systemic nature of economic crisis and its origins in social relationships; the complex tendency-countertendency analysis presented by Castells is an excellent antidote to the simplistic explanations ("oil prices," "wage demands," or "government spending") circulated by the media. Within academia, this is a book for scholars, graduate students, and upper-level undergraduates. It is a must for political study groups. With some preparation in basic concepts and some help through the first chapter, the neophyte can also learn from it. Non-Marxists will find it free of the rancor that sometimes repels them in the writings of Marxists. It is especially timely now that the veils of liberal propriety are being stripped from the relationship of the state to the class system.

Corporations, Classes, and Capitalism. By John Scott. London: Hutchinson Publishing Group, 1979. Pp. 219. £8.95 (cloth); £3.95 (paper).

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The rise of the joint stock corporation as the dominant economic form is perhaps the central fact of modern political economy, emphasized by virtually every major theoretical perspective. The corporation not only dominates the economy, its characteristic bureaucratic form shapes government, education, health, science, and other institutions.

Corporations, Classes, and Capitalism is an impressive attempt to deal with an important aspect of the increasing social significance of the corporation. John Scott discusses the major issues concerning the relationship between class structure, corporate control and ownership, and the dynamics of political economy. Topics include the relationship of property and control in corporations, the relationship of financial and industrial capital, the effect of corporations on the class structure, the economy, and the state, and the internationalization of capital through the multinational corporation. While there is no theoretical breakthrough to frame these topics, the book makes a valuable contribution through an unusually sharp presentation of the issues and an unprecedented collation of cross-national data.

The book's organizing principle is an attempt to carve out a conceptual path between the two major competing perspectives on the corporation: the "theory of industrial society" and the "theory of capitalist society." Both are well known. The theory of industrial society is the model upon which Berle and Means built their thesis of the separation of ownership and control. It stems from a Weberian model of modernization, and major contributions since Berle and Means have been made by Parsons, Dahrendorf, Aron, Kerr, and Galbraith. The model assumes that the structure of modern society is based on scientific rationality stemming from the logic of industrial technology. The major principle for the analysis of corporations is the separation of ownership and control. It is a nonclass model in which management is viewed in terms of rational direction of industrial technology through bureaucratic organization.

The model of capitalist society is rooted in Lenin's concept of finance capitalism. Its major tenet is the separation of money capital from the function of capital, or a separation of finance (speculative) and a "real" (productive) capital. Under monopoly capitalism, control is wielded by a small number of financial groups which dominate at the national level but compete through imperialist conflict.

Scott caricatures both models, especially the Marxist model. Some Weberians share his criticisms of the theory of industrial society. Many Marxists have criticized the Leninist model on the same grounds that he does.

His middle-road "theory of industrial capitalism" is basically a synthesis of a particular Weberian (Giddens) and a particular Marxian (Zeitlin), each of whom share Scott's criticisms of his "own" perspective.

Fortunately, constructing the middle ground is not the major contribution of the book. The book's core is a set of chapters which skillfully present middle-range issues and valuable data as the basis for a set of reasonable conclusions. For example, in discussing property relation and the mediation of control, Scott carefully considers how the corporation has changed the connection between the legal forms and social relations of ownership. He draws important distinctions between control over production and benefit gained from production, between strategic and operational control of the firm. Following this, he insightfully inventories various modes and methods of control. His discussion of capital ownership and strategic control includes valuable data on the distribution of income, wealth, and ownership, while a careful review of data on the various modes of corporate control highlights similarities and differences among Western industrial nations. The chapter on the relationship of the corporation to class structure synthesizes a broad range of data on class organization, the relationship of the capitalist class to other social institutions, and associated issues. His definition of class is Weberian (it is defined in terms of life chances and cross-generation reproduction). However, his discussion of the ways in which the rise of the corporation "structurates" the capitalist class by socializing capital throughout the class directly pertains to the Marxian concern with relationship to the means of production: in the corporate economy, each owner's relationship to production permeates the economy rather than being confined to a single enterprise. Scott concludes that, contrary to the theory of industrial society, there is still an organized class of propertied families who are increasingly free from particular property interests but who maintain a general class interest and sufficient closure to insure continued class reproduction.

My final assessment of the book is mixed. The attempted middle road leads nowhere. There is no grand synthesis, but a fragile negotiated truce. The book's strength lies in its careful presentation of various positions on important issues and an impressive mustering of relevant facts. Scott's cross-national comparisons are especially revealing. His sober discussion of issues and data admirably refrains from jumping to profound speculation. But this diplomacy is also his weakness. His own perspective remains a dimly sketched synthesis, drawing from the perspectives he criticizes but adding very little that is new. The major contradictions remain unresolved; they simply recede into the background.

Nonetheless the book deserves a wide audience. Those familiar with the debate over ownership and control of corporations and related issues will benefit from the clear and skillfully drawn discussion of the issues. Those who are not will find a cogent presentation of the issues in a slim and readable volume.

Knowledge and Decisions. By Thomas Sowell. New York: Basic Books, 1980. Pp. x+422. \$18.50.

Michael J. White
Princeton University

Societies function by means of decision making. Every society has a choice between carrying out decision procedures on a collective (centralized) basis or on an individual (decentralized) basis. Thomas Sowell's wide-ranging book about polity, economy, and society argues that individual decision making is, in the end, both more efficient and more equitable than the collective sort. Sociologists, who expend much effort in the study of collective behavior and bureaucratic organizations, will find Sowell's arguments provocative, although perhaps not always persuasive.

Knowledge and Decisions is divided into two parts: part 1 ("Social Institutions") introduces the fundamental concepts of knowledge, decisions, and trade-offs; part 2 ("Trends and Issues") examines historical developments in economics, law, and politics. Chapters 1 and 2 treat the problems of the fragmentary nature of knowledge and the nature of decision making. Sowell takes note of two facts. First, society's historical accumulation of information is not the property of any single individual or group. Second, decisions come in a great variety of forms: formal or informal, "package deals" or incremental options. Centralized, categorical decision making operates without knowledge of all the relevant facts. In contrast, prices (in the marketplace and elsewhere) quickly convey information about the relative scarcity of resources in an incremental and decentralized way. This contrast provides the major theme of the book.

Chapters 3, 4, and 5 focus on trade-offs: economic, social, and political. Sowell argues that knowledge itself is the central resource and commodity in the economy. This oft-ignored fact has led us to the "physical fallacy": We convince ourselves that there is intrinsic value to an object, when in fact its value depends on its relationship to other objects in society at a given time.

Social and political trade-offs are in principle the same as economic trade-offs. They are just less obvious. Socially we err when we presume (invoking the "animistic fallacy") that social events all result from purposive efforts of actors toward a specified goal. Politically we err when we presume (invoking the "democratic fallacy") that the majority always can and should have what it wants. What is missing from the political and social decision-making systems is a weighting scheme (market) that allows us to trade between alternatives in accordance with their importance in our lives. Bureaucracy is inefficient because of the categorical nature of its decisions. The market incentive for incremental decisions is absent.

In sum, part 1 argues for a systemic approach in which it is possible to have a rational society without the explicit cognitive intention of individual actors. Insulation from feedback (e.g., price movements) can wreak havoc with social organization. Both Adam Smith and Karl Marx are called as witnesses to the case for the "invisible hand."

Chapters 7–10 turn to more detailed treatment of trends in society. In the economy we see the increasing sabotage of the market system by government and its bureaucratic accomplices. Sowell cogently argues that controlling prices and wages, restricting producers and sellers, and planning all have distorted information and caused inefficiency.

In law and politics, too, the government usurps the decision-making power of knowledgeable individuals. The courts are particularly problematic because of their insulation from feedback. The consequences of "judicial activism" are evident in the courts' inconsistent and sometimes counterproductive decisions on such matters as race relations and crime. Sowell identifies three critical political developments: (1) worldwide growth in size and scope of government, (2) the rise of ideological politics, and (3) the growing political role of intellectuals. These three trends, he concludes, threaten the very existence of freedom in modern life.

Knowledge and Decisions is both forceful and readable. Sowell has taken many of the basic tenets of neoclassical economics and applied them with clarity to society at large. He makes his case without resort to the jargon, graphs, and equations of many contemporary economists. Because of this approach, his book will appeal to many who are not trained in economics. Policymakers in particular would be well advised to grapple with the issues he raises.

Sowell is at his best when discussing economics. In the realms of politics and law, he cites numerous examples to support his view, but there is less assurance that his arguments would withstand theoretical scrutiny and empirical test. There is no attempt to prove directly that a systemic (market) solution to problems of society, politics, and law is necessarily better than the admittedly error-prone system we now have. When discussing economic issues Sowell can draw upon a large literature directed specifically to the question of efficiency. In politics and law, work along those lines is less substantial. A nagging question remains unanswered: If these infringements on our freedom are so bad, then how and why do they persist? An especially curious aspect of the book is that Sowell rails against intellectuals as if they collectively conspire against individual freedom. His writing here is ironically reminiscent of harangues against "capitalists" who subvert the freedom of workers.

Most frustrating of all is Sowell's reluctance to provide some hint of what actions should be taken to remedy the illness. Surely he does not simply believe that we should "turn the clock back" or eliminate government. Perhaps Sowell's prescriptions will be the subject of his next work.

Who Should Know What? Social Science, Privacy, and Ethics. By J. A. Barnes. Cambridge and New York: Cambridge University Press, 1980. Pp. 232. \$17.50 (cloth); \$4.95 (paper).

Irving Louis Horowitz
Rutgers University

J. A. Barnes's *Who Should Know What?* is an entirely literate, engaging essay in the sociology of sociology. Written in the broad tradition of Robert S. Lynd's *Knowledge for What* and C. Wright Mills's *The Sociological Imagination*, the work makes up in fair-minded decency for what it lacks in literary bombast. For a senior honors or first-year graduate course in the ethical foundations of social research, one could hardly choose a better text. Such an overview seems to be Barnes's intent, and his book is clearly successful when evaluated on its own terms. This is not to suggest that the volume successfully resolves the major problems which it poses—it most certainly does not—but it does present the proper issues in a fair-minded way, and it is accessible to those with a minimum of philosophical training.

The slim volume has a certain datedness. It reflects the concerns manifested by social science during the Vietnam era and its Watergate aftermath, rather than those of the current period, in which the West is faced by a series of challenges from a Soviet-dominated "East" and an OPEC-dominated "South." Indeed, this very change in circumstances illustrates one of Barnes's major themes: the difficulty in presuming an axiomatic equation between any social or economic group and the nature of research itself. While he is clearly empathetic to the many peoples and situations studied by social science and less than pleased with the linkages between researchers and the powerful, he is supple and imaginative enough to appreciate the degree to which changing circumstances alter images of commitment no less than paradigms.

The book is divided into three parts, nine chapters in all. In an opening chapter, Barnes describes his task as a practical attempt to resolve ethical issues in social inquiry by taking into account the distribution of power among scientists, citizens, sponsors, and gatekeepers. There follow three chapters on the historical development of social inquiry, emphasizing the natural science paradigm within which social research evolved; the institutionalization of social inquiry, by which the author simply means the role of powerful agencies and bureaucracies in manipulating research to certain predetermined ends; and the challenge to universalism, which refers to the powerful reminders, often from radical and Third World sources, of the partisan and hence particularistic characteristics of any authentic social science in a world divided in systems and outlooks.

The second and most innovative part of Barnes's text concerns the process of inquiry: why people want to make or prevent social inquiries and the ways in which information is made public. Here the emphasis is on the differential interests of researchers and the investigated and on the comple-

mentary processes of collecting data and communicating results. Barnes shows how these themes directly link up to new computer data-gathering techniques and lead to a much more accelerated notion of professionalization, in the very process of discussions concerning safeguarding and gate-keeping. The third and final part deals with social science as a specialized activity which has significant relations to and impact on the wider society. Here the emphasis is on such issues as professionalism, commitment, privacy, and the nature of pluralistic, democratic societies as sources of continuing growth no less than serving to focus new issues for the social sciences.

That Barnes is himself caught in the dilemmas he poses should come as no surprise. He appreciates that in giving full attention to the rights and interests of all parties to an inquiry there is a danger that empirical research may become innocuous and trivial. He also recognizes the need to provide some protection for subjects of investigation, especially those least able to defend their own interests. While this formulation has the full weight of contemporary rhetoric behind it, it may be time to leave such polarizations to the intellectual dustbins. The need of the moment is more to examine how researchers are themselves managed and manipulated than how they serve as managers and manipulators. Barnes gives too little weight to Third Worlders and First Worlders alike as special interest elements. He tends to underestimate the emotive no less than the substantive powers of the "powerless," who in fact may command sizable chunks of political turf in advanced no less than in backward societies.

Curiously, Barnes perceives the threat of social science to be great because he exaggerates its potency. The unstated premise of his text is that measures to insure safety and privacy are needed because of the impact of social research. It might well be that the actual power of social science is less than he suggests and that the actual consensus about values is also less as a result. The concern with privacy may really reflect an implicit belief in an omniscient social science (which Barnes does not really possess any more than I do). The other side of this coin, which is only barely hinted at near the end of the work, is the issue of publicity: social science is and must remain a public act. Efforts to curb publication of findings and results, in the name of higher truths, special interests, or perceived dangers to the downtrodden, all entail the serious danger of censorship—worse, of self-censorship in the name of principle. For that reason, publicity and the right to know are no less part of the framework of pluralist democracy than privacy and the right to deny access to persons. Democracy is a system in tension. It would be foolhardy to deny that social research is also a system in tension, albeit one of lesser proportions and fewer consequences. Barnes presents these tensions well, but his attempt to resolve them is less successful.

For the most part, Barnes navigates perilous waters with admirable clarity and fairness. He is profoundly aware of manifold critiques of the social sciences, especially of sociology and anthropology, as agents serving the

powerful while studying the powerless. He is also acutely sensitive to the intrinsic requirements of scientific analysis as such, but he knows that the desire for universalistic criteria reflects the influence of natural scientific models which do not allow for the confusion and contradiction of interests which social research is required to examine. It might have been wiser for the author to appreciate the fact that social science confronts a wider array of ethical issues than power issues. For example, there are matters of choice among topics that have wide or narrow applications, decisions to emphasize constructive or critical themes. It might have been worthwhile in a volume of this sort to dedicate at least a few pages to the state of the social sciences in the socialist bloc nations of Eastern Europe. But one can forgive the selective nature of the work, which is after all intended to be intensive rather than extensive.

Less forgivable is the myopia that leads the author to assert with self-righteous indignation that "pirating is a breach of national or international law on copyright" (p. 155) in the context of a U.S. Department of Commerce English-language translation (not for publication) of a French anthropologist's essay on a Montagnard village in Vietnam. While this appropriation may well have been a breach of etiquette, the unwary reader should know that the term "pirating" refers to acts engaged in for commercial ends by many Third World countries to avoid obtaining legitimate publishing rights and paying royalties to Western publishers of record. It is a problem of great concern to publishers. The example Barnes provides scarcely fits the term.

The biggest disagreement I have with *Who Should Know What?* occurs in the same context. Barnes criticizes the author of this 1957 report for his failure to disguise identity, for in this way the ends of "American aggression" could have been thwarted, and at the same time the subjects of his analysis "would have suffered during the war no more than other groups did. Had he known how much use was to be made of ethnographic intelligence by the United States forces, he might have taken the precaution of using disguises . . ." (p. 155). The problem here is simply that Barnes is falling victim to the very concerns he raises elsewhere: blaming social research for problems of social life. While he does not urge that the masses should shoot the messenger, he is urging the messenger to dilute, even falsify, the message—which after all is what disguising names, sources, addresses, and places is ultimately about. The reasons that this older style of disguise has fallen out of scientific favor are that it does not work (clever individuals can always detect such things) and that it lessens the worth of ethnography by making it more difficult to determine its truth or falsity. Finally, it moves social science back to a prejournalistic phase, in which the obligations of in-depth reporting are subordinated to some presumably higher commitment to a set of political ideologies or social values. To adopt this posture is to answer the question, Who should know what? in terms of political ideology, not social reality—the very antithesis of what Barnes seeks. This again points out that no final answer can be given to the ques-

tions raised, but a higher level of sensitivity and creativity can be sought. We need to carve out a set of values serviceable to social scientists as an interest group and a professional group of no more—but certainly no less—social worth than other interest groups in society.

In the concluding passage of the work, Barnes notes that “we need not only empirical social science but also an awareness of the ethical issues that form an intrinsic part of its praxis” (p. 188). This is fine as far as it goes. But it is more important for us to move ahead with the task of showing how ethical issues are themselves subject to social scientific analysis, that is, to move beyond a sociology of knowledge into a sociology of values. I fear that we have become so enamored of problems in the ethical status of social science that we have tended to dampen any corresponding sense of the empirical correlates of ethical propositions. If social science is to be subject to, or guided by, a set of ethical postulates, it behooves us to examine in concrete historical and empirical settings the scientific status of these ethical guidelines. The earlier work of Richard Brandt, Ralph Barton Perry, and Abraham Edel in this connection is especially important. The ghostly fact-value formula not only haunts *Who Should Know What?* but plagues much theorizing about the ethical status of social research. We can view moral propositions in one of two ways: as stationary targets at which we take aim with our bow and arrow, or as moving targets subject to a wide variety of interpretations. I daresay that, before social research gets too uncomfortably close to moral majoritarian fervor, we should recall the shifting nature of ethical propositions and their differential uses. In this sense, the work of Barnes may help us locate the moral framework, but it may also have a dampening effect on social science once the moral beast is ensconced.

CORRECTION

Conversations at Random: Survey Research as Interviewers See It, by Jean M. Converse and Howard Schuman, which was reviewed in our July 1981 issue (pp. 213–14), is no longer available from John Wiley & Sons. Instead, it is handled by the University of Michigan Institute for Social Research (paperback, \$5.50).

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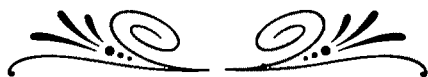
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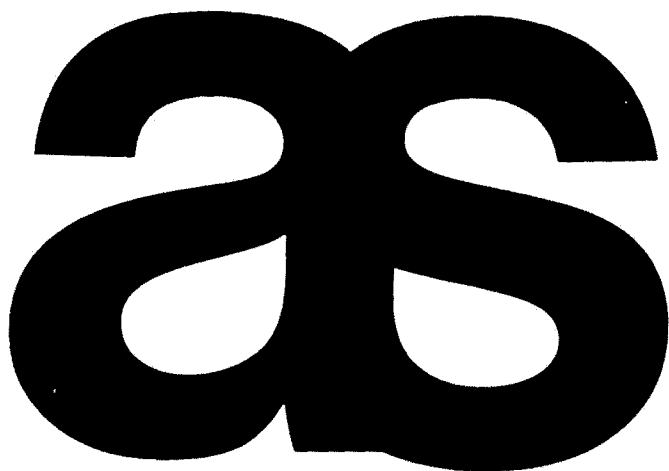
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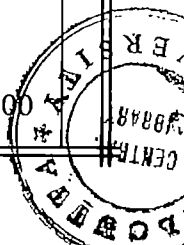
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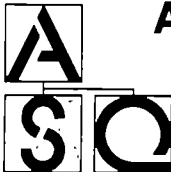
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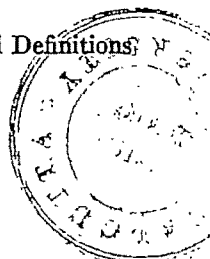
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A Liberal Theory of Prestige¹

Keith Hope
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Earlier empirical and theoretical work showed that members of the public distinguish quite sharply between the rewards of an occupation and its value to society. The present paper argues that the prestige of occupations, as it is ordinarily graded, is a simple average of these two dimensions, implying that prestige has both a factual and a normative component. The argument is theoretical rather than inductive: it begins from the assumption that assessments of prestige do in fact involve considerations of economic reward, and it employs the data in conjunction with this well-grounded assumption to infer what other considerations must be involved. The argument is prosecuted by means of polar coordinate analysis. This method is applicable because R^2 for the regression of social standing (taken to be a synonym for prestige) on other aspects of occupation is close to one; and virtually all the variance of each vector of occupational assessment lies in a space of three dimensions. The final discussion suggests that, although the data point to the existence of a normative consensus, this consensus bears little relation to prestige in its traditional sense. However, in a modern democratic society the traditional prestige which bolsters the elite is a fragile thing, whereas the normative consensus on occupational prestige may constitute an important source of legitimation of the distribution of social rewards.

Tommy Wilson always tried to be on the side of the angels: he endeavored to think about serious matters; and he attempted to express his thoughts in distinguished phrases. Those were exceptional attributes in the United States after the Civil War, when most men of ability were concentrating on the acquisition of wealth. They gave Tommy Wilson both prestige and an endearing idealism. [FREUD and BULLITT 1966, p. 8]

In this paper I examine and contrast two theories of the nature of occupational prestige. The first theory is that the prestige of an occupation is its pay-off, that is, its objective rewards, summed up in the standard of living

¹ Part of this paper was drafted during a spring seminar spent at the University of Arizona in 1976, and I express my gratitude for the invitation to teach in the Sociology Department at Tucson. Work on the final version of the paper was stimulated by a challenge to explain the nature of prestige scales which was thrown out by O. D. Duncan. Numerous comments by the anonymous assessors for *AJS* helped to improve the argument of the paper and clarify its presentation. Requests for reprints should be sent to Keith Hope, Nuffield College, Oxford OX1 1NF, England.

it sustains. The second, which may be called the "liberal" theory, is that prestige is partly a matter of objective rewards and partly a matter of moral evaluation. The empirical analysis comes down on the side of the liberal theory and against the objective rewards theory.

The argument of the paper is carried on mainly at the level of shared or collective perceptions and beliefs, and it is prosecuted by the manipulation of angles and vectors representing averaged perceptions and beliefs. But evidence is cited to show that there is a high degree of consensus among the averages of different groups in society. Perhaps even more surprising is the evidence that the two components of prestige are very sharply distinguished. It is demonstrated that when these are represented as error-free vectors they form an angle of 70° if each is graded by a different informant. And even when both are graded by the same informant, the angle shrinks only to 65° .

But before presentation of the theory and the evidence, a general look at the nature of prestige is in order.

DOES PRESTIGE MATTER?

It might be supposed that the task of explaining the nature of prestige can require no sociological defense, since occupational prestige, or a close correlate of it, is the most frequently employed variable in the quantitative study of social stratification. However, there are at least three reasons for supposing that a defense may be called for. The first is that "synthetic" or "composite" social variables have in the past decade fallen under suspicion (House 1978). A second reason is that sociologists have recently manifested a tendency to reinterpret prestige scales as indicators of socioeconomic status (Featherman and Hauser 1976). And a third reason is that sociologists of class have chosen to ignore the vertical hierarchy of social stratification entirely (Goldthorpe, Payne, and Llewellyn 1978). These three reasons may, indeed, be regarded as aspects or manifestations of one and the same distrust of unidimensional analytical continua. Broadly speaking, House and those who think like him want to substitute several continua; Featherman and Hauser want to reinterpret a psychic dimension as a class dimension ("socioeconomic status" is an American way of saying "class"); and Goldthorpe, Payne, and Llewellyn want to abandon the idea of a status hierarchy and substitute the idea of barely ordered classes.

The objection to synthetic scales may easily be countered, since all sociological measures are synthetic; the completely hard or factual variable is as elusive as the sense datum.² No doubt some variables are more synthetic

² It is arguable that the ethnomethodologists' unending quest for an indisputable social fact is analogous to the logical atomists' search for an atomic utterance. If this is granted, the analogy has something to teach us. E.g., the ethnomethodologists' move

than others, but it is not easy to determine the considerations on which such a judgment might be grounded. Indeed, the tendency to substitute a variable such as socioeconomic status for a psychic evaluation, such as people's judgment of prestige or social standing, may well prove, on close examination, to constitute a move from a dimension which is a given with respect to sociology, to a dimension which is theoretically complex. The issue cannot be debated with any confidence until we know what we mean by "synthetic" or "composite." Attempts to provide a nonformal definition of these terms almost always get bogged down in an irreducible element of metaphor. But if we resort to a formal definition, we may (following Wrinch and Jeffries [1921]) equate simplicity with paucity of freely adjustable parameters. Then, adopting Popper's (1968, chap. 7) development of this idea, we may identify a simple variable as one having a single degree of freedom and a complex or composite variable as one having several degrees of freedom. It then becomes evident that any linear variable can be simple in itself, although, if it can be perfectly reproduced by combining other variables, it is a composite with respect to its components. Thus such terms as "simple" and "composite" have meaning only in relation to some theoretical framework and changing the framework can convert the synthetic into the simple, or vice versa.

In this paper I take the simple dimension of occupational prestige and show that it may be considered as compounded of two other variables. I advance this argument by reference to the fact that the prestige or social standing of an occupation is almost perfectly reproducible from (lies in the same space as) a small set of other occupational characteristics.

These characteristics are attributes of occupations as perceived by members of society. That is, we are analyzing occupational prestige as *verstehen* and not as objectively recorded socioeconomic status. This procedure should not be held against occupational prestige. It is sometimes argued that the psychic dimension of occupational ranking is trivial because the rankings do not enable us to draw inferences about social behavior, such as who will defer to, or dine with, or marry, whom. This argument is indiscriminating in that it would apply to any scale or principle of classification not derived from observations of social behavior, and it would apply even to a scale which was so derived with respect to generalization to kinds of behavior from which it was not derived. But in any case the argument is invalid, for it is one thing to say that knowledge of the way in which people perceive an occupation does not license inferences about how they will behave

from an interest in research instruments as a way of improving our knowledge of "social facts," to interest in them for their own sake, without reference to their validity, bears a remarkable resemblance to the shift from logical atomism to logical positivism (Urmson 1956). Ethnomethodologists are the true positivists. Quantifiers are, or may be, the true students of *verstehen*.

with respect to it, and it is quite another to say that the existence of readily elicited and shared perceptions of occupation has no consequences for behavior. A scale which is not inferred from social behavior may or may not have important correlates in social behavior. Whether it does is a matter for further empirical investigation. Although there is no logical connection between perception and social action, it would be surprising if no empirical and intuitively comprehensible connection between prestige and social choice could be discovered.

It is of course true that the capacity to order the names of occupations, printed on little cards, does not imply anything about general social behavior. Such an inference would be invalid, but the validity of a test does not inhere in its mode of construction. It is not logically justifiable to infer conclusions about general behavior from an artificial test, but neither is it correct to deny the possible sociological links between the perceived prestige hierarchy and more general behavior. Indeed, the very fact that people can grade occupations with a fair degree of consensus and stability constitutes a *prima facie* reason for supposing that this skill or capacity must be furnished by occasional, if not frequent, employment in the course of everyday life. Broom, Jones, McDonnell, and Williams (1980, p. 16) have shown ingeniously that men looking back over their careers tend to identify the occupation which has the highest prestige as their "best" job.

Treiman (1977, pp. 26 ff.) quite rightly takes Goldthorpe and Hope (1972) to task for ignoring the sociological evidence showing the relations between the prestige ordering of occupations and forms of differential association such as marriage (Laumann 1966; Hope 1972) and commensality (Dumont 1970). In sociological theory relations of this nature are usually regarded as aspects of social status as opposed to class, and it is indeed true that when we began our work on occupational grading (Goldthorpe and Hope 1972) we surmised that people's assessments of occupational roles reflect no more than their perceptions of the class position of those roles. More precisely, we hypothesized that, in grading the social standing of an occupation, informants are reporting the net pay-off which they believe the typical incumbent will gain from it. We were willing to grant that a number of factors may contribute to the final judgment of pay-off (which we called "general goodness" or "desirability"), but we suggested that these factors are all of the sort about which there is intersubjective agreement that it is better to have more of the factor than less, other things being equal. We explicitly surmised that the social standing of an occupation lacks any element of status or normative approval. This hypothesis implies that prestige or social standing reflects only the nonsocial rewards of a job, that is, such factors as economic benefits, conditions, and job satisfaction. The view was summarized in the claim that, in grading the social standing of occupations, informants are reporting their assessment of the general

goodness of jobs. (The phrase "general goodness" is perhaps not a happy way of referring to a view which explicitly excludes normative elements. But it has its origins in the common working-class expression, "He has a good job down at so-and-so's," an expression which rarely takes into account anything but material rewards.)

That was the starting point, and it has at least the merit of clarity. But it was not the finishing point, for the data belied the hypothesis. In a paper I delivered to the British Sociological Association in 1973 under the title, "What Are People Doing When They Grade Occupations?" later published as appendix A of the monograph entitled *The Social Grading of Occupations* (Goldthorpe and Hope 1974), I examined the evidence from a study in which a sample of Oxford electors graded 40 occupations on each of four dimensions. The title of the paper was picked up from a phrase in a thoughtful research report in which Gusfield and Schwartz (1963) reflected on the lessons to be learned from the early NORC work on occupational grading. They asked "what respondents did when they were asked to rank occupations" (p. 265), and they observed (p. 266) that "'prestige' must be something other than the perception that a given job has a 'better shake' than some other or else the term is useless and should be dropped." They examined the possibility that ratings of occupations may be merely "descriptions of a factual order, in which the existent fact that A is a 'better' job than B is recognized" (p. 271). Thus they distinguished very clearly between the view that occupational assessment is purely factual and the view that it comprehends both factual and evaluative elements.

The hypothesis that people's assessments of jobs are factual alone, rather than factual and normative, has its roots in working-class culture. It appears from Gusfield and Schwartz's account that the American blue-collar worker uses the image of the "better shake," an image which presumably reflects a conception of life as a lottery or as a game with dice. So the factual hypothesis has a respectable working-class ancestry: jobs are there to be bled for what they are worth, it's a boss's world, don't let them make a mug of you, etc. However, the very existence of these common hortatory injunctions implies, or at least suggests, that some members of the working class are inclined to see intrinsic worth in their jobs and to adopt a sacramental attitude to work. So while our starting hypothesis had its roots in working-class culture, it would be a mistake to suppose that that culture does not acknowledge the existence of aspects of prestige involving status.

The alternative to the general desirability hypothesis was voiced by a fairly typical member of the British status group of liberal intellectuals. T. H. Marshall wrote (1953, p. 45): "But an occupational ranking cannot be arrived at by the mere recognition of objective facts, as in the case of ranking by income. There must be a value judgment, and it is based on the conventional values current in the society." So Marshall holds that the

social standing of an occupation is some sort of average of its pay-off and its noneconomic contribution to society (see R. H. Turner [1958] for an American expression of the same position); this view presupposes that the two elements (or rather people's perceptions of them) are less than perfectly correlated. (It further implies that national or class differences in the ranking of occupations, after correction for differences in their factual rewards, may reflect differences in social value systems.)

In his report on the NORC inquiry, Reiss (1961, pp. 34 ff.) pointed out that people at the bottom of the social scale tend to emphasize the financial reward of a job, while those at the top emphasize the value to society. The factual and the liberal hypotheses are merely reflections of these two poles. The question they provoke is this: Does the difference between them reflect the existence of differing cognitive maps of the occupational structure, or does it reflect something else, such as, perhaps, the direction in which people would most like to move through that structure? No elaborate argument is required to establish that people who are short of money tend to see the world in monetary terms. At the other end of the scale, the disdain of British liberal intellectuals for the pursuit of solely material ends may be traced in part to the origins of that status group in evangelical religion (Annan 1955). But the question remains, Can people view the occupational structure from a common standpoint? Is there a social consensus, and does it comprehend both factual and normative elements?

Over against the liberal and the factual hypotheses is a third hypothesis, which denies the presupposition of the liberal position and asserts that "it doesn't matter what you ask people to grade occupations on, the gradings will be the same on every dimension. All the apparent differences will be due to error or ignorance." In our monograph (Goldthorpe and Hope 1974) we referred to this as the Duncan-Artis hypothesis (cf. Kraus, Schild, and Hodge 1978). It is possible to derive this third view from a functionalist theory of society: both value and rewards reflect contribution to a society's survival and prosperity, and so they are not distinct. Or the view might be simply an induction from the finding that gradings of occupations in response to different stimulus phrases are moderately correlated with one another, the implicit assumption being that the observed correlations are little lower than the unreliability of the gradings, so that correction for attenuation due to unreliability would raise all correlations close to unity.

Empirically, there is no way to separate out the concomitants of two perfectly correlated variables. Before the liberal intellectual protagonist, with his belief in norms as well as facts, can come to grips with his quasi-Marxist antagonist, with his belief in facts alone, it is necessary to impugn this alleged perfect concordance among dimensions of grading. In order to do so we must turn our attention away from the correlations and focus on the "alienations," or deviations from perfect correlation (the square-root

complement of the correlation coefficient, $\sqrt{(1 - r^2)}$, is sometimes known as a coefficient of alienation). By application of analysis of variance it is possible to show that the departures from correlation among occupational gradings, so far from being entirely a matter of error, are highly structured and can be disaggregated into a set of additive terms, each of which has a precise signification. In particular, it can be shown that graders make a strong distinction between the pay-off of an occupation and its social value; gradings of occupations on standard of living correlate only about 0.42 with gradings on value to society.

Another finding is that there is virtually no sign of disagreement among classes on any of the dimensions on which occupations are graded, and this social consensus extends to value to society. So if classes do have different value systems the latter do not manifest themselves in the ordinary occupation-grading task.

Similar remarks may apply to national differences: members of a graduate class I taught at the University of Arizona in 1976 replicated the original experiment (Goldthorpe and Hope 1972) on a nonrandom sample of 10 informants and obtained astonishingly similar results. When gradings on the four dimensions are standardized (i.e., centered on a mean of zero and normed to a common standard deviation) and averaged, separately for Oxford and Tucson, the correlation between the averages (representing social standing) is 0.957. The correlation between the two towns for standard of living is 0.942. The correlations for power and influence and prestige are slightly higher, while that for value to society is somewhat lower (0.852).³

Finally, it was shown that, when one sample of informants grades occupations on four different dimensions and another sample grades the same occupations on the single dimension of social standing, the mean vector of gradings supplied by the second sample is virtually identical ($r = 0.98$) with the mean vector of the first sample. This implies that social standing, as ordinarily assessed, may be viewed as some sort of average of assessments made along several dimensions. The following section of this paper is devoted to a detailed examination of the meaning of this claim and of the evidence for it. The argument is that the social standing of occupations is a simple average of the factual and normative dimensions represented by, respectively, the standard of living which an occupation affords to its incumbent and the value which accrues to society from his prosecution of it. In other words the liberal intellectual is right and the general goodness

³ There is a hint here of a possible normative discrepancy. However, the Tucson sample was made up of students and secretaries, while the Oxford sample contained a broader social range. So, if there is a discrepancy, it is not clear whether it is due to class or to nationality. Clearly a proper international replication is called for (cf. Treiman 1977), but this need not involve large numbers of respondents.

inference from working-class expressions is wrong. Since one of our studies (admittedly carried out on a small and unrepresentative sample) showed that gradings on prestige are also very close to the centroid of the grading space, I take social standing and prestige to be synonyms.

DETAILED DISCUSSION OF THE EVIDENCE

Having established the broad outlines of the argument and having shown its relevance to the study of stratification, I turn now to an assessment of the evidence. This is derived from four samples listed here.

Pilot Sample

Initially, 10 people graded 40 occupations on four dimensions; they repeated the task on a second occasion. The purpose of the exercise was to explore the value of analysis of variance for the clarification of the structure of occupational gradings. It was a study in conceptual analysis (Goldthorpe and Hope 1972).

Oxford SIQV Sample

A similar task was carried out by 160 people drawn at random from the Oxford electoral register. The four dimensions were: standard of living (*S*), power and influence over other people (*I*), level of qualifications (*Q*), and value to society (*V*). The sample was analyzed as a whole and was then split (*a*) by social class, (*b*) by sex, and (*c*) by age. Differences between classes, sexes, and age groups were all negligible. Results for the four classes (upper, middle, lower, and students) were reported in detail in Goldthorpe and Hope (1974, appendix A) and are reproduced here in figure 1.

Oxford Social Standing Sample

In this sample, 65 people drawn at random from the Oxford electoral register graded the same occupations on the single dimension of social standing (Goldthorpe and Hope 1974, appendix A).

Construction of Hope-Goldthorpe Scale, National Sample

This conformed to a complicated design which was quite different from that of previous studies, enabling us to obtain gradings of 860 occupational titles representing 124 categories of jobs. The only point which need be

noted here is that informants were asked to grade occupations on social standing; therefore, the Hope-Goldthorpe scale is an ordinary occupational prestige scale.

The gist of the argument in this section is contained in figure 1. The diagram is a polar coordinate projection (Hope 1968, chap. 5). We must imagine that we are looking down on the North Pole of a sphere. The circles are lines of latitude, drawn out only as far as 40° from the pole at the center of the figure (the equator would lie at 90°). Points marked x on the spherical surface represent the ends of diameters, each of which necessarily passes through the center of the sphere. A diameter stands for a vector of 40 numbers, being the grades (scaled to a sum of squares of one) assigned to the 40 occupations. The diameters whose endpoints are marked on the sphere represent, not gradings by individuals, but means of such gradings. These mean vectors are also standardized to unit length.

Any two diameters form a certain angle when they cross at the center

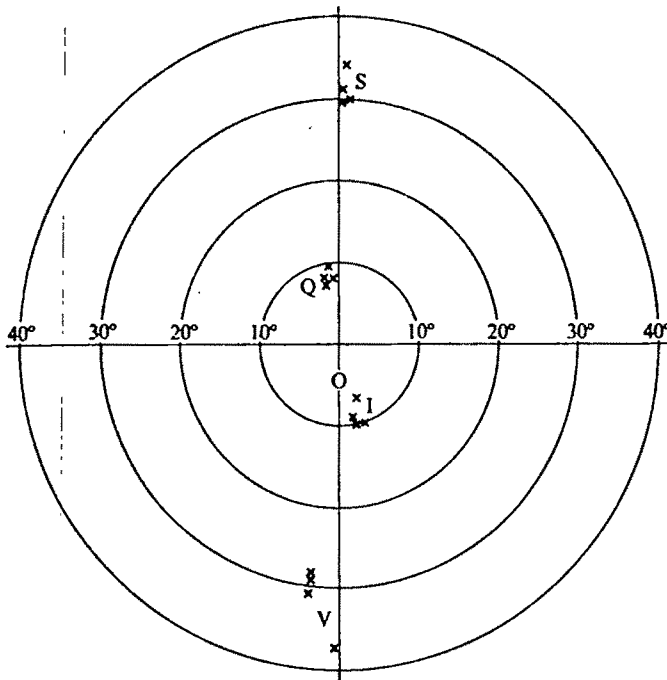


FIG. 1.—Each cluster of four points is a plot of the projections of four vectors on a spherical surface. The vectors represent the means of occupations graded on an attribute by four classes of respondent. The points within a cluster are all so close together that the classes are not separately labeled. The four clusters represent gradings on standard of living (*S*), power and influence (*I*), level of qualifications (*Q*), and value to society (*V*). Reproduced from Goldthorpe and Hope (1974), p. 17, by permission of Oxford University Press.

of the sphere. The cosine of that angle is equal to the correlation between the two vectors represented by the diameters. Correlations may be read off the figure by noting the angular separation of the endpoints of diameters. For example, the vectors associated with the letters *S* and *V* lie slightly more than 60° apart, and reference to a table of cosines tells us that this angular distance implies a correlation of somewhat less than 0.5 between gradings on standard of living and gradings on value to society.

Four points are associated with each of the letters *S*, *I*, *Q*, and *V* because the Oxford SIQV sample was broken down by occupation of informant into four classes: upper, middle, low, and students, and the data for each class were analyzed separately. It is essential to the argument to note that virtually all the variance of each set of 40 mean gradings lies in the three-dimensional space of the polar coordinate projection. In particular, R^2 for the regression of *O* (which stands for the mean of the 65 respondents who graded the social standing of the 40 occupations) on the 16 vectors of the SIQV sample is 0.97. The vector represented by *O* correlates 0.98 with the unweighted mean of these 16 vectors (Goldthorpe and Hope 1974, p. 16).

In principle, the data project into 17 dimensions: one supplied by the mean gradings of the Oxford social standing sample (*O*), the other 16 supplied by the mean gradings on *S*, *I*, *Q*, and *V* by each of the four social classes. But the overwhelming bulk of the variance of each of the 17 mean vectors lies in the space of figure 1. The North Pole of that diagram represents the first principal component of the 16 vectors of the SIQV sample, and the axes at right angles to the polar line (and to one another) represent the second and third components. These three components account for 97% of the overall variance of the 16 *S*, *I*, *Q*, and *V* vectors. Thus, the cosines of angles formed by the vectors where they cross at the center of the globe almost exactly reproduce the correlations among those vectors in the 16-dimensional space as a whole, that is, the ordinary zero-order correlations among the mean gradings. As already indicated, 97% of the variance of the seventeenth vector (social standing represented by point *O*) projects into the space of the diagram.

The two outstanding features of the diagram are the wide extent of separation among the four dimensions, *S*, *I*, *Q*, and *V*, and the closeness with which the four classes of informant huddle together in grading any one dimension. To take the first point first: it must be emphasized that the discrepancies between *S* and *V* are very wide indeed. And they are real discrepancies, which cannot, in any appreciable degree, be attributed to error, because no vector is derived from fewer than 24 graders.⁴ Because

⁴ A sample of 24 may not seem very large. But one of the presuppositions of work on the Hope-Goldthorpe prestige scale is that it is misleading to think of graders as objects of inquiry when they are, in effect, functioning as agents, informants, or "tests." Two dozen may be a small sample, but it is a large number of parallel tests

the correlations are scarcely attenuated by unreliability, we can choose to say either that S and V correlate about 0.45 or that they alienate (perhaps "disrelate" would be a better term) 0.90. These two statements are equivalent because they are simply alternative ways of partitioning an angle of 90° into complementary cosines.

Now to turn to the second point: the discrepancies among classes in grading any one dimension are very small indeed. An overall measure of the extent of agreement is the intraclass correlation 0.965, which represents the degree of consensus among the four classes across the whole "hand" of dimensions. This value is lower than it might have been because the students lie somewhat apart from the other three classes in their assessment of value to society, but, so far from being at odds with the others they appear merely to be expressing the same distinction as the rest, but in an exaggerated form.

The letter O (which does not have a cross associated with it in fig. 1) stands for the mean vector of the sample which graded the 40 occupations on social standing. The important fact about it is that it lies nearly halfway between S and V . Accepting this as a fact, what can we infer from it about the nature of occupational grading?

If we set out from the proposition that, whatever else they are doing when they grade occupations, respondents are taking into account the standard of living which those occupations sustain, then because standard of living lies about 30° from O in one direction (and because the social standing axis is almost perfectly predictable from the other four axes in the space) we must look for one or more axes which would balance out S in people's minds when they are deciding on the social standing of an occupation. If there were no such counterbalancing considerations, social standing would be identical with standard of living. The simplest supposition is that standard of living is weighted against value to society in arriving at a judgment of social standing. This corresponds to Gusfield and Schwartz's (1963) case in which assessment of occupations contains both

or variables. The point was made in *The Social Grading of Occupations* (Goldthorpe and Hope 1974, p. 174) that "assessments with a reliability in excess of 0.9 may be achieved by averaging gradings over no more than $k = 10$ respondents when the individual respondent has a reliability in the region of $\bar{r}_{ii} = 0.6$." Most of the informants in a typical prestige-grading inquiry are in fact wasted in a nugatory attempt to whittle away the last 5% of grader error variance, while other sources of error are permitted to flourish unchecked. It was this conceptual realignment which enabled the national inquiry to obtain gradings of 860 occupations from 620 informants in a manner which also furnishes useful estimates of various sources of error. There can be no doubt that individual social actors perceive subsets of occupations in all sorts of idiosyncratic ways (Coxon and Jones 1978). The questions I have attempted to answer are these: To what level of analysis do we have to descend before the constancies break down? And what is the relative importance of the distinguishable components of departure from consensus?

factual and normative elements, that is, it represents the liberal view of the nature of prestige.

Of course, an objector might choose to assume that standard of living is not taken into account in judgments of social standing, but that respondents who are asked to grade the social standing or prestige of occupations confine themselves to operating in a fairly restricted Arctic circle near the North Pole of social standing. This supposition is not very plausible however: respondents, particularly working-class respondents, do often refer to pay and "perks" when they are asked what considerations sway their judgments of jobs. The occurrence of such idioms as Gusfield and Schwartz's "better shake" reinforces the commonsense observation that the material prosperity accruing to an occupation is a culturally enshrined dimension for the assessment of that occupation (cf. Coxon and Jones 1978).

Let us agree, then, that respondents almost certainly use standard of living in grading the social standing of an occupation. Can we infer from the diagram that they must of necessity also be using value to society? Could it not be argued that the same gradings on social standing could result if they used *two* other axes which, with *S*, formed the apices of a great-circle triangle on the spherical surface of the figure, *O* being at its center? This argument is perfectly sound, but it does not in fact entail that *V* is not being taken into account. If we know that (*a*) social standing lies almost entirely within the space of the diagram (as it does), (*b*) axes as eccentric as *S* are involved in the assessment of social standing (and we have shown that *S* is so involved), and (*c*) respondents are able to grade on an axis *V* whose degree and direction of eccentricity almost exactly balance those of *S*, then it becomes difficult to argue that considerations of social value are not influencing the assessment of occupation. Even if two other vectors were to be substituted for *V* in order to counterbalance *S*, the direction in which they exerted their effects would, in sum, have to be the direction in which *V* deviates from the centroid, and the magnitude of the effect would have to approximate to the distance which separates *O* and *S*.

The argument starts from the propositions that (*a*) people take *S* (standard of living) into account in grading *O* (social standing or prestige), and (*b*) *S* lies 30° of arc away from *O*. Both propositions have good empirical backing. The argument then goes on to ask what other dimension complements *S* in such a way that the two sum to social standing. It looks for the complementary dimension in the region of space which lies 30° of arc on the far side of social standing, rather as an astronomer might look for a hitherto unobserved mass which is disturbing the orbits of known bodies. Implicit in the argument is an assumption about cognitive space to the effect that people do not employ complex weighting equations in arriving at their general assessments of occupations. In the absence of this assump-

tion it has to be allowed that any two vectors on which social standing can be regressed with an R^2 of one would suffice to define social standing.⁵

Granted the assumption, the preceding argument seems watertight within the limits set by a reading of the figure. The main criticism which might be brought against it is that the figure is, for one reason or another, inaccurate. It would not be plausible to appeal to random error in the positioning of S , I , Q , and V , since those axes are very close together in the four subgroups of the random sample of 160 whose axes are reported in the diagram. It might, however, be possible to impugn the position of O on these grounds. It might also be claimed that the population from which the diagram was derived (the electors of the city of Oxford) was too particular to form a basis for such precise quantitative inferences. Clearly, replication on other populations is called for. However, the most plausible criticism which can be advanced against our interpretation of the figure is that gradings on the four axes were elicited with axes being administered to graders in a constant order. There might well be order effects on the relative positions of axes. It should also be mentioned that the effect of grading across axes instead of across occupations is unknown. Given the small number of respondents required to obtain a reliable mean for any one axis, it would not be too costly in future studies to avoid order effects altogether by administering only a single axis to each respondent.⁶

PRESTIGE = STANDARD OF LIVING + VALUE TO SOCIETY

Because of the reasons advanced above for supposing that prestige is a function of just two dimensions along which occupations vary, I provide, in table 1, a reanalysis of the Oxford SIQV data from which the two redundant dimensions (power and influence and qualifications) have been omitted. In all other respects the analysis replicates that in table A2 of *The Social Grading of Occupations*. The relevant comparison is between the two sets of variance components, one (SV) from the present analysis and the other ($SIQV$) from the analysis of all four dimensions.

The first difference to note is a substantial drop in the component for occupations. What this tells us is that the average correlation (not corrected

⁵ The astronomer is able to locate the approximate position of a disturbing mass because he has a simple, strong theory about how bodies act upon one another in physical space. But he, or rather his predecessor Newton, arrived at that theory only through a process of working with highly simplified and unrealistic assumptions (Lakatos 1974, pp. 134 ff.). If sociological theory means anything, it must signify a willingness to "chance one's arm" with very simple hypotheses which have determinate empirical consequences instead of following the data passively.

⁶ It might be argued that not all axes have the same variance (or equivalently, that some axes are given greater weight than others). To tackle these more sophisticated questions future experimenters will have to resort to more refined methods of scaling.

TABLE 1
ANALYSIS OF VARIANCE OF GRADINGS OF 40 OCCUPATIONS ON TWO DIMENSIONS (Standard of
Living and Value to Society) BY 160 RESPONDENTS ON TWO OCCASIONS

Source	df	Sum of Squares	Mean Square	Variance Component (SV)	Variance Component (SIQV)
Occupations.....	39	9,579.51	245.6286	.2446	.4236
Occupations by respondents.....	6,201	4,847.87	.7818	.0562	.0811
Occupations by occasions.....	39	13.89	.3561	— .0021	— .0002
Occupations by dimensions.....	39	3,491.18	89.5173	.2766	.1349
Occupations by respondents by occasions.....	6,201	1,826.42	.2945	.0044	.0242
Occupations by respondents by dimensions.....	6,201	3,399.55	.5482	.1312	.0859
Occupations by occasions by dimensions.....	39	29.37	.7531	.0029	.0011
Error.....	6,201	1,772.21	.2858	.2858	.2492
Total.....	24,960	24,960.00		.9996	.9998

NOTE.—Each set of 40 gradings is standardized to a mean of 0 and a sum of squares of 39. In the calculation of variance components it is assumed that all effects are random. The last column is reprinted from Goldthorpe and Hope (1974), table A1.

for reliability) between respondent A grading *S* and respondent B grading *V* is 0.24. In table 2 this value is recorded as r , without subscript. The occupations by respondents term, accounting for 5.6% of the variance, tells us that there is a certain amount of disagreement between respondents in their assessment of the overall prestige of an occupation. Adding this to the base value of 24.5% gives the coefficient $r_r = 0.30$ in table 2. This represents the average correlation between gradings of *S* and gradings of *V* supplied by the same respondent. The largest single term in the analysis apart from error (27.7%) reflects shared discrepancies between the *S* and *V* dimensions of occupation. Hence the average correlation between two different respondents each grading on the same dimension is $r_d = 0.2446 + 0.2766 = 0.52$. Finally, there is some disagreement over the discrepancies, and this is reflected in the occupations by respondents by dimensions component (13.1%). Adding this to the other variance components gives $r_{rd} = 0.71$, which is the average correlation between two sets of gradings by the same respondent on the same dimension.

The three variance components which reflect systematic change from one occasion of grading to the next (the grading task was administered on two occasions separated by an interval of three months) are all so close to zero as to be negligible. So we may ignore them and treat $1 - r_{rd}$ as an estimate of error and r_{rd} as a reliability coefficient. The corrected coefficients show that the angle between *S* and *V* when these are graded by different people is 70° ($\cos^{-1} 0.3452$), which shrinks to 65° ($\cos^{-1} 0.4245$) when the same person is doing the grading on both *S* and *V*. The variance around either *S* or *V* is quite wide: the average angle between the gradings of two different persons on the same dimension is $\cos^{-1} 0.7355 = 43^\circ$. Figure 1 assures us, however, that this noise is not socially structured, since it cancels out within quite small samples of respondents and is not reflected in any noticeable differences between the mean gradings of different classes. We conclude that, when they grade the social standing or prestige of occupations, people record a balance of normative and factual judgments. If

TABLE 2
ELEMENTS OF A POOLING SQUARE

	UNCORRECTED		CORRECTED FOR UNRELIABILITY	
	<i>SV</i>	<i>SIQV</i>	<i>SV</i>	<i>SIQV</i>
r2446	.4236	.3452	.5838
r_r3008	.5048	.4245	.6957
r_d5212	.5586	.7355	.7698
r_{rd}7086	.7256	1.0000	1.0000

NOTE.—See Thomson (1951) regarding pooling squares. The elements in the first two columns are sums of variance components in table 1. These have been divided by r_{rd} to give the elements in the last two columns.

this conclusion is not startling, it must be said that this analysis has also established the logical basis without which it is not even possible to state it, namely, the distinction between judgments of fact and judgments of value. Some workers have been inclined to assert that the perceived occupational hierarchy is unidimensional, and most workers have carried on their research as if this were so. (Exceptions are Rossi and Inkeles [1957], Sarapata and Wesolowski [1961], and Yuchtman and Fishelson [1972].) A little thought shows that establishment of the very considerable degree of divergence between factual and evaluative modes of assessment has restored to the general public a set of social constructs of which social researchers had formerly deprived them. A correlation as low as 0.42 between two primary dimensions of social assessment gives to social actors a very considerable degree of latitude in the differential transmission of culture and advantage, and it imposes on the student of stratification the necessity for treating occupation as a variable lying in at least two dimensions. And, of course, once a variable has been shown to project into two dimensions, it may be analyzed in any of the infinite number of dimensions which may be formed by combining the original two.

It may be noted that the correlation of 0.42 between normative and factual dimensions lends no support whatever to a functionalist interpretation of the distribution of rewards. Nor, however, does it tell against such a view, because we have no right to assume, without further evidence, that value to society is generally believed to be the proper basis for the determination of rewards. The distribution might, in principle, be sanctioned by some quite different normative or factual dimension.

Grasmick (1976) has reported an alternative approach to the analysis of occupational distinctions from which he, too, infers that value to society is a significant component of prestige. His method involved asking subjects to report their assessment of the similarity of occupations, pair by pair, in terms of their prestige. The mean similarity matrix for 25 occupations was subjected to smallest-space analysis. The component for a one-dimensional solution correlated 0.98 with prestige assessed in the usual way. Grasmick interpreted the second component of a two-dimensional solution as a contrast between occupations with high and low value to society. The number of occupations on which this interpretation is based is quite small, and the interpretation leans heavily on Goldthorpe and Hope (1972), so, although the results are in line with the argument of the present paper, they cannot be adduced as entirely independent testimony.

Kraus, Schild, and Hodge (1978) report a similar investigation from which they conclude that there is no second dimension of occupational dissimilarity. However, when they compare selected subgroups of their sample (e.g., men vs. women, or highly educated vs. poorly educated) they obtain correlations between second dimensions which are not negligible and which

are somewhat larger than the analogous correlations for random subgroups. The latter may be relatively depressed by the fact that the 25 occupations analyzed were, when the gradings were obtained, mixed in with a set of 65 occupations, and whereas the 65 were entirely different between random subgroups, there must have been some overlap between the nonrandom subgroups. There is the further consideration that informants were asked to group occupations in terms of their similarity. If informants chose to group in terms, say, of averaged standard of living and value, much of the second dimension would be lost within the groups. This study provides excellent evidence for the existence of a prestige ordering: it does not tell at all strongly against the existence of other orderings.

WHAT IS PRESTIGE?

In this final section I examine an ambiguity in the meaning of "prestige," an ambiguity which causes confusion whenever the normative component of the concept is under discussion. It stems from the fact that in its earliest, European, usage, "prestige" commonly applied to princes and ambassadors and heads of colleges, men, that is, who united power or money with elaborate ceremony and ritual. Their positions were honorific roles which were also cynosures.

Although the word "prestige" did not become part of ordinary English usage until after his time, Macaulay ([1827] 1889) described the phenomenon admirably when he wrote, in his essay on Machiavelli:

The people [of Italy] had observed the whole machinery of the church, its saints and its miracles, its lofty pretensions and its splendid ceremonial, its worthless blessings and its harmless curses, too long and too closely to be duped. They stood behind the scenes on which others were gazing with childish awe and interest. They witnessed the arrangement of the pulleys, and the manufacture of the thunders. They saw the natural faces and heard the natural voices of the actors. Distant nations looked on the Pope as the viceregent of the Almighty, the oracle of the All-wise, the umpire from whose decisions, in the disputes either of theologians or of kings, no Christian ought to appeal. The Italians were acquainted with all the follies of his youth, and with all the dishonest arts by which he had attained power. They knew how often he had employed the keys of the church to release himself from the most sacred engagements, and its wealth to pamper his mistresses and nephews. The doctrines and rites of the established religion they treated with decent reverence. But though they still called themselves Catholics, they had ceased to be Papists.

Macaulay, who took a dramaturgical view of history, treated the rituals associated with kingship as so many pageants which serve to focus public attention and bind popular sentiment.

But kingship was subject to change. The visual representations which

set James I in the center of a pantheon were replaced, in the reign of William and Mary, by classical scenes in which the principal actors were, not living monarchs, but mythical gods and heroes. The process was taken still further as the "efficient" parts of the constitution became disjoined from the "dignified." "The Crown," said Bagehot, "is the fountain of honour; but the Treasury is the spring of business" (Bagehot 1974, chap. 1).

The first American president was not unaware of the value of pomp and ceremony. However, by stepping down from the presidency, Washington enhanced his standing in the eyes of all liberal people and ensured that "prestige," in the sense described, would never have an important place in American society. Young (1966, p. 54) writes: "George Washington, the President with the greatest prestige in the nation before the Civil War, 'got into one of those passions when he cannot command himself . . . [saying that] he had never repented but once the having slipped the moment of resigning his office, and that was every moment since [he had taken it], that by God he had rather be in his grave than in his present situation. That he had rather be on his farm than to be made emperor of the world, and yet that they were charging him with wanting to be a king.'" Jefferson came down firmly on the side of republican simplicity.

Prestige, in its older sense, accrues to certain honorific positions which are deferred to because they are the bearers of central social values and because deference behavior is an important part of the show. Shils's (1968) analysis applies to a society in which such roles exist. Proximity to central social values is, Shils tells us, the essential condition of "deference-entitlement." And a style of life is deferred to, not because it is easy or opulent, but "because it is a pattern of conduct which is a voluntary participation in an order of values."

Now at first sight it might seem that this analysis should lead us to suppose that occupations high in value to society are those with a strong title to receive deference. But this supposition would be erroneous, for value to society is not to be confused with nearness to central social values; the efficient is not to be confused with the dignified. High value to society is ascribed to jobs which are functional in the pursuit of normatively approved ends such as health, safety, learning, and the relief of distress. Centrality is, I suggest, more a matter of the maintenance of the political fabric, especially by the exercise of public offices which have a representative function. Monarchs, ambassadors, chief justices, regius professors, leaders of the opposition, even registrars general, are central and therefore prestigious. Value to society is, more mundanely, efficacy in the protection and maintenance of individual members of society. Ambassadors are not regarded as being especially valuable, but they rank very high in prestige.

The ambiguous status of pop stars and football players helps to illuminate the nature of deference and its relation to values. Their roles are the cynosures of millions and the leading parts in the continuous pageant which unfolds on our television screens. They therefore possess an important element of prestige, namely, the capacity to command attention by the dramatic exposition of a role. But it is generally held that they do not deserve deference, for their roles are neither central nor representative. They have notoriety but not eminence.

Prestige in the old sense is the same as social standing or social honor, and ascribing it is a way of saying that incumbents of a certain role tend to command more attention than other people and perhaps are accorded more deference. So far as ordinary, nonhonorific jobs are concerned, the sort of jobs which are graded in a typical occupational grading inquiry, prestige means something different: it is virtually an average of the rewards of a job and the value ascribed to it.

But does high prestige attract deference? The answer depends on whether we look at the small number of central, honorific roles, or at the ordinary roles of most people. When we make up a list of job titles for our occupation-grading studies we are dealing with jobs which are almost all in some degree remote from the center. Thus the scope for deference claims is rather restricted, and the constraints have latterly been rendered more severe by the spread of education and of functional interdependence among technically based jobs.⁷

The idea that there is some classical sociological sense in which prestige is intimately related to displays of deference is set out in a paper by Lockwood (1966), who holds that the deferential worker, like the middle-class person but unlike other working-class people, views society as a prestige or status hierarchy. If this is the origin of the idea that prestige means deference-entitlement, we can see immediately where the argument of Goldthorpe and Hope (1972) has gone wrong. The logic of the position is confused because two arguments, both of them invalid, have been run together. One argument goes as follows: classes differ in their value systems; they do not differ in ascribing prestige to occupations; therefore, prestige cannot be a reflection of values. The argument would hold good only if we knew for a fact that classes differ in that part of their value system which relates to the evaluation of occupations. But our subsequent data (Goldthorpe and Hope 1974, appendix A) showed that value to society is evaluated in a constant manner across classes. So there is at least one aspect of social

⁷ A standard 19th-century argument against upward mobility was that the man of "servile" origins would, when placed in a position of authority, act the part of a tyrant because he had not learned to practice self-restraint in the expression of desires and sentiments, that is, he would seek to extort deference. This was thought to be bad for liberty; today it is inimical to efficiency.

evaluation on which classes are agreed, and that aspect is a component of prestige.⁸

The other argument goes like this: people who act deferentially or expect to receive deference, rank occupations in terms of prestige or status, so prestige *means* the right to receive, or perhaps the probability of receiving, deference. Adapting Dr. Johnson, we might characterize this as the "who himself is fat, should drive fat oxen" argument.

Deference to honorific roles does not have a great deal to do with occupations as we understand them. Indeed the rank of gentleman, which was a 19th-century Englishman's guarantee of some degree of consideration in the eyes of the lower orders, was quite explicitly not an occupation status. Prestige, in the old honorific sense, commanded deference, and deference behavior was part of the show which bolstered the prestige. But prestige as it is understood in our occupation-grading inquiries is a more democratic concept; it has a strong normative component, but that component relates much more to the nonpecuniary services which a man or woman performs on behalf of the community than it does to questions of deference. Prestige in the old sense still sanctifies parts of the English elite, and the process by which it is maintained, conferred, and withdrawn is a fascinating one. But occupational-grading inquiries tell us nothing about it. What the analysis of this paper has established is that there exists a normative consensus which is based on respect for intangible contributions to the well-being of citizens. This is arguably a more powerful source of legitimation of the social order than the kind of fragile charade described by Lord Macaulay.

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⁸ A referee comments that we know classes have different values, and so the interclass consensus on value to society suggests that the normative component of prestige may be trivial. That *may* be so, but there is no particular reason to equate the socially agreed with the trivial.

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Sexual Taboos and Social Boundaries¹

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The strong taboos against homosexuality, bestiality, and transvestism that exist in many Western societies are the result of attempts to establish and defend strong ethnic, religious, or institutional boundaries. If religious, military, or political leaders decide to strengthen the boundaries of their group, they tend in consequence to impose harsh penalties on forms of sexual behavior that breach social or symbolic boundaries. This may be because they have adopted a code of belief and conduct that emphasizes the need to maintain boundaries of all kinds, including those between humans and animals and between males and females, or because they wish to prohibit the formation of sexual relationships that cut across the internal and external boundaries of their group or organization. The principal groups and institutions examined are the Old Testament Jews, the Parsees, ancient Greek states and their armies, early and medieval Christianity, the German National Socialist movement, and the modern British armed forces.

The strong taboos against such forms of sexual behavior as homosexuality, bestiality, and transvestism that characterize the societies of Europe and North America have long proved something of a mystery. They cannot have a general psychological or biological origin, for many other societies have much weaker taboos against such behavior or even lack them altogether (Carstairs 1964, pp. 419–33; Ford and Beach 1952, pp. 125–58; Gebhard 1970, pp. 214–15; Suggs and Marshall 1971, pp. 229–31; West 1977, pp. 132–36). Nor is their origin economic, for neither society as a whole nor any powerful group within it gains any economic or material advantage from the enforcement of these taboos. The strength of the taboos in Western societies and the severity with which infringements have been punished suggest that such forms of behavior as homosexuality, bestiality, and trans-

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vestism have been interpreted by the societies concerned as "injurious to the whole community" (Brown 1952, p. 142)² instead of merely to individuals or their families. A detailed examination of the history of a number of Western societies and institutions shows that the origin and maintenance of these taboos stem from the fact that these forms of behavior have been perceived by religious and military leaders as a threat to crucial social boundaries. They have sought to punish the homosexual, the transvestite, and to a lesser extent other sexual deviants both as a means of reinforcing the distinctive identity of a group by emphasizing its boundaries and as a means of maintaining the boundaries between the different layers of a military or religious hierarchy.

The earliest examples of these processes within the Judeo-Christian tradition on which European and American culture is based (Weinburg and Williams 1974, pp. 17-18; West 1960, pp. 64-65, 70-72) are to be found in the Old Testament. The biblical texts that deal most clearly with the taboos against homosexuality and bestiality and the reasons for them appear in the Book of Leviticus.

You shall not lie with a man as with a woman: that is an abomination. You shall not have sexual intercourse with any beast to make yourself unclean with it, nor shall a woman submit herself to intercourse with a beast: that is a violation of nature. You shall not make yourselves unclean in any of these ways, in these ways the heathen . . . made themselves unclean. [Lev. 18:22-24]³

If a man has intercourse with a man as with a woman, they both commit an abomination. They shall be put to death; their blood shall be on their own heads. [Lev. 20:13-14]

A man who has sexual intercourse with any beast shall be put to death and you shall kill the beast. If a woman approaches any animal to have intercourse with it you shall kill the woman and the beast. [Lev. 20:15-16]

The justification for the savage penalties threatened in the latter texts is clearly set out in the first passage quoted. The purpose of the taboos is to set apart the Jewish people, the chosen people, from their heathen neighbors. The taboos help to maintain and reinforce the boundaries of the group and enable it to retain its distinctive identity under adverse circumstances. The reason for such taboos is made even more explicit later in the law of

² Brown calculated that "the average punishment of men for homosexuality in societies conceiving the act as harming the community is 3.1 in contrast to an average of 1.2 for societies not believing this to be true" (1952, p. 142). The punishments are rated for severity on a four-point scale where 1.0 = a small fine, 2.5 = a public flogging, 2.9 = multiple mutilation, and 3.9 = death (see Brown 1952, p. 137).

³ All biblical quotations are from the New English Bible, Oxford and Cambridge University Presses.

holiness, in the Jewish dietary taboos which stress the distinction between clean and unclean animals:

I am the Lord your God: I have made a clear separation between clean beasts and unclean beasts and between unclean and clean birds. You shall not make yourselves vile through beast or bird or anything that creeps on the ground for I have made a clear separation between them and you declaring them unclean. You shall be holy to me because I the Lord am holy. I have made a clear separation between you and the heathen that you may belong to me. [Lev. 20:24-27]

The rules of conduct that make the Jews distinct from their heathen neighbors are not arbitrary, nor are most of them simply a rejection or inversion of their neighbors' practices. The dietary laws may in a particular case forbid to the Jews a food that is distinctively characteristic of their neighbors' diet and the sexual taboos may include a sexual practice routinely or ritually practiced by a neighboring people. However, the evidence for this is weak and contradictory (Bailey 1955*a*, pp. 59-60; 1955*b*, p. 44; Gray 1964, p. 15; Hooke 1938, pp. 26-27; Wooley 1936, p. 172) and certainly cannot provide an explanation for the rules and taboos as a whole.

The force of the taboos lies less in their content than in their structure, in the way they insist on the separation of categories so that the keeping apart of like and unlike is an everyday reminder of God's setting apart of the Jews, the chosen people, from the heathen, the lesser breeds without the law. This keeping apart of categories runs throughout Leviticus and is, as Mary Douglas (1970, pp. 54-72; 1975, pp. 207-9, 306-10), J. R. Porter (1976, pp. 82-93), and J. Soler (1973) have shown, the key factor in determining what may and may not be eaten. Douglas's analysis of the dietary rules as a connected system is far superior to the alternative ad hoc explanations of particular dietary taboos in terms of public health (Maimonides 1963, p. 598), ecology (Harris 1975, pp. 36-46), or demography (Bailey 1955*a*, p. 58; Darlington 1968, pp. 182-86; Harris 1975, p. 44).

The practical and utilitarian ad hoc explanations break down altogether in the light of a divine instruction such as the following: "You shall keep my rules. You shall not allow two different kinds of beast to mate together. You shall not plant your field with two kinds of seed. You shall not put on a garment woven with two kinds of yarn" (Lev. 19:19). These restrictions can confer no possible economic, medical, or demographic advantage on the people who keep them. Imposing a ban on the breeding of that invaluable but sterile hybrid the mule (Leighton 1967, p. 46)⁴ must have been a serious handicap for an agricultural people. Many of the rules obviously

⁴ Leighton notes that Lev. 19:19 meant that "the ancient Israelites were forbidden by law to breed mules. Consequently, the Israelites had to obtain their mules from other peoples" (Leighton 1967, p. 46; see also Ez. 27:19).

reflect the practical preoccupations of the Jewish people (Weber 1952, pp. 351-54), but they are in no sense subordinate to them.

Most of the rules of the law of holiness relate to the basic categories of the natural world and of human experience. Such categories as the living and the dead; mortal and divine; human and animal; air, sea, and land; male and female; past, present, and future are common to most peoples. They provide a framework of basic "natural" categories that render the universe meaningful. What is peculiar to the Jewish people is that these natural categories are also moral categories and anything that is ambiguous or threatens to blur the boundaries of these categories is treated as abominable. Hence the ban on the consumption of shellfish, which are not fully sea creatures or land creatures but live on the littoral margin of each, or on the eating of flightless birds, which do not belong properly to the air as birds should and yet are not proper land animals either (Douglas 1970, pp. 54-72). There is a similar explanation for the rules relating to the slaughter of animals for food which insist that the blood (the life) must be removed from the meat (dead and hence consumable) before it is eaten (Lev. 17:10-14).

We can see also why sorcery, necromancy, and witchcraft are forbidden (Ex. 22:18; Lev. 12:26-27, 20:6-7; Deut. 18:9-15; 1 Sam. 15:23, 28:7-20; 2 Chron. 33:6) and why "any man or woman among you who calls up ghosts and spirits shall be put to death" (Lev. 20:27). Such people are dangerous because they break down the division between the living and the dead or between the present and the future (Is. 8:19-22, 47:13-15).

The book of Leviticus makes explicit a central moral distinction that runs throughout the Old Testament—the Jews must either live in a world of carefully separated discrete categories (i.e., remain a people with a distinct identity) or face a world of utter confusion (Douglas 1970, p. 67; Davies 1975, p. 97). The biblical account of the creation involves the resolution of the world into clear categories from primeval confusion (Gen. 1:1-19),⁵ and the flood represents the return of that confusion as the separation of the land from the sea is eliminated. The building of the tower of Babel, an impious attempt to join together the separate categories of heaven and earth, is punished by the infliction of confusion on its builders, the beginning of the mutual unintelligibility of men's various languages (Gen. 11:1-9), an unintelligibility removable only by the divine gift of tongues (Acts 2:2-12).

It is now possible to provide a complete explanation for the harsh treatment of homosexuality, bestiality, and transvestism in the scriptures. These are all forms of sexual behavior which break down the boundaries between some of the most fundamental categories of human experience—the cat-

⁵ For a comparison with the myths regarding the creation and the flood of other Middle Eastern peoples, see Hooke (1963).

egories of male and female and human and animal. This is why homosexuality and bestiality are condemned in Leviticus and why in Deuteronomy God tells the people of Israel through his prophet Moses: "No woman shall wear an article of man's clothing, nor shall a man put on a woman's dress; for those who do these things are abominable to the Lord your God" (Deut. 22:5).

It is easy to see how transvestites break down the categories of male and female, but the situation is slightly more complicated in the case of homosexuality. The essential point to grasp is that "male" and "female" are complementary categories, each defined in relation to the other. The male is by definition complementary to the female and only remains male so long as his sexual behavior relates exclusively to females. Any sexual behavior directed by a biological male toward another male will (at any rate so far as the scriptures are concerned) automatically place him in the same category as a female, for whom this is the normal sexual orientation.⁶

Because homosexual behavior involves a person placing himself or herself in the wrong sex category it erodes the boundary between these categories. This is why homosexual behavior is linked in Leviticus with bestiality, a sexual practice which breaks down the division between the equally fundamental categories of the human and the animal (see also Epstein 1948, p. 135).

It must be emphasized that there is nothing natural or inevitable about this moral framework based on "natural" categories. The framework derives from the particular social structures and historical experience of the Jewish people. The text of the priestly work of Leviticus known to us today dates from a time during or shortly after the exile of the Jews in Babylonia (Porter 1976, pp. 2-6). If a people are to survive exile with their identity intact they must find a social substitute for the natural boundaries of terrain and distance that no longer isolate and insulate them from their neighbors. The laws of holiness were the Jewish answer to this problem. The priestly editors represented them as deriving from the time of Moses, when the people of Israel had undergone a period of exile and bondage in Egypt but escaped from it to settle eventually in the promised land. Leviticus is clearly rooted in much older traditions derived from the Jews' earlier experience of exile in Egypt (Porter 1976, p. 6).

Once again it must be stressed that there was nothing inevitable about the survival of the Jews as a people. Many peoples in history have been sent into exile and have simply disappeared. The unique achievement of

⁶ Note Lev. 20:13-14: "If a man has intercourse with a man as with a woman, they *both* commit an abomination" (emphasis added). This condemnation of both those who indulge passively in homosexual acts and those who are the active partners is characteristic of such civilizations as those of the Jews and the Zoroastrians, which exhibit strong and consistent taboos against homosexuality rooted in a hatred of boundary breaking.

the Jews was that they responded to their experiences of exile by developing a code of moral rules that enabled them to retain their separate identity in a hostile environment. There exists here what I will call a "social vaccination" effect. These early experiences of bondage in Egypt and exile in Babylon, which seemed to threaten the very identity of the Jewish people, in fact led them to evolve laws, traditions, and customs which later enabled them to survive as a distinct people despite conquest by the Greeks and the Romans and the centuries of further exile and persecution in the Diaspora. These early threats to Jewish identity were not strong enough to destroy it but were sufficiently strong to stimulate the formation of enduring social boundaries which then enabled that identity to survive the more drastic threats and problems of the subsequent millennia.

It is difficult to find another people who have survived the experience of galut in the same way as the Jews, in order to see whether other survivors possess similar sexual taboos. Indeed, the historian and classical scholar Michael Grant (1973, p. 18) goes so far as to say: "This preservation by the exiled Jews of their national and religious habits amended by geographical circumstances but still intact is one of the strangest phenomena in history." However, some relevant parallels may be drawn between the history of the Jews and that of another "Western" people, the Parsees, who are often called "the Jews of India" (Duchesne-Guillemin 1961, p. 2).⁸ The Parsees left their homeland in Iran "1,200 years ago to save their religion, the teachings of Zoroaster, from being Islamized by the invading Arabians" (Kulke 1978, p. 13). Since that time they have lived in exile, mainly in Gujarat and Bombay in western India (Kulke 1978, pp. 23-40) and more recently in Britain and Canada. The Parsees, like the Jews, survived in exile by maintaining strict social boundaries between themselves and their neighbors (Dhalla 1914, pp. 323-25, 367-69).

Their religion was in many ways similar to Judaism and lent itself to the preservation of a separate Parsee identity.⁹ Zoroastrianism like Ju-

⁷ Before they went into exile in India, the Zoroastrians were very much part of the Western world and were well known to the other ancient peoples discussed here, namely, the Jews, the Greeks, and the early Christians (Moulton 1892).

⁸ The phrase "the Jews of India" refers primarily to the commercial acumen of the Parsees in modern India, but it is significant that when the Portuguese Garcia d'Orta visited India in 1535 he mistook them for Jews (Kulke 1968, p. xvi).

⁹ Like Judaism, Zoroastrianism has many universal aspects, but it is preeminently the religion of a "people." Even before they were forced into exile the Zoroastrians experienced alien rule and cultural penetration. Like the Jews, the Zoroastrians of Iran had to resist Hellenization under the rule of Alexander the Great and his successors (Dhalla 1914, pp. 12-13, 184-85, 367). As in the case of the Jewish people, the key to the survival of the Parsees has been their combination of a refusal to compromise on any question concerning the preservation of their identity and boundaries with a remarkable willingness to adapt and to innovate in all other areas (Kulke 1968, pp. xvi, xxi; 1978, pp. 28-29). They have made a remarkable contribution to Indian economic and intellectual life (Kulke 1978, pp. 120-31).

daism was a prophetic, monotheistic, iconoclastic religion which laid great stress on strict obedience to elaborate moral and ritual rules (Carter 1918, p. 38; Dhalla 1914, p. 81). Indeed, "the Vendidad, the religious code of Zoroastrians," has been described as being "more minute than the Jewish Leviticus" (Carter 1918, p. 89). The laws of the Parsees like those of the Jews lay particular stress on the need to keep the body pure from defilement and to maintain a world of clearly separate categories. The most important categories are the living and the dead and the "natural elements" earth, fire, and water (Carter 1918; Dhalla 1914). Traditionally even a meteorite was regarded as a creature of Ahriman (Satan in Zoroastrian theology) because it marred the order of nature by impiously breaching the boundary between the earth and the heavens (Carter 1918, p. 73).¹⁰

Given the similarities in social situation and structure of beliefs between the Zoroastrians and the Jews, it is significant that the Zoroastrians also exhibit the same strong and consistent traditional taboos against homosexuality as the Jews and that their scriptures advocate equally savage punishments for the transgressor. Dhalla has summed up the traditional view thus:

Zoroaster denounces [unnatural crime] as the worst crime against morality (*Ys.* li. 12) Ahriman [Satan] is its creator (*Vend.* i. 121). There is no sin greater than this and the man practising it becomes worthy of death (*Sad Dar*, ix. 2). This is the only crime which entitles anyone to take the law into his own hands and to cut off the heads of the sodomites and to rip up their bellies (*ib.* ix. 3f.). The *Dātistān-i Dēnāk* (lxxvi. 3) modifies this, and states that, before taking the law into one's own hands, one should try to impress the heinousness of the crime on the minds of the wicked sinners, but, if that is of no avail, one may kill them on the spot. The sodomite is called a demon, a worshipper of demons, a male paramour of demons, a wife of demons, as wicked as a demon; he is a demon in his whole being while he lives and remains so after death (*Vend.* viii. 32). The faithful should not have any intercourse with such a man except by way of attempting to reclaim him from this inexpiable crime (*Dātistān-i Dēnāk*, lxxii. 10). [Dhalla 1911, p. 296; see also Dhalla 1914, p. 290]

The consistent hostility of Judaism and Zoroastrianism toward homosexuality and related forms of sexual deviance may be usefully contrasted with the attitudes displayed in yet another culture which has had a great influence on Western civilization, that of ancient Greece. The ancient Greeks may be regarded as in a sense the opposite of the Jews: a people whose boundaries were ambiguous and yet unthreatened and who never developed strong, fierce, and consistent taboos against homosexuality, bestiality, or transvestism (Bullough 1976, p. 119). The Greeks "tolerated most sexual activity as long as it did not threaten the survival of the

¹⁰ Planets (i.e., wanderers) were also suspect, for they breach the boundaries of the constellations of the fixed stars.

family" (Bullough 1976, p. 119), and "few aspects of sex were ritually prohibited" (Bullough 1976, p. 119). Their taboos against homosexuality were weak and inconsistent (Bullough 1976, pp. 100–102; Karlen 1971, pp. 26–38)¹¹ and fluctuated over time.¹² Indeed in some cities¹³ during the classical period, homosexual interaction was an acceptable and approved form of behavior for certain prestigious social groups.¹⁴

The Greeks' attitude toward homosexuality is not surprising given that they lacked the single decisive social, moral, and religious boundary that characterized the Jews. The boundary of the Greeks, taken as a whole people, was mainly cultural and linguistic and was a weak boundary in that anyone who acquired the language and culture of Greece could become a Greek. It was further weakened by the existence of a stronger set of internal boundaries—the political and legal boundaries of the city-states. Even these boundaries were ambiguous, for immigrants and refugees were often admitted to the city as residents but not necessarily to citizenship. Many permanent residents of the city, though fellow Greeks, merely had the status of metics or *perioikoi* (strangers). The confused overall situation is summed up by M. I. Finley (1971, p. 35): "The Greeks, in sum, thought of themselves not only as Greeks (Hellenes) as against the barbarians, but also and more immediately as members of groups and subgroups within Hellas. A citizen of Thebes was a Theban and a Boeotian as well as a Greek, and each term had its own emotional meaning backed by special myths. And there were still other groupings, such as 'tribes' inside the community or larger abstractions outside it (like Dorians or Ionians), to make up a complicated and sometimes even contradictory structure of memberships and loyalties."

¹¹ During the classical period, praise for homosexuality coexisted with mockery of it; laws for regulating homosexual behavior were enacted alongside prohibitions of it. What is utterly lacking is any sense of homosexuality being a serious threat to society as a whole or to its identity and boundaries (Bullough 1976, pp. 100–102; Karlen 1971, pp. 26–38).

¹² It is very difficult to ascertain how homosexuality was regarded in Greece prior to the classical period. Among the Mycenaeans and in the "dark age" that followed homosexual behavior was probably not common or socially approved or it would have figured in the works of Homer. There is also no mention of the punishment or fierce condemnation of such behavior, so it is equally probable that no strong taboos existed against it. We know even less about the sexual taboos of the latter part of the "dark age" prior to the classical era (Bullough 1976, p. 104; Flacelière 1962, pp. 50–51; Karlen 1971, pp. 12–14).

¹³ Attitudes toward homosexuality varied somewhat among Greek cities and states in the classical period, though in none of them were there strong consistent and vigorous taboos (Dover 1978, pp. 81–83, 185–92; Karlen 1971, p. 34). Roughly speaking, attitudes were permissive in Elis and Boeotia, restrictive in many Ionian areas, and complex, ambiguous, and contradictory in Athens and Sparta.

¹⁴ Such behavior was limited largely to a leisured upper-class group: "There is no evidence that homosexuality met with any general social approval" (Flacelière 1962, p. 195; see also Karlen 1971, pp. 32–33, 35–38).

The Greeks had a strong and unthreatened sense of cultural superiority over the barbarians,¹⁵ but they lacked altogether the Jewish sense of being a holy people set apart by God. The Greeks had neither a coherent organized religion with agreed common scriptures and dogma nor a professional hierarchy of priests (Andrewes 1971, p. 254), and the links among their religion, morality, and identity were weak, shifting, and pragmatic. In his study of Greek homosexuality, K. J. Dover (1978, p. 203) described the social and religious background of the Greek tolerance of such sexual deviance thus: "The Greeks neither inherited nor developed a belief that a divine power had revealed to mankind a code of laws for the regulation of sexual behaviour; they had no religious institution possessed of the authority to enforce sexual prohibitions. Confronted by cultures older and richer and more elaborate than theirs, cultures which none the less differed greatly from each other, the Greeks felt free to select, adopt, develop and—above all—innovate. Fragmented as they were into tiny political units, they were constantly aware of the extent to which morals and manners are local." Under these circumstances it was impossible for strong sexual taboos to evolve, for there were no rigid boundaries to maintain (and there was no one to urge their maintenance); the weak sanctions against homosexuality that emerge from time to time in classical Greece are concerned merely with the protection of particular aspects of family life (Bullough 1976, p. 101).

Curiously, it was not this permissive attitude toward sexual deviance on the part of the Greeks that was to influence Christianity and thus the Western world but instead of the ascetic doctrines of some of the Greek philosophers which became extremely influential in the early years of the Christian era (Bullough 1976, pp. 159–72). These doctrines were not directed against homosexuality or bestiality as such but against sexual indulgence or enjoyment of any kind. The adoption of this ascetic outlook by the early Christians did, however, have the significant consequence that the Jewish sexual taboos were retained by the church even though most of the other distinctively Jewish rules and taboos were discarded.

Most of the taboos observed by the Jews as a means of preserving their distinctive identity were abolished by the early church when it began to recruit members among the gentiles (Acts 15:1–12; Rom. 3:29–31; Gal.

¹⁵ The Greeks as a whole, though attacked by outside enemies, never really feared that they might lose their identity or culture. They had confidence in their ability to force or induce others to absorb or imitate their culture. After they had been defeated and forcibly united by the Macedonians (fringe Greeks who had imbibed Greek culture) they became part of Alexander the Great's drive to found a world empire, multiethnic in composition but Greek in culture. Colonization and Hellenization produced Greek cities all over the Mediterranean world, and the Greeks' sense of secure cultural superiority survived their loss of political power and autonomy (Darlington 1968, pp. 207, 234; Finley 1971, p. 18).

5:1-6, 6:11-16; Grant 1973, pp. 147-49). The food taboos were repudiated (Acts 10:9-16, 11:5-10) and the Sabbath was changed to the Lord's day, Sunday. However, the sexual taboos were retained (Coleman 1980, pp. 88-101, 277-78) and Saint Paul condemned perverts and idolators (Coleman 1980, pp. 89-91) in very strong terms indeed: "God has given them up to shameful passions. Their women have exchanged natural intercourse for unnatural and their men in turn giving up natural relations with women burn with lust for one another; males behave indecently with males and are paid in their own persons the fitting wage of such perversion (Rom. 1:26-27).

Saint Paul also denounced homosexuality in his first letter to the Corinthians (Coleman 1980, pp. 95-97) where he declared that "none who are guilty either of adultery or of homosexual perversion . . . will possess the Kingdom of God" (1 Cor. 6:9-11).

In this way Christian asceticism ensured that the fierce Jewish sexual taboos whose purpose had been the defense of the boundaries and integrity of the Jewish people were fossilized, preserved, and incorporated into Christian doctrine. Those church fathers whose ideal was celibacy and who upheld "the superiority of the virgin state and the higher merit of continence" (Bullough 1976, p. 172, see also p. 182; West 1977, pp. 120, 126) could hardly have been expected to repudiate the Jewish taboos against homosexuality and bestiality, even though the other rules for maintaining the boundaries of the Jewish people had been abandoned (Coleman 1980, p. 124).¹⁶

However, the view that the hostility toward homosexuality, bestiality, and transvestism to be found in Christian societies is *simply* the result of the ascetic doctrines expressed in the scriptures or in theological tradition is inadequate in several respects. Those who put forward such an argument tend to confuse hostility toward these particular forms of prohibited sexual behavior with hostility to eroticism in general (Boswell 1980, p. 164). Furthermore, they have been unable to account satisfactorily for the marked shifts that have occurred in the *intensity* of these particular sexual taboos

¹⁶ It should be noted that Jews had already extended part of their category-bounded morality to the rest of mankind. The more elaborate rules applied only to the chosen people, who had a special covenant with God. However, all mankind was bound by God's covenant with Noah and the Noachian commandments that stemmed from it (see Gen. 9:4-6; Acts 15:19-29, 21:20-25). Regarding the relationship between the Noachian code and the sexual taboos of the New Testament, see Coleman (1980, pp. 92-93). In time the Jews came to apply their sexual taboos also to non-Jews. Epstein (1948, p. 134) notes that "the Talmud considers this moral injunction against copulation with beasts applicable to all human society and binding also upon heathens. (Kid 82a) . . . A heathen under Jewish jurisdiction committing the crime of buggery suffers the death penalty by the hand of the Jewish court. His case, however, differs from that of a Jewish offender in two ways. First his death penalty is execution by the sword not by stoning (San 57b); in the second place the beast is not slain with him."

during the course of Christian history (Boswell 1980, pp. 163-64). The key question that needs to be answered is why Christian societies have tended to repress homosexuality, bestiality, and transvestism with greater ferocity during some periods of their history than others. Once again the answer is to be sought in the attempts of powerful or influential groups to define and defend important social boundaries which they feel are being threatened. Under these circumstances the forms of sexual behavior seen as breaking the boundaries of "natural" categories tend to become the special subject of persecution, and the scriptural taboos are then invoked (Boswell 1980, pp. 7, 165-66) to justify it.

The intense hostility toward homosexuality that has been such a strong feature of the modern Christian world dates largely from the later medieval period. The early Middle Ages were a relatively more tolerant era (Boswell 1980, p. 206), and it was really only in the latter half of the 12th century that homosexuality began to be singled out from other sins for "special attack" (Boswell 1980, p. 277). During the earlier part of the century, with one significant exception in 1120, there was much less persecution of homosexuals. This reflected the fact that no one at that time felt obliged to define or defend the unclear and unthreatened boundaries of Christendom and the Church (Heer 1962, pp. 1-6).

The "earliest and most drastic legislation" (Boswell 1980, p. 281) against homosexual behavior to be enacted during the 12th century occurred, significantly enough, in the crusaders' kingdom of Jerusalem. At a council held by Baldwin II, king of Jerusalem, and Garmund, patriarch of Jerusalem, in Nablus in 1120, four canons were passed "aimed specifically at sodomists" (Bailey 1955a, p. 96). The first of these declares, "If any adult shall be proved to have defiled himself voluntarily by sodomitical vice, whether actively (*faciens*) or passively (*patiens*), let him be burnt" (quoted in Bailey 1955a, p. 96).

These harsh enactments can be understood only in terms of the peculiar position of the Latin Christians who ruled the crusader kingdom of Jerusalem. They were a group in exile who were defending the farthest frontier of Christendom against Muslim attack. They were encircled by enemies who continually threatened or besieged them (Ben-Ami 1969, pp. 16, 130-31) and eventually drove them out altogether. The crusaders lost Jerusalem in 1187 and their last stronghold, Acre, in 1291. The Latin church's religious leaders in Jerusalem, inspired as they were by the spirit of the crusades, opposed "with rigid intransigence" (Ben-Ami 1969, p. 128) any tendency on the part of the secular rulers of Jerusalem to reach an accommodation with their Muslim neighbors. The Latin church also feared the potentially subversive and heretical influence of the kingdom's Greek Orthodox and Syrian Christian population and strove at all costs to maintain its "discriminating and rigid monopoly" (Ben-Ami 1969, p. 62) con-

trol over religious affairs. Under the influence of the Latin church (Ben-Ami 1969, p. 61), the Frankish knights and warriors who controlled the kingdom of Jerusalem kept themselves stringently apart from their Muslim and schismatic subjects. In consequence the normally open and mobile knightly class became "converted into a legal caste with religion and ethnicity as a rigid dividing line between it and the subjected working population" (Ben-Ami 1969, p. 58).

As usual a group whose social boundaries were sharply drawn, strongly threatened, and rigorously defended became obsessed with the need to maintain boundaries of all kinds. The sexual behavior of the sodomists, which was perceived as transgressing natural boundaries, was savagely suppressed because it was a reminder and a metaphor of the threatened identity and integrity of the group itself.

In the heartland of Christian Europe hostility toward homosexuality became marked only toward the end of the 12th century. In 1179 the third Lateran Council formally condemned homosexual acts, the first ecumenical council to do so (Boswell 1980, p. 277; Goodich 1979, p. 43). Later in the century this condemnation was reinforced by a strengthening of the sanctions against sodomy and a tightening up of the Church's administrative rules concerning its detection and punishment (Goodich 1979, p. 45). By the 13th century, sodomy had come to be regarded as a grievous sin against nature, and in canon law "sodomy became one of those infamous crimes that incurred the greatest dishonour and ill repute. For the cleric this meant deprivation of his office and benefice; for the layman the loss of all civil and political rights" (Goodich 1979, p. 71).

During the first half of the 13th century hostility toward homosexuality became even more intense in most West European societies. From about 1250 onward many states began to introduce laws that expressed this hostility (Boswell 1980, pp. 289-91). The second half of the 13th century was characterized by a wave of harsh secular legislation and "between 1250 and 1300 homosexual activity passed from being completely legal in most of Europe to incurring the death penalty in all but a few of the contemporary legal compilations. Often death was prescribed for a single proved act" (Boswell 1980, p. 293; see also Goodich 1979, p. 77).

The harshness of such legislation is in marked contrast to the somewhat milder attitudes of the earlier medieval period. Indeed, Boswell (1980, p. 295) goes so far as to argue that "during the 200 years from 1150 to 1350 homosexual behavior appears to have changed from being the personal preference of a prosperous minority satirized and celebrated in popular verse to a dangerous anti-social and severely sinful aberration." Significantly enough, it was during this same 200-year period that many of the Christian world's most crucial social boundaries, those between Christendom and its neighbors of other faiths (Heer 1962, pp. 6-7), between clergy

and laity (Heer 1962, p. 8), between faithful members of the church and dissenters (Heer 1962, p. 173), and between citizens of different European nations (Strayer 1964, pp. 111-12) came to be defined more rigorously and defended more desperately than before.

In many ways the growth of religious intolerance and rigidity from the late 12th century onward was an inevitable consequence of the great reform movements of the 11th and 12th centuries. The church reformers' efforts had produced two contradictory results (Lambert 1977, pp. 42-43). First, they had stimulated a wave of religious zeal and enthusiasm among the laity (Heer 1962, p. 162; Lambert 1977, pp. 39-41; Russell 1965, pp. 5-10, 54) as well as the clergy. Second, they had succeeded in increasing the autonomy of the church, in centralizing its powers, and in creating a clear moral and institutional separation of clergy and laity based on the "growing sense that the priesthood and all who stood by the altar at mass were a race apart, 'separated for the work'" (Brooke 1964, p. 50). The ultimate aim of the reformers was the "complete isolation of the hierarchy into a peculiar and inviolable caste" (Milman 1867, 4:12), a group "separated from the people by an impassable barrier" (Lea 1932, p. 157). This was to be achieved by the rigorous enforcement of clerical celibacy and the elimination of clerical concubinage (Milman 1867, 4:12-13). "The priest must be a man set apart from his fellows, consecrated to the one holy purpose, revered by the world as being superior to human passions and frailties, devoted soul and body to the interests of the Church, and distracted by no temporal cares and anxieties foreign to the welfare of the great corporation of which he was a member" (Lea 1932, p. 182). The rigid enforcement of clerical celibacy meant that the clergy would cease to have sexual and family ties which cut across the boundary between clergy and laity and that their service and loyalties would be entirely subordinated to the will of the church (Coser 1974, pp. 155-57). Clerical celibacy had in theory been the rule of the church for many centuries, but it was only in the 12th century that the decrees against clerical marriage began to have any real effect (Brooke 1964, pp. 59, 63; Lea 1932, p. 242). The result of this and other reforms was to create a clear and rigid boundary between the clergy and the rest of society. Paradoxically, even though the enforcement of celibacy prevented the formation of a hereditary priesthood and increased the rate of mobility into the priesthood of laymen without a clerical background, the overall effect of the reform was to render the priesthood a more closed group than it had been before. As Bilaniuk (1968, p. 67) has noted, "Absolute clerical celibacy by its very nature tends to establish a priestly ruling class or a closed caste of unmarried men in charge of the Church."

During the 12th century the laity, whose religious enthusiasm had been kindled by the reformers and frustrated by the increasingly centralized, hierarchical, and sacerdotal character of the church (Lambert 1977, pp.

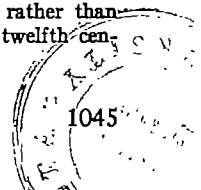
Sexual Taboos and Social Boundaries

39-43), became receptive to heretical teachings which undermined the special powers and position of the clergy. Heretical groups such as the Cathars, the Humiliati, and the Waldensians sprang up and in different ways challenged the authority of the clergy and the distinction between clergy and laity (Christie-Murray 1976, p. 103; Moore 1977, p. 280). The church's reaction to its critics was the classic response of an institution which is in the process of defining and closing its boundaries to a perceived threat to those boundaries, and it set off a vicious spiral of repression, further closure, and increased repression.¹⁷ In 1181 and again more successfully in 1209, crusades were mounted against the Cathars (Christie-Murray 1976, pp. 107-8), and shortly thereafter the Inquisition was developed (Lea 1963, p. 79) with the aim of eradicating heresy altogether.

One of the features distinguishing Christianity from Judaism and many other religions is that it is basically a credal religion, "concerned with right belief as well as with right conduct" (Russell 1965, p. 2). Accordingly, the boundaries of Christian society tend to be defined in credal terms. "When assent to a body of doctrine becomes the criterion for membership in Christian society a visible principle of exclusion is explicitly introduced: those who do not so assent are outside that society" (Russell 1965, p. 249). The existence of major groups of heretics in the late 12th century tended to blur and challenge the credal boundaries of Christian society. The heretics claimed to be Christians, yet denied many of the orthodox doctrines of Christianity. The church's counterattack in the 13th century sought to delineate carefully the margins of permissible belief and to produce "treatises and handbooks that classified heretical tenets and made clearer where precisely the boundary between heresy and orthodoxy lay" (Lambert 1977, p. 95).

The "long process of the stricter definition of dogma" (Peters 1971, p. xi), however, tended to create heresy by labeling as heretics persons of a loyal disposition who held eccentric or confused beliefs (Lambert 1977, p. 171). Indeed, the very processes of investigation tended to manufacture "artificial or semi-artificial heresy" (Lambert 1977, p. 170). Also, the nature of the Inquisition was such as to demand and produce ever tighter and more rigid Christian credal and moral boundaries. "Naturally the influence of those who were given inquisitional powers tended to be directed towards the tightening of the Church's law on heresy in favour of condemnation rather than toleration of doubtful beliefs and practices. The desire of the inquisitor was for clear-cut condemnations and lists of erroneous

¹⁷ In the early days of the reform movement the church had shown great skill in canalizing and institutionalizing waves of religious enthusiasm or movements for religious reform. During the 13th century the church gradually lost its capacity to do this, and "its increasing dependence upon the Inquisition to combat rather than initiate reform . . . helped to turn into heresy what in the eleventh and twelfth centuries would have been legitimized within the church" (Leff 1961, p. 42).



beliefs that could be incorporated in handbooks and used as a basis for action in the field" (Lambert 1977, p. 169).

In addition to the sharpening of religious boundaries, such as those between clergy and laity or between orthodox Christians and heretics, the 13th century also saw new and determined attempts rigorously to define and enforce all manner of secular, legal, and national boundaries (Strayer 1964, pp. 106-8).

These attempts were the work of the lawyers, a "new class of men produced by the increased activity of twelfth century governments" who "set the tone of the thirteenth century" (Strayer 1964, p. 109). "The thirteenth century was a legalistic century, a century in which men sought exact definitions of all human relationships. . . . And because the thirteenth century was legalistic, because it was a period of definitions and detailed explanations, it was a much less tolerant century than the twelfth. It was no longer possible to harmonize divergent views by thinking of them as merely different aspects of universal truth" (Strayer 1964, p. 109).

The emphasis on the clear definition and rigorous defense of religious and legal boundaries in the 13th century inevitably led to the increased intolerance of forms of sexual behavior involving a breakdown of the boundaries between natural categories (notably homosexuality) that has been discussed earlier. Not merely does the rise in intolerance of these forms of sexual deviance follow closely in the wake of an increased general concern with social boundaries but also the ways in which these sexual offenses are linked together and described make it clear that the enactors of the new laws are obsessed with the urge to preserve boundaries of all kinds. Thus the early treatise on English law, *Fleta*, composed about 1290, declares; "Those who have dealings with Jews or Jewesses, those who commit bestiality and sodomists are to be buried alive, after legal proof that they were taken in the act and public conviction" (FLETA 37.3, quoted in Bailey 1955a, p. 145). This linking together of homosexuality, bestiality, and sexual relations with non-Christians (all boundary-breaking sexual activities) was not limited to England, for in Paris in the 13th century "one Jean Alard found guilty of co-habiting with a Jewess was burned as was she, 'since coition with a Jewess is precisely the same as if a man should copulate with a dog.' . . . The crime for which Alard was convicted was described formally as 'sodomy.' . . . Sexual relations with Turks and Saracens have also been held to constitute bestiality" (Masters 1962, p. 286). Masters also cites a similar case of a deacon who was burned at Oxford in 1222 after being found guilty of marrying and having sexual relations with a Jewish woman.

In the Middle Ages in England "sodomy appears to have been regarded in much the same way as sorcery" (Bailey 1955a, pp. 146-47), and indeed they continued to be placed in the same category until the 17th century

(Bingham 1971, p. 461; Mazlish 1971, p. 471). Once again we find a linking together of boundary breakers as being threatening and unnatural. After the English Reformation such cases were transferred to the temporal courts, and an act of Parliament from Henry VIII's reign laid down the death penalty for "the detestable and abominable vice of Buggery committed with mankind or beast" (25 Hen. VIII, c. 6, quoted in Bailey 1955a, p. 147; see also West 1977, p. 280). Once again homosexuality and bestiality were classed together in the single category-destroying crime of buggery. Even today in those parts of the United States and the British Isles where the traditional religious veneration of categories and boundaries is still powerful, harsh penalties can be imposed on those found guilty of homosexual acts or bestiality (Levin 1980, p. 14; Weinburg and Williams 1977, pp. 22-23).

The growth in hostility toward homosexuality that followed the transformation of the Catholic church into a centralized bureaucratic hierarchy of celibate priests from the later medieval period onward indicates the importance of strongly bounded hierarchical organizations of this type as upholders of strong sexual taboos. In most Western societies the church and the armed forces have tended to display a greater concern with the maintenance of strong and rigid external and internal boundaries than have other institutions. This concern has been especially strong when the church or army has been large, centralized, bureaucratic, and hierarchical. The intense preoccupation of the clerical and military leaders of these organizations with boundary questions manifests itself directly in two main ways.

First, both church and army maintain sharp boundaries between their own personnel and outsiders. Very different rules of conduct apply to persons within these organizations and those outside. Those inside often possess special powers and sometimes privileges from which outsiders are excluded. At the same time their personnel are subject to demands and disciplines far in excess of what is expected of people in other walks of life (e.g., the celibacy of the priesthood discussed above). They are members of what Lewis A. Coser (1974) has termed "greedy institutions." One index of the strength and impermeability of the external boundaries of churches and armies is the fact that in English and in many other languages there are special words to describe persons who are not clergy or soldiers. We habitually describe individuals as laymen or civilians, but we have no special words for persons who are not coal miners or not bankers or not teachers.¹⁸ People with other, usually more directly economic, occupations

¹⁸ Professions that would like to set themselves apart in the same way but cannot achieve public recognition of their separate status and dignity are forced to borrow the terms "civilian" or "layman" to describe outsiders. Policemen will sometimes refer to members of the public as civilians, and doctors and lawyers refer to persons outside the profession as laymen. However, the distinctions they are seeking to make are not recognized by the rest of the population, who do not use these words in this fashion.

are not set apart in the way that priests or soldiers are. Also they can move in and out of their occupations much more easily—there are no rigid boundaries to cross as in the case of the priesthood or the military (Davies 1975, pp. 178–82). To become a priest, a man must have an acceptable vocation, and he can enter the priesthood only after a long period of probation and by means of elaborate rituals and ceremonies. The vows he then takes are lifetime ones and cannot easily be renounced. It is somewhat simpler to become a soldier, and indeed, men are often conscripted, but once a man has joined one of the armed forces it can be extremely difficult for him to leave. In most occupations a man can give a week's or a month's notice but a soldier joins the army for several years or for the duration.

It is important to note at this point that the boundary between the clergy or the military and the rest of society coincides with the boundary between the sexes. Throughout most of Western history only men have been allowed to become soldiers and priests. Women have been admitted to most professions and occupations for some time, but only very recently have they entered the armed forces and even now they are hardly ever used in combat roles (Tiger 1968, pp. 80–84) and can evade conscription or quit the service much more easily than men.¹⁹ Many of the more liberal Protestant churches now have women as ministers, but in these denominations the minister is not set apart from the congregation in the way that a Roman Catholic, Greek Orthodox, or Church of England priest is.²⁰ The most strictly hierarchical churches still have male priests and bishops only.

Second, military and ecclesiastical hierarchies have much sharper bound-

¹⁹ Most of the countries which admit women into their armed forces nevertheless do not conscript women to be soldiers. In Israel women are conscripted, but they can evade conscription easily by pleading religious objections to the idea of women being in the army. About 40% of the potential Israeli female conscripts dodge the draft in this way. Only a trivial number of Jewish men try to evade army service; those who do are regarded as moral outcasts. Further, women are not normally used in combat roles. In each war Israel has fought, the casualties have been almost entirely male (see also Rein 1980, p. 66).

²⁰ In Britain the Congregationalists ordained their first woman minister in 1919, the Church of Scotland has ordained women since 1969, and the Methodist church has admitted women to the ministry since 1974. In Europe most of the Evangelical Lutheran churches and Calvinist Reformed churches have admitted women to the ministry. About 90 of the 280 member churches of the World Council of Churches now ordain women, but the traditional priestly and episcopal churches (Roman Catholic and Eastern Orthodox) are strongly opposed to the ordination of women. The question of female ordination has created a crisis within the Church of England and the American Episcopal church for that section of the church which regards its clergy not as pastors but as priests, i.e., members of a group set apart by their sacred powers and functions. A number of American Episcopalian priests have left their church in protest against the ordination of women and have joined the Church of Rome. The pope welcomed them into the Catholic church and has permitted them to function as Roman Catholic priests, even though most of them are married, thus indicating that the exclusively male character of the priesthood is an even more decisive mark of its separate and special status than its celibacy is.

aries between different grades or ranks than do most other kinds of institution. The divisions between officers, noncommissioned officers, and privates, and those between bishops, priests, and deacons are very marked indeed and have few real parallels in other Western institutions. In monasteries there are notable cleavages between the abbot, choir monks, and lay brothers (Davies 1975, pp. 178-82).

All these divisions serve to demarcate groups characterized by very large differences in power and status. Social intercourse between persons of different rank is extremely restricted, and any undue familiarity can be punished according to the disciplinary code of the institution.

The marked concern with boundaries exhibited by hierarchical religious and military organizations inevitably leads to the development or reinforcement of strong taboos against deviant sexual behavior. Such organizations tend to be particularly intent on prohibiting male homosexuality and transvestism. This tendency is not surprising given the all-male character of many of their basic units, the celibacy of Catholic priests and monks, and the absence of women from monasteries, army camps, and fighting units. Under these circumstances homosexual relationships are liable to cut across vital social boundaries in a very real way and transvestism is liable to undermine the dignity of a distinctively and exclusively masculine group whose maleness gives them privileged access to the honored roles of priest and soldier.

The leaders and indeed the personnel of prestigious religious and military organizations are likely to see the survival of their own distinctive status and dignity as depending on the maintenance of strict boundaries between insiders and outsiders and (since the insiders are exclusively masculine) between male and female. The fear, horror, and ridicule that any kind of effeminacy, transvestism, or physical contact between two males can arouse in the clergy is well shown in a letter written in 1191 by Hugh de Nunant, the bishop of Coventry, concerning the deposition of William, Bishop of Ely, the king's chancellor (Roger de Hoveden 1853, pp. 236-38). What is most significant about the letter is the explicit way in which the author links the taboos against cross-dressing to the maintenance of the dignity of the priesthood. (The portion of the document set off by brackets was left in the original Latin by the Victorian translator, probably for reasons of prudery. The translation of that section is mine.)

After he [William, Bishop of Ely] had remained in the castle of Dover some days, unmindful of his profession . . . he determined to set sail and as he did not care to do this openly, he hit upon a new kind of stratagem and pretending to be a woman, a sex which he always hated, changed the priest's robe into the harlot's dress. Oh shame! the man became a woman, the chancellor a chancelloress, the priest a harlot, the bishop a buffoon. Accordingly . . . he chose to hasten on foot from the heights of the castle

down to the sea-shore, clothed in a woman's green gown of enormous length instead of the priest's gown of azure colour; having on a cape of the same colour, with unsightly long sleeves, instead of a chasuble, a hood on his head instead of a mitre, some brown cloth in his left hand, as if for sale, instead of a maniple, and the staff of the huckster in his right hand in place of his pastoral staff. Decked out in such guise the bishop came down to the sea-shore and he who had been accustomed much more frequently to wear the knight's coat of mail, wondrous thing! became so effeminate in mind as to make choice of a feminine dress. Having seated himself on the shore upon a rock, a fisherman, who immediately took him for a common woman, came up to him; and having come nearly naked from the sea, perhaps wishing to be made warm, he ran up to this wretch and embracing his neck [with his left arm, with his right hand began exploring his lower parts. Suddenly lifting up his tunic, he most immodestly, boldly stretched out his hand towards (the bishop's) private parts and feeling his thigh, discovered immediately that there was a man inside "the woman."] At this he was greatly surprised, and starting back in a fit of amazement shouted with a loud voice, "Come all of you and see a wonder; I have found a woman who is a man!" Immediately on this his [the bishop's] servants and acquaintances who were standing at a distance came up and with a gentle kind of violence pushed him back and ordered him to hold his tongue; upon which the fisherman held his peace and the clamour ceased and this hermaphrodite sat waiting there. In the meantime a woman who had come from the town; seeing the linen cloth which he or rather she was carrying as though on sale, came and began to ask what was the price and for how much he would let her have an ell. He, however, made no answer, as he was utterly unacquainted with the English language; on which she pressed the more and shortly after another woman came up, who urgently made the same enquiry and pressed him very hard to let her know the price at which he would sell it. As he answered nothing at all, but rather laughed in his sleeve, they began to talk among themselves and to enquire what could be the meaning of it. Then suspecting some imposture, they laid hands upon the hood with which his face was covered and pulling it backwards from his nose, beheld the swarthy features of a man lately shaved, on which they began to be extremely astonished. Then rushing to the dry land, they lifted their voices to the stars crying out, "Come let us stone this monster who is a disgrace to either sex." Immediately a crowd of men and women were collected together, tearing the hood from off his head and ignominiously dragging him prostrate on the ground by his sleeves and cape along the sand and over the rocks, not without doing him considerable injury. . . . The populace were inflicting vengeance on him with insatiate eagerness, reviling him, inflicting blows and spitting upon him. . . . He [the bishop of Ely] became an object of extreme disgrace to his neighbours, of dread to his acquaintances and was made a laughing-stock for all the people. I only wish that he had polluted himself alone, the priest, and not the priestly office. May, then, the Church of Rome make due provision that such great guiltiness may be punished in such a way, that the offence of one may not contaminate all, and that the priestly authority may not be lessened thereby. [Quoted in Roger de Hoveden 1853, pp. 236-38]

The bishop's letter is sufficiently explicit to need little explanation and sufficiently detailed and characteristic to require no augmenting. The bishop of Coventry depicts his enemy, the bishop of Ely, as violating all the taboos at once. He is described as crossing the barriers between clergy and laity and between male and female at one and the same time. He is shown neglecting his dedication to unworldly immaterial goals and appearing in the guise of a female huckster of cloth. There is even a reference to the bishop's previous martial and masculine appearance in "a knight's coat of mail" (as a bishop he should not really have dressed up or behaved as a soldier, but his doing so clearly did not carry the same ludicrous shame and disgrace as his appearance as a woman does) as a contrast to his present effeminacy. The bishop of Ely's humiliation began when he was groped by a fisherman who uncovered his nakedness (a kind of quasi-homosexual behavior, albeit in error) and ended with an attack on him by a righteous mob enraged at his transvestite imposture. If this was seen as an appropriate punishment for a man who disguised himself as a woman to escape capture by his enemies, it is clear that a true transvestite masquerading for sexual reasons would have been treated with extreme harshness. Finally, the bishop of Coventry's letter returns to the central concern of the church—the concern that "the priestly authority may not be lessened thereby."

The maintenance of strict boundaries between the different levels of a military or ecclesiastical hierarchy requires that people of different rank should not have sexual relations with each other. In an all-male organization such as an army this means in practice a ban on male homosexuality.²¹ It was for this reason that Britain's present and former military leaders were so strongly opposed to the Sexual Offences Act of 1967, which legalized homosexual behavior between consenting adults in England and Wales.²² Significantly, the 1967 reforms do not apply within the British armed forces, and any man can still be charged under the Army, Navy, or Air Force Acts with disgraceful conduct of an indecent or unnatural kind. Dr. David Owen, minister for the navy in the then Labour government, who had been a member of the select committee on the Armed Forces Bill,

²¹ When hierarchical, disciplined, military or ecclesiastical bodies contain members of both sexes, heterosexual relations (even including marriage) may be forbidden between a man and woman holding positions that differ markedly in status and authority. In church hierarchies characterized by clerical celibacy the problem does not (in theory) arise but occasionally difficulties have arisen in the Salvation Army. The military leaders of the British army have from time to time taken steps to prevent the marriage of men and women of different rank, usually by posting the man overseas (Ballhatchet 1980, p. vii). Ballhatchet (1980, p. 10) also comments on the leadership's fear of homosexuality leading to indiscipline as described above.

²² Many ex-military men have entered the House of Commons and the House of Lords in Britain, and they tended to speak and vote against these reforms (Richards 1970, p. 185). The legalization of homosexual behavior applied only to England and Wales, not to Scotland or Northern Ireland.

said in the British Parliament: "We examined the question of homosexuality as it applies to the forces. The Committee's unanimous recommendation was that it should remain subject to the Naval Discipline Act and the Acts affecting the other Services. It was put to us forcibly that if this bill became applicable to the services it would *undermine discipline*. That is what we are arguing about, *the effect on discipline in the services* and among Merchant Seamen at sea" (Great Britain 1966, vol. 738, col. 1109-10; emphasis added).

Owen's concern about discipline in the services was shared by Viscount Montgomery of Alamein, the Second World War British military leader:

What is the greatest single factor making for success in battle or for efficient and well-trained armed forces in peace? It is morale. And what is the very foundation of morale? It is discipline. If these unnatural practices are made legal, a blow is struck at the discipline of the British armed forces at a time when we need the very highest standards of morale and discipline with these forces serving throughout the world. Take an infantry battalion. Suppose the men know the officers are indulging in unnatural practices and it is legal and nothing can be done. Take a large aircraft carrier with two thousand men cooped up in a small area. Imagine what would happen in a ship of that sort if these practices crept in. [Great Britain 1965, vol. 266, col. 647]

Montgomery's concern about the armed forces was shared by the Labour member of Parliament Simon Mahon, who wished also to see homosexual behavior prohibited in the Merchant Navy, another all-male disciplined hierarchical organization.²⁸ He successfully put forward an amendment to the Sexual Offences Bill (with the support of eight Liverpool and Merseyside members of Parliament, i.e., men and women representing constituencies with strong connections with the Merchant Navy) that stated: "This house declines to give a second reading to a Bill which fails to afford the exemption and protection to the Merchant Navy now provided in the Bill to Her Majesty's Royal Navy, Army and R.A.F. and fails to take into account that this omission will create circumstances which can lead to corruption of young seamen and to conditions which will be prejudicial to the best interests of the Merchant Navy and to the discipline and good order at sea which are vital to the best interests of our nation's merchant ships" (Great Britain 1966, vol. 738, col. 1068).

In support of his amendment, Mahon argued: "There are tremendous difficulties about trying to discipline a crew. There is nothing wrong with discipline and one has to have it at sea or in any other aspect of society—reasonable and good discipline. . . . It is difficult for the master of a ship to maintain discipline, and this Bill will make it worse. The same applies to

²⁸ See also Percival (Great Britain 1967, vol. 748, col. 2161), who notes that the Merchant Navy is a disciplined hierarchical service with "officers, warrant officers and other ranks."

chief engineers or chief stewards" (Great Britain 1966, vol. 738, col. 1089).

The repeated use of the term "discipline" refers to the preservation of the hierarchical divisions and ordering of the various services. One Conservative member of Parliament, Ian Percival, made this explicit in the debate on the report stage of the Sexual Offences Bill when he noted: "I understand the reason why the Forces have been exempted from the provisions of the Bill. In the Forces there is a hierarchy from the top down, for the purposes of discipline, in which there is superior after superior all the way down" (Great Britain 1967, vol. 748, col. 2161).

The concern expressed by British members of Parliament about the effect that permitting homosexuality might have on the maintenance of hierarchical order, boundaries, and discipline in the armed forces is also a feature of the generally liberal Wolfenden report (*Report of the Committee on Homosexual Offences and Prostitution* [1957], p. 53; see also the *Report of the Select Committee on the Army Act and the Air Force Act* [1953]). In general the report advocated the abolition of legal penalties for homosexual behavior but made an exception in the case of the armed forces:

Offences in Disciplinary Services and Establishments

We recognise that within services and establishments whose members are subject to a disciplinary regime it may be necessary, for the sake of good management and the preservation of discipline and for the protection of those of subordinate rank or position, to regard homosexual behaviour, even by consenting adults in private, as an offence. For instance, if our recommendations are accepted, a serving soldier over twenty-one who commits a homosexual act with a consenting adult partner in private will cease to be guilty of a civil offence or of an offence against Section 70(1) of the Army Act, 1955 (which provides that any person subject to military law who commits a civil offence shall be guilty of an offence under that section, and hence liable to be dealt with by Court Martial). The service authorities may nevertheless consider it necessary to retain Section 66 of the Act (which provides for the punishment of, *inter alia*, disgraceful conduct of an indecent or unnatural kind) on the ground that it is essential, in the services, to treat as offences certain types of conduct which may not amount to offences under the civil code. Similar problems may arise in relation to other services and establishments.²⁴

The fear that the existence of homosexuality in the armed services could endanger the maintenance of a disciplined hierarchy is to be found in other countries besides the United Kingdom. Thus in the United States, the Department of the Army has told a Senate subcommittee: "It is the policy of the department of the Army that homosexual personnel will not be per-

²⁴ The phrase "for the protection of those of subordinate rank" is often used when justifying the punishment of homosexual offenses in the forces. Sometimes (as here), people mean what they say, but often it is simply an attempt to put a liberal gloss on the argument that sexual familiarities between persons of different rank should be prohibited in the interest of maintaining hierarchical boundaries.

mitted to serve in the Army in any capacity; prompt separation of homosexuals is mandatory. The Army considers homosexuals to be unfit for military service because their presence impairs the morale and discipline of the army" (quoted in Williams and Weinburg 1971, p. 24).

D. J. West (1977, pp. 302-3) has summed up the view of homosexuals held by the U.S. military authorities as follows: "Supposedly they constitute a threat to morale, their personal characteristics limit their military effectiveness, their presence exposes other young men to the risks of sexual seduction, and *their propensity for forming attachments across the barriers of rank disrupts discipline*" (italics added).

In the past, of course, the leaders of hierarchical military and naval organizations often treated homosexuality even more harshly than they do today. Arthur N. Gilbert (1976, p. 72), in his study "Buggery and the British Navy 1700-1861," writes that "military attitudes reflected what the army and the navy saw as their own unique problems of order and discipline. In particular navy records reveal a ferocity towards morals offenders far beyond that of civil society."

Gilbert (1976, p. 79) notes that "from the seventeenth century on, buggery was specifically mentioned in the Articles of War. . . . In spite of the unwillingness to convict on the part of some court martial boards, by and large the records show that buggery was treated as a serious offence—as serious as desertion, mutiny, and murder. Between May 1st and June 19th, 1708, for example, twelve men were sentenced to death in the Royal Navy. Of the twelve, six were deserters, one was convicted of murder, and the remaining five were found guilty of buggery. Between 1703 and 1710, twelve sailors were tried for buggery or attempted buggery and six of these men were sentenced to death."

Significantly, "the navy treated sodomy in the officer corps as a more serious crime than murder. On at least two occasions during the Napoleonic wars officers were hanged for buggery. In 1807 Lieutenant William Berry was sentenced to death largely on the evidence of a boy, Thomas Gibbs, who testified that the lieutenant had committed buggery on him and fellatio as well. In another case, Captain Henry Allen of the sloop Rattler was convicted and executed for sodomy in 1797" (Gilbert 1976, pp. 84-85).

Many of Gilbert's explanations of the extreme severity with which the British navy treated homosexuality depend on the naval officers perceiving homosexuals as a threat to the rigid external and internal social boundaries that characterized their organization. Gilbert (1976, p. 87) argues that "the ambiguity of homosexual relations probably challenged naval order in a special way. The navy prided itself on the fact that its values were clear, direct and simple. Every man knew his place in the hierarchical order of things and in the same way, the lines between proper and improper conduct were sharply drawn. The ideal of both officers and crew was to live

simply, to obey orders without question and to know right from wrong. . . . Homosexuality made the ideals of 'manliness'—an all important part of naval virtue—ambiguous."

The severity with which the British navy treated homosexuals reflected a cumulative response to a form of sexual behavior which threatened simultaneously the three kinds of social boundary that have been discussed. First, homosexuality broke down the boundary between male and female in a way which the religious traditions of both Jews and Christians regarded as abhorrent. This would in any case have resulted in severe sanctions being applied even if only civilians had been involved. Second, it undermined the ideals of manliness on which naval virtue depended. The boundary between naval personnel and civilians coincided in part with that between men and women, and any threat to the latter boundary affected the former. Finally, homosexuality threatened the structure of the naval hierarchy by creating unpredictable sexual relationships that could erode the boundaries between the different ranks of the navy.

The composite strength of the severe taboos against homosexuality in the armed forces of modern Western countries may be contrasted with the unusual permissiveness of three military or quasi-military organizations that tolerated and indeed even fostered homosexual relationships—the armies of two ancient Greek states, Elis and Boeotia, and the German National Socialist S.A. (*Sturmabteilung*) movement, the Brownshirt militia. This contrast demonstrates once again that the key factors underlying military taboos against homosexuality are the Judeo-Christian religious and cultural tradition and the determination of military leaders to exercise centralized control of their organization through the maintenance of a strict hierarchy with stable boundaries.

K. J. Dover (1978, p. 192; see also Xenophon's *Symposium* 8:34) writes of the two Greek states:

In Elis and Boiotia erastes and eromenos [the adult and the youth in a homosexual pair] were posted beside each other in battle (Xen. *Smp.* 8.32). Both states, that is, exploited an aspect of the homosexual ethos for military purposes. About Elean military organisation we know no more, but the 'Sacred Band' of Thebes formed c. 378 was composed entirely of pairs of homosexual lovers; it was the hard core of the Boiotain army, a formidable army at all times throughout the middle period of the fourth century, and at Khaironeia in 338, where Philip II of Macedon crushed Greek opposition, it died to a man. A certain Pammenes, a Theban military commander, advocated this type of pairing as a principal of military organisation (Plu. *Dial.* 761a) and it was a practice at Thebes (*ibid.*), when an eromenos came of age, for his erastes to make him a present of armour.

The passage from Plutarch (*Moralia* 9.761) to which Dover refers reads in full:

In your City Thebes, Pemptides, isn't it true that a lover made his beloved a present of a complete suit of armour when the boy was registered as a man? Pammenes, a man versed in love, changed the order of the battleline for the hoplites [heavy infantry] censuring Homer as knowing nothing about love because he arranged the companies of Achaeans by tribes and clans and did not station lover beside beloved in order to bring it about that "shield supported shield and helmet helmet" for he considered that love is the only invincible general. It is a fact that men desert their fellow tribesmen and relatives and even (God knows) their parents and children; but lover and beloved when their God is present no enemy has ever encountered and forced his way through. [Plutarch 1961, p. 379]

The Theban leaders controlled homosexuality within their army by institutionalizing rather than forbidding it (Dover 1978, pp. 51, 191).²⁵ They could thus ensure that any homosexual relationships formed were compatible with the structure of the army and did not cut across vital organizational boundaries in a random and unpredictable fashion. It was relatively easy for them to do this because of the existence of stable, socially approved bonds between pairs of soldiers at different levels of age seniority and military experience in the society of the city itself. Such a policy was possible for two reasons. First, there were no strong taboos based on religion to prevent or vitiate such institutionalization (an option which would not, of course, in general be open to Christian or Jewish army commanders).²⁶ Second, Thebes was a small state with a population of only 40,000–60,000 people including men, women, and children, citizens, metics, and slaves (Finley 1971, p. 55). Thebes would have had an even smaller army than the population figure suggests, for only free adult male citizens could serve in the army. Indeed, the elite Theban Sacred Band mentioned earlier consisted of a mere "three hundred picked men. Every member of it was the sworn lover of another" (Anderson 1970, p. 158).²⁷ A small army such as this could survive as a disciplined body without an elaborate hierarchical command structure. Boeotia was not bureaucratically organized, nor were its armed forces, and its army would have lacked the numerous rigid social

²⁵ To some extent a parallel may be drawn with attempts by religious organizations to control the heterosexual behavior of their members either through marriage or through celibacy.

²⁶ It is relatively rare to find institutionalized homosexuality as a stable pattern within a military unit drawn from a Jewish or Christian society. Only in a different religious setting have such relationships survived on a stable basis, e.g., among the Samurai of Japan, to the great disgust of the Jesuit missionaries to the island (Varley, Morris, and Morris 1970, p. 103).

²⁷ Anderson (1970, pp. 158–59) adds that "of these pairs one was known as the charioteer and the second as his 'crew.'" This reflects an earlier period in Theban military history when two-horse chariots were used. Clearly the homosexual pairing coincided with the organizational structure, such as it is, of the two-man chariot team. However, by the classical period chariots were obsolete and the Sacred Band was used as front-line heavy infantry (Anderson 1970, pp. 159–64).

and bureaucratic boundaries of a large modern army. Any clashes that might have occurred between the organizational demands of the military leaders and the particular homosexual relationships of pairs of soldiers would probably not have created acute problems of morale or discipline.²⁸

Similar conclusions may be reached from a study of the widespread practice of homosexuality in the National Socialist S.A. militia and its predecessor the *Frontbann* in the 1920s and early 1930s. As early as 1925 the journalist Carl von Ossietzky (1971, p. 65), in an article entitled "The National Pederasts," had written: "In the course of their search for the secret murder gangs the Berlin police have arrested a Frontbann leader with some of his young followers. The chieftain and his gang were surprised, not while they were pledging eternal struggle against the Jews and the French but, to put it briefly the paragraph of the criminal code on homosexuality will play a part in the proceedings. . . . The fact is that in most of these para-military organisations which allegedly serve renewal and discipline and which present themselves as muscular and manly there is another cult apart from that of ultra-patriotism. We have become increasingly accustomed to the figure of the 'nationalist leader' who occupies his spare time by seducing young boys."

Konrad Heiden (1945, p. 235) went further and described homosexuality as being pervasive and indeed institutionalized within the S.A. movement and its predecessors: "The perversion was widespread in the secret murderers' army of the post-war period and its devotees denied that it was a perversion. They were proud, regarded themselves as 'different from the others,' meaning better." This is perhaps not surprising, since so many of the leaders of the S.A. were open homosexuals, notably Captains von Petersdorff and Röhrbein; Count Ernest Helldorf; Walter Stennes, leader of the S.A. in north and east Germany; and Edmond Heines, commander of the S.A. in Silesia (Heiden 1945, pp. 294-95). Above all, at the very top of the S.A., as Samuel Igra (1945, p. 72) notes: "The vices of Captain Röhm commander of the storm troops with three million men under his control were a matter of worldwide knowledge and comment."

It was possible for the S.A. to tolerate and indeed encourage homosexuality in this way for two familiar reasons. The first is that the S.A. was the revolutionary and ideologically radical wing of the National Socialist party and its members expressed contempt for the churches and hatred for the Jews. It was thus easy for the leaders of the S.A. to reject the Judeo-Christian tradition of hostility toward homosexuality. The second reason

²⁸ Such clashes would present a much more significant problem in a large modern army (Hauser 1962, p. 60). Even in a small Greek army such problems would have arisen, as we can infer from the lines in Plutarch's "Dialogue on Love" that immediately follow those quoted above (1961, p. 379) or from Xenophon's Banquet VIII 34-35 (1922, pp. 472-73). In any case, Greek armies do not seem to have been highly disciplined hierarchical bodies (Pritchett 1977, pt. 2, pp. 237-44).

is that the S.A. was not a coherent, disciplined military organization with strict boundaries. Although large, the S.A. was based "on a tiny unit, the so-called 'schar' (squad); such a squad was formed when, somewhere, a leader arose of his own accord 'and set up the squad'" (Heiden 1945, p. 315). For all its military boasting and appearance of discipline, the S.A. was nothing more than a loosely organized aggregate of groups of uniformed toughs whose main task was to brawl in the streets and beerhouses of Germany with rival political factions (Bleuel 1974, p. 129). It had no clear internal or external boundaries that might be threatened by the prevalence of homosexual practices and relationships. Certainly the leadership of the S.A., itself pervaded by homosexuality, was indifferent to any such problems that might arise.

Members and veterans of the regular German army were, however, shocked and horrified by the homosexuality of the S.A. Regular army officers, with their regard for the religious traditions of German society, their belief in the need for a disciplined military hierarchy with strong boundaries, and their memory of the harm done by past scandals, were strong upholders of the taboos against homosexuality (Davies 1975, pp. 128-31). Even in the 1920s one of Hitler's main military supporters, the veteran First World War General Ludendorff, had sent a delegation to him led by Count Ernst Zu Reventlow to say that "the general viewed the activities of the homosexuals in the S.A. with great misgivings" (Heiden 1945, p. 236; see also Bleuel 1974, p. 130).

When Hitler came to power in 1933 he badly needed the support of the regular army to make his position secure. The army was willing to support Hitler but only reluctantly and provided that he agreed to curb its rival for control of the armed power of the state, the radical and potentially revolutionary S.A. movement. The army leaders' complex hostility to the S.A. is well described in Heiden's (1945, pp. 581-82) account of their alarm at Röhm's demand that the S.A. be integrated into the regular army:

Röhm as a member of the Reich Cabinet now raised his voice and demanded that the S.A. be made a part of the Reichswehr [regular army]. Even if only a fraction of the three million S.A. men, most of whom were engaged in civilian activities, could become soldiers, as many S.A. leaders as possible were obviously to become soldiers and this with a rank corresponding to their S.A. rank. Thus one fine morning these armed poultry farmers or department store porters would wake up with the rank of general or at least colonel, just because they had won the titles of S.A. group or brigade leaders as a result of various scuffles in beer cellars or back alleys. Blomberg [the Reichswehr minister] sharply rejected Röhm's demands. By his aggressive move Röhm gave the enemies of the S.A. their long desired occasion to blame and deride what almost everyone in Germany knew about the S.A. and its degenerate leaders; they attacked Röhm's newly organised Berlin headquarters as the scene of extravagant and obscene orgies.

General Weïner von Blomberg went so far as to declare that "rearmament is too serious and difficult a business to permit the participation of peculators, drunkards and homosexuals" (Wykes 1972, p. 98).

Hitler was soon forced to choose between the regular army and the S.A., and he chose to purge the leaders of the S.A. In June and July 1934 most of them were murdered or imprisoned on Hitler's orders. Hitler now denounced the S.A. leaders as perverts and used this as an excuse for purging them. Goebbels made this a central feature of his denunciation of those purged and murdered:

They were literally buried in mud. A storm of public defamation descended upon these dead men which described their unnatural tendencies with the most loathsome details; Goebbels' propaganda made it almost their principal crime that they had defiled the Führer's pure movement with their dirty practices. . . . In the speech which Hitler made before the non-arrested party leaders on June 30th he chiefly accused Röhm and his men for their loose and depraved conduct and declared that for that alone they deserved to die. To Lutze whom he had appointed Röhm's successor he sent an order comprising twelve points, which dealt almost exclusively with parties, drinking bouts, automobile trips, squandering and unnatural lewdness indulged in by Röhm and his gang—and stressed the necessity of putting an end to all that. [Heiden 1945, pp. 594–95; see also Speer 1975, p. 91]

At one level we can analyze all these events simply in terms of power politics. Hitler purged the radical wing of the National Socialist movement in order to gain support of the army and eliminate a dangerous rival. Such an explanation is necessary, but it is incomplete. It explains neither the ferocity with which the S.A. leaders' homosexuality rather than some other facet of their lives or character was denounced, nor why it was possible to turn the fact of their homosexuality into such a strong propaganda weapon. Also, the victims of the purge were often selected on moral rather than political grounds, for "as Hess later suggested, these arrests were fairly arbitrary, made chiefly on the basis of feeling—and Hitler's feeling seems frequently to have been based on a suspicion that the person concerned had homosexual tendencies" (Heiden 1945, p. 595).

The arbitrary singling out of the homosexuals among the S.A. leadership for arrest and execution marked the beginning of a period during which homosexuality was prohibited and punished with greatly increased severity (Bleuel 1974, pp. 292–303). Eventually homosexuals would even be sent to concentration camps.

This increasing hostility toward homosexuality is rather what one might have expected in a society increasingly obsessed with racial boundaries and strict hierarchical authority. The homosexuality of the S.A. members and leadership could be tolerated within the loosely bounded and coordinated structures of the National Socialist party before it came to power, but

such behavior was incompatible with the hierarchical and rigidly bounded system through which achieved totalitarian power was exercised.²⁹

CONCLUSIONS

Three general conclusions can be drawn about the relationship between sexual taboos and social boundaries. First, on the basis of a comparative examination of a large number of Western societies and institutions, it is clear that the strong taboos that exist against homosexuality, bestiality, and transvestism in the West are the result of attempts to establish and defend strong ethnic, religious, or institutional boundaries. Where such pressures are weak or absent the taboos against these forms of sexual deviance are also weak or absent.

Second, if religious, military, or political leaders decide out of ideological commitment, organizational necessity, or a sense of external threat to strengthen the boundaries of their group, people, or institution they tend in consequence to impose harsh penalties on forms of sexual behavior that breach social or symbolic boundaries. In many cases this is because they are seeking to maintain the identity and boundaries of their group by instilling in its members a code of belief and conduct that emphasizes the need to maintain boundaries of *all* kinds, including those between humans and animals and males and females. In the case of rigidly hierarchical all-male political, military, or ecclesiastical organizations, homosexual relationships are banned because they might be formed between a person within the organization and another outside or between persons of markedly different rank. Sexual relationships formed across crucial social and organizational boundaries in this way are seen by the leadership as subversive of morale and discipline.³⁰

Third, in any particular social situation deviant sexual behavior or relationships may be seen as threatening more than one social boundary. In these circumstances the severity with which infringements of the taboos

²⁹ It is no accident that the other totalitarian socialist state in Europe, the Soviet Union, also engaged in intensive persecution of homosexuals from 1934 on (Mannheim 1946, p. 71; Millet 1971, p. 173; West 1977, pp. 143, 278). Significantly, homosexuality was classed as "a social crime, on a par with sabotage and counter-revolutionary activity" (West 1977, p. 278). The leaders of modern totalitarian states, such as National Socialist Germany or the Soviet Union, which lay emphasis on the maintenance of strict external and internal boundaries, clearly see homosexuality as a threat to these boundaries much as the leadership of earlier ecclesiastical and military hierarchies did.

³⁰ Sexual deviants are often also regarded as security risks because they may form sexual contacts with outsiders (thus breaching the boundary of the institution) and then for reasons of affection or blackmail proceed to betray the home institution. This problem is as much a result of the taboos as a cause of them, but it does provide a secondary reinforcement of existing taboos (see U.S. Senate 1950; West 1977, p. 307).

against such behavior are regarded will reflect the sum of the perceived threats to various boundaries which a breach of the taboo produces.

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Dependency Theory and Taiwan: Analysis of a Deviant Case¹

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The case of Taiwan represents a challenge to two predictions from dependency theory: that foreign economic penetration leads to slow economic growth and also to heightened inequality. Since the early 1950s Taiwan has received massive foreign aid and investment, but it has also had one of the highest sustained rates of growth in the world, while income inequality on the island has decreased substantially. An examination of this deviant case is pursued by consideration of the various mechanisms dependency theorists claim are responsible for the linkage of foreign economic penetration to stagnation and inequality. In the Taiwan case, none of these mechanisms work out as predicted. Instead, a variety of factors—including the nature of the Japanese colonial experience, the emphasis on labor-intensive enterprises, and the absence of an entrenched bourgeoisie—created a situation in which both rapid growth and increasing equality could occur. Consideration of Taiwan draws attention to flaws in the arguments of most dependency theorists and suggests a more optimistic picture for at least some developing societies that have to deal with foreign economic penetration.

In recent years a large number of empirical studies have been carried out to test aspects of dependency theory (e.g., Chase-Dunn 1975; Robinson 1976, 1977; Bornschier, Chase-Dunn, and Robinson 1978; Bornschier and Ballmer-Cao 1979; Snyder and Kick 1979). Attention has focused on two key hypotheses derived from that theory: (1) The greater the degree of reliance on foreign investment and foreign aid in a country the slower the rate of economic growth there. (2) The greater the degree of reliance on foreign investment and foreign aid in a country, the greater the degree of

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inequality of incomes there.² These hypotheses are seen as flying in the face of prevailing ideas derived from neoclassical economics and modernization theory. And, even though there are inconsistencies, in general the cross-national comparisons in these studies provide some support for both hypotheses. (A convenient summary of a number of studies is provided in Bornschier et al. [1978].) Findings such as these have led some scholars to argue that if a poor country wants to develop rapidly and equitably it must be prepared to close itself off from most foreign economic influences (Beckford 1971; Chirot 1977; Moulder 1977).

However, there are a number of countries whose experience seems in conflict with these tenets of dependency theory. The most striking case is Taiwan.³ For 50 years (1895–1945) Taiwan was a Japanese colony and hence in a condition that would seem to qualify as a rather extreme form of economic dependence. After World War II and the Chinese Civil War Taiwan became a political client of the United States and received massive amounts of American aid. When U.S. aid was gradually phased out in the 1960s, large amounts of foreign investment poured into Taiwan in its place, and Taiwan became extraordinarily dependent on foreign trade. But when we look at the other side of the picture the presumed negative consequences of this dependency are not apparent. Taiwan has had one of the highest sustained growth rates of any country in recent years, averaging around 9% (in gross domestic product at constant prices) for the period from the early 1950s to the early 1970s (and around 6% in per capita increase in GDP; see Kuznets [1979, p. 45]). Furthermore, the rate of economic growth has increased noticeably since foreign investment replaced foreign aid as the main source of overseas capital. As a result of this growth, some dependency-oriented writers have seen fit to declare that Taiwan has graduated from peripheral to semiperipheral status (Chirot 1977; see also Snyder and Kick 1979).

Perhaps more remarkable have been the trends in inequality. In 1953 a Taiwan household survey revealed a Gini coefficient of income inequality of .576, indicating a substantial amount of inequality not atypical for countries as poor as Taiwan was at that time (with a per capita income of less than U.S. \$100). Since then available evidence indicates that the inequality of incomes in Taiwan has decreased steadily and substantially, resulting in a Gini coefficient from an island-wide survey in 1972 of only .290. This

² We use the term dependency theory here to include "world-systems" theory and other variants. We will not discuss another form of dependency, that based on foreign trade, although in this realm Taiwan is just as clearly a deviant case for the theory (see discussion in Galtung [1971] and in Tyler and Wogart [1973]).

³ There is some doubt about how appropriate it is to refer to Taiwan as a country, since the government there and its rival in Peking both claim that there is one nation, China, of which Taiwan is a part. Since de facto and economically Taiwan and China are separate we will ignore the issue and refer to Taiwan as a country.

figure would seem to indicate less inequality than in almost all developed societies; it is comparable to the low levels in East European socialist societies (Jain 1975).⁴ This trend not only is in clear conflict with the expectations of dependency theory, but also flies in the face of previous findings of conventional Western economics, which have led to assertions of an "inverse *U*-trend," with inequality increasing at first with growth, and leveling off and declining only at advanced stages of economic development (Kuznets 1955; Lewis 1955; Adelman and Morris 1973; Paukert 1973).

On these grounds, the experience of Taiwan merits examination. Our procedure in the succeeding pages will be as follows: First, we will present more details and figures about the trends in Taiwan, particularly over the past three decades, in order to examine its apparent successes. Then we will use the dependency theory literature to investigate the mechanisms that are supposed to connect foreign economic dependence with slow growth and inequality and try to analyze whether they were operative in the case of Taiwan and, if they were not, to determine the reasons. We shall be particularly interested in whether the factors that countered the negative effects of dependency in Taiwan are idiosyncratic and unrepeatable, or whether they suggest strategies applicable to other poor countries that wish to develop rapidly and equitably.

TAIWAN IN THE 20TH CENTURY

Taiwan is an island about the size of the Netherlands, with a current population of around 17 million. Prior to 1895 it was a province of China, albeit a rather weakly incorporated one, with a sizable aboriginal population. As part of the settlement of the Sino-Japanese war, Japan obtained control of the island in 1895. For the next 50 years it exploited the re-

⁴A word must be said about the quality of data collected on Taiwan. In general Taiwan has a much better developed statistical reporting system than other countries at a comparable stage of development, so we will generally accept figures from Taiwan as relatively reliable. However, the 1953 Gini index was computed from a household survey whose sample was not very representative. Since urban areas, where incomes were more unequal, were oversampled, the figure of .576 probably overstates the degree of inequality to some degree, perhaps by as much as 20% (see the discussion in Ho [1978, pp. 141-42]). However, more recent data derived from island-wide representative samples reveal a clear decline in inequality from the 1950s to the low level indicated in 1972, so the lack of reliability of the 1953 figure does not negate the conclusions stated in the text. To some degree these relatively recent family income survey data may still underestimate nonwage (e.g., profit and dividend) income and thus the true extent of inequality. However, since such underestimation is likely to be a more serious problem in other developing societies with poorer statistical systems, and since there is no reason to assume that the extent of this underestimation has increased, the two conclusions reached here still seem warranted: inequality in Taiwan is relatively low and has declined over time.

sources of its new colony. There is very little mineral wealth on Taiwan and no petroleum to speak of, but the island does possess relatively fertile land and a mild, semitropical climate. Over time, Taiwan was expressly oriented to serve Japan as a source of agricultural products, particularly sugar and rice. Japan in return shipped manufactured goods to Taiwan for sale there. What little industry developed on the island was small in scale, except in the area of food processing, and it was almost solely in the hands of the Japanese. Therefore there was little chance for Taiwanese to move into the industrial elite, not to mention the political elite. However, Japan did invest heavily in the infrastructure needed to make Taiwan pay off as a source of agricultural produce. Irrigation networks, rural roads, railways, harbors, modern banks and credit cooperatives, rural schools, and other facilities were invested in, and agricultural production, literacy, and health standards increased considerably. Japan later reaped sizable profits from its agricultural colony. It has been estimated that Japan extracted about 10% of Taiwan's GNP in the years after 1911, far more than the estimated 1.5% of national income the British extracted from India in the same period (see Little 1979, p. 453). The Japanese did not tamper in major ways with local social structure, however, leaving the patterns of land ownership relatively undisturbed. Agriculture remained on a family farming and tenancy basis. Only shortly before the Pacific war did any substantial shift toward industrialization outside food-related industries occur, and that shift was stimulated by the possibility that Taiwan would be cut off from industrial goods in the coming conflict (see Tsurumi 1977; Ho 1978).

After the Japanese lost the war, the island was retroceded to China. Initially, it suffered under a corrupt and incompetent warlord governor, Ch'en Yi, who did little to repair the war-damaged economy; his attempts at expanding state control of the economy appear to have been motivated more by carpetbagger instincts than by any goal of economic stabilization and development.⁵

During 1948 and 1949, the Chinese Nationalists found refuge in Taiwan as they lost the Chinese Civil War. More than a million and a half "main-

⁵ One reason which is sometimes given for discounting Taiwan as an example of successful economic development during the postwar period is the claim that it was easy to build on the achievements of the Japanese colonial period. In fact, between 1942 and 1949, the institutional structure which had helped to push Taiwan's productivity to high levels suffered grievously. A sophisticated observer of Asian agricultural conditions made the following observations following a two-week trip through west coast rural Taiwan in September 1949: "Of all the farmyards I have seen in the Far East, Southeast Asia, and in the Middle East, that of the average Taiwanese tenant is among the worst, both in appearance and in equipment. Tenants' huts, so-called barnyards, equipment, and livestock, as well as their health point to nothing but poverty" (Ladefinsky 1977, p. 98). Per capita product in Taiwan's agricultural sector did not surpass peak 1938 levels until after 1960 (computed from Ho 1966).

landers" fled to Taiwan at that time; they and their descendants now constitute about 15% of Taiwan's population. In the ensuing years the United States stepped in to defend Taiwan against the danger of the Chinese Communists pursuing the civil war to its conclusion by seizing the island. American advisors also offered counsel on economic, military, and other matters in subsequent years, although the extent of their influence can be debated.⁶ Over the period from 1952 to 1968 more than U.S. \$1.7 billion in aid was extended to Taiwan, excluding aid in the military assistance program (the latter totaled about \$2.3 billion over the years). The figure for nonmilitary aid, which means something like U.S. \$187 for every person on Taiwan, constituted 40.7% of gross domestic capital formation (GDCF) in the period from 1952 to 1960, a figure which fell to 12% in the years 1961-68 (Ho 1978, pp. 111-20; Ranis 1979, p. 250). Private foreign investment was minimal before 1960, but then began to increase substantially, constituting 1.7% of GDCF in 1952-60, 5.7% in 1961-68, and 6.9% in 1969-74 (Ho 1978, pp. 111-20; Ranis 1979, p. 250). Also, Taiwan has utilized international credit markets increasingly in recent years, including credits of more than U.S. \$6 billion from the Export-Import Bank to finance 10 major construction projects which are now nearing completion.

A number of other developments over the years deserve note. In the early 1950s, an island-wide land reform was carried out, which placed a limit on the amount of land a family could own, and through which the government bought out large landowners with bonds and with shares in the government-run companies that had been taken over from the Japanese. For most of the 1950s, the government pursued a conventional import-substitution program in its efforts to develop Taiwan's industrial base, with tariffs, import controls, and multiple foreign exchange rates. Toward the end of this period this strategy began to peter out, as the domestic market was increasingly saturated with goods produced by local industry. By about 1961 Taiwan had shifted to an aggressive export-promotion program instead, with exports of manufactured goods taking over increasingly from agricultural products in Taiwan's trade with the world. By this time exchange rates had been unified, and import and tariff regulations were eased to enable Taiwan's industry to purchase needed technology and raw materials from abroad in a rational manner. Taiwan also began to try aggressively to attract foreign investment. After 1965 special export-processing zones were set up in major port areas. Their purpose was to

⁶ In one area at least the American influence was relatively direct and undisguised. The Joint Commission on Rural Reconstruction, the major body guiding agricultural development and channeling American aid to that sector, was established under joint American and Republic of China control, and had Americans as well as Chinese guiding its affairs (see Shen 1964, 1970).

allow foreign firms to build plants there that would take advantage of the cheap labor force, with most technology and raw materials imported, and with the output almost totally exported for sale elsewhere. After these shifts in policy the economic growth rate increased markedly in the 1960s. Exports also increased in importance, growing to a size equal to more than 50% of Taiwan's gross domestic product in the mid-1970s (Scott 1979, p. 350). By about 1968 the legacy of growth began to be felt in a perceptible labor shortage, which contributed to rising wage rates. Later, in 1973, Taiwan was badly hit by the oil embargo and world recession, but her economy recovered rapidly and has continued its pattern of fairly steady growth since that time. (On these trends, see Ho [1978] and Kuznets [1979].) In table 1 we present selected indicators dealing with the Taiwan economy over the past few decades.

Before proceeding further, we must be more precise about how good a test case for dependency theory Taiwan presents. Precision involves sub-

TABLE 1
SELECTED INDICATORS OF TAIWAN'S ECONOMIC AND SOCIAL DEVELOPMENT

Indicator	1953	1958	1963	1968	1973	1977
Gross domestic product (GDP) index numbers...	51	71	100	159	268	336
Per capita GDP index numbers.....	71	83	100	140	211	246
Agricultural production as a percentage of GDP....	34	27	23	19	13	12
Gross domestic capital formation (GDCF) index numbers.....	42	54	100	253	434	528
U.S. aid as a percentage of GDCF.....	36.9	44.2	4.0	11.6*	-20.3*	N.A.
Foreign investment as a percentage of GDCF....	1.6	.8	4.7	7.9	8.9	N.A.
Exports as a percentage of net national product (NNP).....	11	N.A.	16	N.A.	58	N.A.
Manufactured goods as a percentage of all exports	8.2	15.4	38.8	64.5	83.9	84.9
Gini coefficient of income distribution.....	.56	.44†	.32‡	.33	.29§	N.A.
Income share of bottom 20% of all households (%).....	1.7	N.A.	7.7	7.8	8.9	N.A.

SOURCES.—Rows 1 and 2: Directorate-General of Budget, Accounting and Statistics 1979, pp. 294-95 (in constant prices). Row 3: *ibid.*, p. 295. Row 4: computed from *ibid.*, pp. 316-17 (in 1971 constant prices). Row 5: from Kanis 1979, p. 250. Row 6: *ibid.* Row 7: from Little 1979, p. 473. Row 8: 1953 and 1958 figures are computed from *Industry of Free China* 1962, pp. 106-7, table 22 (using cement, "ores, metals and metallic products," "other merchandise," and "cotton piece goods" as categories encompassing manufactured goods for export); for 1963-77 data, see Directorate-General of Budget, Accounting and Statistics 1978, pp. 250-51. Row 9: Ho 1978, p. 141; Fei et al. 1979, pp. 66, 92-93. Row 10: Ho 1978, p. 141.

* No further economic aid commitments were made to Taiwan after 1967; the negative figure for 1973 reflects payments on long-term loans.

† 1959 data.

‡ 1964 data.

§ 1972 data.

stantiating our claim that Taiwan's degree of dependency, which accompanied her rapid growth and increasing equality, has been relatively high by world standards. One problem here is that Taiwan's development has occurred in three phases and, while dependency has been strong throughout, the forms it has taken have shifted dramatically, as we have already indicated. We will assume, in the absence of good comparative figures, that the fact of being a Japanese colony during the first phase removes any doubt about the relatively high degree of economic dependency experienced by Taiwan in that period. In the second, postwar phase (from 1945 into the 1960s) Taiwan received unusually large aid shipments. From 1958 to 1965, for example, Taiwan ranked fifth in the world in the total amount of U.S. aid per capita, with almost six times as much aid received per capita as the average for 120 countries (Taylor and Hudson 1972, table 6.4). However, little direct foreign investment flowed in during the early years of this phase. Only in the 1960s did direct foreign investment in Taiwan begin to "take off," but then it did so impressively. From a relatively small base established earlier, in the late 1960s direct foreign investment in Taiwan grew almost 30% faster than the mean for a sample of 88 other developing countries, according to the indicator used by Bornschier et al. (Bornschier, personal communication). So in this third phase Taiwan's degree of dependency also was above the average. Only if one claims that one particular form of foreign influence is the crucial one for producing dependency's negative consequences can one discount Taiwan as a test case. Since most dependency writers assume that the various forms of dependency have similar effects, the use of Taiwan as a test case seems fully warranted.⁷

DEPENDENCY AND SLOW ECONOMIC GROWTH

What linkages are seen between dependency and retarded economic growth? Several major arguments have been presented, and we will try to discuss each in the context of Taiwan. (For a good general summary of such arguments, see Chase-Dunn [1975].)

1. Dependent countries suffer from direct exploitation. Foreign firms repatriate profits overseas, rather than reinvest them in the domestic economy, thus limiting the growth that can be achieved (see Amin 1974; Frank 1969).

⁷ One article (Bornschier et al. 1978) does argue that the flow of direct foreign investment has an initially positive impact on economic growth, while a large accumulated stock of foreign-owned capital depresses growth. (In other words, in the short run foreign investment helps, but in the long run it hurts.) If this is the case Taiwan could be seen as benefiting from its postwar interlude of relatively low foreign investment. However, Bornschier et al. argue also that in the long run dependence on foreign aid hinders growth, and that both foreign aid and investment ("flows" and "stocks") foster inequality. So even with their more specific predictions of the effects of dependency, the case of Taiwan remains deviant.

For the Japanese period this picture seems at least partially accurate. While large initial investments were made by the Japanese in the early years of colonial rule, it is clear that Japanese firms on the island subsequently reaped large profits and repatriated a goodly share back to Japan (see Office of the Chief of Naval Operations [1944, p. 86] on the links between the colonial government and the *zaibatsu* firms). However, since the rate of economic growth until just before the war was quite high in comparative terms for the period (more than 4% a year during the years 1911–38), it is at least clear that this did not result in the absence of growth or “underdevelopment,” and this should remind us that economic investment is not a zero-sum game, as some dependency arguments seem to imply.

In the postwar period the situation changed. Initially, foreign capital came mainly in the form of aid rather than investment, and most aid was in the form of grants rather than loans (see Ranis 1979, p. 244). As a result, there was clearly a large net flow of capital into Taiwan, rather than away from it. Most nonmilitary foreign investment in this period took the form of contracts to build dams and other kinds of projects, which involved one-time profits and no issues of reinvestment or continuing exploitation. By the 1960s this situation changed, and foreign investment in manufacturing firms was eagerly attracted into Taiwan (Ranis 1979, pp. 245–50).

The rapid rate of expansion of foreign investment in Taiwan in the 1960s (climbing somewhat erratically from \$19.9 million newly invested in 1964 to a peak of \$248.9 million in 1973) was clearly faster than the rate at which profit accrued on such capital. While some of the profits were repatriated to developed countries, a large proportion was reinvested in operations in Taiwan. The international capitalist world system facilitates the flow of capital into reinvestment in the most profitable ventures. The high rate of profitability and profit potential of operations on the island continued to attract new firms and more foreign capital into the island (much as neoclassical economic arguments say it should), and these firms fueled further expansion. Again, a zero-sum conception of the process hinders understanding. It seems ironic that dependency theorists, who generally fault others for using a closed-system, nation-state frame of reference, should in this instance fail to appreciate the consequences of the openness of an economy to the “world system.”

2. Foreign suppliers and economic interests tend to “dump” outmoded equipment and technology on poor countries, so that they cannot compete effectively in international markets or grow very fast (see Frank 1969; Sau 1978, pp. 79–80).

On this issue we know of no relevant evidence, pro or con, indicating to what extent Taiwan has received used or outmoded equipment for its industry. However, we might note in passing that one of the fathers of depen-

dency theory, V. I. Lenin, did not agree with this argument. He felt that capitalist countries could be counted on to sell advanced technology to socialist countries to enable the latter to win out in world competition. More to the point in regard to Taiwan, this argument overlooks the differences in factor endowments and economic emphasis between countries. The most modern technology in the West often means technology that will save on labor costs, and of course Taiwan's export-oriented industrialization strategy depended, particularly at first, on taking advantage of its low labor costs. By maintaining low labor costs, even firms having less modern technology might be able to compete effectively with overseas firms having more modern technology. In fact, it would be economically irrational for Taiwanese entrepreneurs to purchase an expensive new machine that saves labor rather than an older and cheaper model that requires more labor, assuming that the items produced do not differ markedly in quality (see Galenson 1979, pp. 388-90). Since the most dynamic sectors of industry in Taiwan have generally been the labor-intensive light industrial sectors, it is not clear that having more modern technology would have produced faster economic growth. This argument seems to suffer from the misconception that modern, capital-intensive firms are invariably superior in terms of efficiency and growth potential—a view that is clearly not true for Taiwan (see Ho 1980).

3. Dependency on foreign interests and foreign economic penetration keep the state weak and prevent it from effectively playing its necessary role in protecting domestic industry and fostering economic growth (see Baran 1956; Galtung 1971; Hayter 1971; Hensman 1971; Rubinson 1977).

For the Japanese colonial period this does not seem to be an accurate description. The government played a central role in investments in schools, roads, railways, irrigation networks, and other infrastructures that facilitated economic growth. But of course the government was a colonial one, manned almost entirely by Japanese. Furthermore, Japan provided Taiwan with a protected and expandable market for its exports that kept them from having to compete directly against cheaper goods from other countries (e.g., sugar from Java, rice from Southeast Asia). In other words, the fact that the Taiwanese economy was directly under the thumb of the Japanese government and large *zaibatsu* cartels did not mean that no strong government existed in Taiwan to foster economic growth. Power and control, like profits, cannot be seen as simple zero-sum phenomena.

After the war the Japanese colonial government and personnel were booted out, but they were replaced by what eventually became more than a million "mainlanders," who monopolized high political posts and in the 1950s maintained a military machine of close to 1 million men (also dominated by mainlanders). While this is admittedly a rather unusual after-

math to a period of colonial rule, it would not have been possible without massive U.S. foreign aid. American aid made it possible to maintain a bloated governmental bureaucracy and one of the highest soldier/civilian ratios of any country. The large amounts of funds controlled by the government, and the massive army, are clear indicators of high state power, as these are usually operationalized in dependency-theory studies.⁸ In other words, economic dependency seems to have been an essential element in maintaining a strong state apparatus in Taiwan, rather than one keeping it weak. Here we might also note that the interests of the American state do not necessarily coincide with those of American multinational corporations, an equation that is usually assumed in dependency arguments. American governmental interest in propping up the Taiwan government as a rival to the new Communist government on the mainland clearly took primacy over concern with facilitating the penetration of the Taiwan market and provided justification for the large amounts of aid.⁹ It would be hard to argue, then, that Taiwan's dependence kept the government weak.

Furthermore, most observers credit the government in Taiwan with playing an important and generally positive role in fostering rapid economic development. The pattern of this government influence, however, is quite different from the standard "late industrialization" syndrome depicted by Gerschenkron, in which state investment in large-scale, capital intensive enterprises spearheads growth. Instead, after an interlude of neglect and mismanagement, the government fostered the reconstruction and further development of the infrastructure developed during the Japanese period. Considerable effort and resources were expended on promoting agriculture, schools, and public health and improving the transportation system. Taiwan seems to be one of the few places in which government pledges to aid agriculture and promote overall growth have been more than rhetoric (Lipton 1977; Griffin 1973). Although in more recent years the government has moved to undertake government-controlled, capital intensive projects (i.e., the "10 major construction projects"), much of Taiwan's industrial growth to date has been fostered indirectly, by promotion of conditions conducive to entrepreneurial activity and profits. The resulting pattern of balanced

⁸ Government consumption equaled 38% of Taiwan's GNP in 1951 and 27% in 1973 (Ho 1978, p. 230), both relatively high figures. In 1965 the reported (and probably underestimated) figure of 8.1% of the adult population in Taiwan in the armed forces was third highest in the world (after Switzerland and Israel; see Taylor and Hudson [1972, pp. 38-41]). It might be noted that American advisors consistently advised the government to reduce the size of the army, but that their advice was ignored.

⁹ In fact, as we have noted, America had little established investment or market stake in Taiwan, and only at a comparatively late stage did U.S. policy evolve a concern with fostering American private investment in Taiwan (see U.S. Department of Commerce 1959; Schreiber 1970).

growth was not inevitable; it depended on conscious government efforts to promote it (see Ho 1978).¹⁰

4. Dependency leads to susceptibility to price manipulations in the domestic and overseas markets. The domestic market becomes flooded with imported consumer goods, while exports to pay for them are harmed by the instability of world demand and prices. The result is often trade deficits, growing indebtedness, and less capital to invest in economic growth (see Prebisch 1950; Emmanuel 1972; Amin 1974).

Here several related arguments are combined, even some that appear contradictory. For instance, in some dependency arguments the imports that are flooding in are seen as cheap foreign manufactured goods designed to undercut domestic industry, while in others they are seen as expensive luxury items that will soak up funds that could otherwise be invested. But in general the claim is made that dependency leads to (1) insufficient control over the domestic economy for the purpose of restricting imports and protecting native industry, and (2) reliance on highly unstable export markets as a source of funds, with the instability making consistent planning and investment impossible and competition with more advanced countries very difficult.

In Taiwan during the Japanese period one-half of this picture appears correct, while the other does not. Domestic industry was suppressed, and Taiwan was flooded with Japanese manufactured goods (more the cheap than the luxury variety). But on the other hand, a stable market for Taiwan's agricultural exports was provided by Japan, and this was a protected market in which Taiwanese rice and sugar could be sold at higher prices than other areas of Asia were offering. The result, as we have mentioned, was relatively predictable and growing trade between Japan and Taiwan, and a fairly impressive rate of economic growth in Taiwan until the war, although the growth took place mainly in the agricultural and agriculture-related sectors of the economy.

After the war Taiwan was able to implement a relatively effective import-substitution policy in the 1950s and to develop its domestic industries. It is not entirely clear why foreign firms did not stake out claims to the Taiwan market and undermine efforts to limit imports. One probable reason is Japan's domination of Taiwan's trade before the war. With the Japanese

¹⁰ That an emphasis on relatively *laissez-faire* economic guidance emerged in Taiwan was not inevitable, since it conflicts sharply with the traditionally intrusive role of the government in economic affairs in China (see Little 1979, p. 475). The fact that the mainlanders dominating the government promoted opportunities and profits for entrepreneurs who were mainly Taiwanese was also not inevitable, since they could have tried to monopolize these themselves. Other countries ruled by "outsider" groups, such as South Vietnam under Ngo Dinh Diem's clique of northerners, have not been noted for their promotion of local entrepreneurship.

temporarily out of the picture, American and other firms were free to enter, but they had little experience in dealing with the Taiwan market. Also, the political and economic situation after the war was somewhat forbidding. The Taiwanese were poor and in the midst of reconstructing their economy, with the help of American aid. There were also severe political and economic conflicts between the Taiwanese and their new mainlander overlords; these culminated in the uprisings of February 1947, which were put down in a sea of blood. Subsequently, of course, the mainlanders lost control of China and made their headquarters on Taiwan, and the chances of the Chinese Communists trying to assault the island remained alive; this made the political climate for trading in Taiwan seem risky until at least the mid-1950s. The more or less perpetual state of national emergency after 1949 generated by the conflict with the Communists was used also to justify a variety of luxury and special war taxes that tended to keep the price of imported goods high. In general, then, foreign trading firms did not have much stake in the Taiwan market to protect at the time that import-substitution policies were put into effect. Even later, Taiwan was seen more as a place to produce goods cheaply for sale overseas than as a market for the sale of foreign goods.

On the export side of things, too, the dependency argument does not fit the Taiwan case. In fact, one could argue that Taiwan has accrued some benefits from the volatility of world markets and prices. This has occurred because most of Taiwan's development has not followed a capital intensive mode, but a labor-intensive, almost protoindustrial mode. In textiles, for instance, there are not only some large factories but also electric looms scattered in homes and shops in many parts of the island. The people working these looms take orders from putting-out agents, who get them from the myriad small export agents, who in turn deal with foreign buyers. The decentralization of this system means that it can quickly adjust to changes in the world market. For example, if styles suddenly change, foreign buyers besiege export agents with orders, and the loom operators can quickly adjust and produce the new style of sweater or skirt. If export agents go bankrupt (as they are almost encouraged to do by Taiwan's lenient bankruptcy laws), the producers turn to other agents; they have lost little because almost all contracts are short-term ones. Similarly, an export agent can put out contracts with a variety of producers and can attempt to get orders from a variety of foreign buyers. In the Taiwan case, then, it seems clear that changes in the international market can produce opportunities to which small and decentralized producers can respond more quickly than large and heavily capitalized ones. And Taiwan has gone from having a large trade deficit, financed by U.S. aid, in the 1950s, to accumulating a sub-

stantial trade surplus and growing foreign exchange reserves in the 1970s.¹¹ In short, a combination of factors (the colonial legacy, initial political uncertainty, a strong state, and diversified, labor-intensive development) allowed Taiwan to exploit opportunities for world trade instead of being exploited by them.

5. Dependency causes such economic growth as occurs to be confined to small enclaves, and these enclaves and the native bourgeoisie in them are more oriented to foreign economic interests than domestic ones. Linkages with the rest of the domestic economy are minimized, reducing the multiplier effects of foreign investment. The result is unbalanced development or economic dualism, with a division between a small modern economic sector and the remaining backward parts of the economy becoming more pronounced (see Baran 1956; Frank 1969; Furtado 1972; Amin 1974; Chirot 1977).

For the Japanese period this characterization is clearly inaccurate because, as Taiwan was developed as a "breadbasket" to serve Japan, market relations penetrated widely in the agricultural sphere. The main crops of interest were rice and sugar, and these were grown by family smallholders and tenants, rather than on plantations, a situation the Japanese did not alter.¹² Fostering the development of this kind of agriculture entailed large investments in irrigation works, schools, roads, banks, marketing agencies, fertilizer manufacture, and so forth. The effects were very clearly diffused throughout the countryside, rather than confined to export enclaves. Even the industry of the period, which concentrated on things like sugar processing and cement, was dispersed where it could serve the agricultural sector, a development facilitated by the roads and other modern communications developments fostered by the Japanese. So the pattern of growth produced was a broad one, which brought about improvements in the stan-

¹¹ It should be stressed that the legacy of the Japanese period was an essential precondition for this pattern of decentralized industrialization. Without extensive literacy, rural electrification, and roads and other transportation and communication networks, industry would have to be concentrated more in cities and port areas. The success of Taiwan in competing with the advanced countries can be seen not only in the shift to a favorable trade balance, but also in the shifting type of exports. In the 1950s Taiwan was the world's third largest supplier of sugar, after Cuba and the Dominican Republic. However, unlike these countries, Taiwan did not remain "stuck" in a monoculture export pattern. The share of food and beverage products in total exports fell from 55% in 1963 to 11.8% in 1977, while the share of manufactured goods rose from 35.8% to 84.9% in the same period. The share of Taiwan's exports going to the developed countries rose from 49.7% in 1956 to 70.3% in 1977 (figures from Directorate-General of Budget, Accounting and Statistics [1979, pp. 242, 245, 251]).

¹² The Japanese sugar companies did own large tracts of land. However, these were broken into small plots and leased to Taiwanese families on a tenancy basis, instead of being farmed as plantations (see Chen 1975).

dard of living of Taiwan's peasants, at least until the approach of the war.

For other reasons, the Japanese period did not give rise to a large native bourgeoisie or "comprador" class serving foreign trading firms comparable to those developing at the time on the mainland of China. The main reason was that the Japanese had quite a different approach to colonization from that of the Western powers. Taiwan was not only a breadbasket for Japan, but also one place to absorb excess population—some of the noninheriting sons of the increasingly overcrowded Japanese homeland. By the end of the war more than 300,000 Japanese civilians lived in Taiwan. They supplied the bourgeoisie, as well as the political elites, and their presence prevented all but a few Taiwanese from rising into managerial or other elite positions. At the end of the war deportation of the entrenched bourgeoisie, almost solely Japanese, resulted in the need to recruit a new managerial class almost from scratch.

After the war the pattern of growth in Taiwan shifted increasingly from agriculture to light industry, and manufactured products increasingly displaced agricultural ones in Taiwan's exports. But the pattern of growth of industry was decidedly not confined to a metropolitan enclave. Instead, manufacturing enterprises sprang up in rural areas and small cities as well as in large cities, and in certain periods rural industry grew even faster than industry in Taipei and other large cities.¹³ This broad growth was made possible by several existing features—the infrastructure of roads, railways, and electric power, not to mention rural education, built up during the colonial period and afterward, and the labor-intensive nature of Taiwan's development, which made it useful to locate near rural sources of labor—as well as by government incentives for firms to locate in rural areas. The pattern of economic development was one that minimized dualism and spread the effects of industrialization very widely.

Later on, after 1965, the government expressly fostered the development of export-processing zones (EPZs), and these fit the enclave argument more closely. As noted earlier, the concept behind EPZs is that foreign firms bring in capital and raw materials, employ Taiwanese labor, and then sell the finished products back overseas without having to pay the usual import duties and fees. But even the several hundred foreign firms in these zones do not fully fit the enclave argument. For one thing, these firms draw labor from the countryside and even rely to some extent on daily commu-

¹³ Samuel Ho (1979, p. 80) presents figures that show that from 1956 to 1966 employment increased annually by 5.3% in Taipei, 5.6% in other metropolitan areas, 5.4% in minor cities, and 3.8% in rural areas. This is already a fairly even picture of growth, but in manufacturing enterprises only, the annual rate of increase in employment was 5.5% in Taipei, 5.6% in other metropolitan areas, 5.2% in minor cities, and 7.2% in rural areas. In other words, employment in manufacturing enterprises increased faster during this period in rural areas than in all urban areas.

ters, so that their effects extend outward beyond the zones (see Wu 1976). These firms also draw from the same labor pool as domestic firms, and they do not pay their workers the noticeably higher wages that the "labor aristocracy" of the enclaves is often depicted as receiving. The required skill level of jobs in the export-processing zones is not appreciably higher than in many other sectors of the economy; there is a high degree of labor turnover in manufacturing (including job turnover in the EPZs), and this turnover provides small domestic factories with workers already inured to proletarian labor discipline. Furthermore, the growth of EPZ firms, particularly in electronics, has provided spin-off business and joint ventures for locally owned firms, stimulating the growth of an indigenous industry (Little 1979, p. 479). Finally, by the time the export-processing zones began to be built up in the late 1960s, Taiwan's industrialization had already developed enough so that these firms could not dominate the picture and subvert the relatively balanced growth pattern that had emerged. (The EPZ firms produced only about 9% of Taiwan's total exports in 1973; see Ho [1978, p. 198].)

The absence of a strong comprador class may also be important. When the Japanese were deported, they were in a certain sense replaced by mainlanders, but not in a total sense. While the political and military hierarchies were monopolized by mainlanders, the same cannot be said for the economy. Many of the capitalists and skilled managerial personnel who fled from China went to Hong Kong, rather than to Taiwan, and the number who went to Taiwan was insufficient to control the new economic opportunities. Of the 321 largest industrial corporations in Taiwan in 1976, 89 were owned by mainlanders, 21 by foreign interests, and the rest by Taiwanese (see Little 1979, p. 479). For small and medium-size industries the proportion of Taiwanese ownership is much higher. Much of the dynamic growth of the Taiwan economy can be attributed to the many small-scale Taiwanese entrepreneurs who arose to fill the vacuum left by the Japanese (see Mark 1979). These new entrepreneurs were not an entrenched group tied to foreign interests, and, in fact, in the 1950s import-substitution phase they were mainly oriented to the domestic market. Those whose enterprises became large and wealthy, and who established ties with foreign firms, were still not able to control governmental policy, which has remained predominantly in the hands of mainlanders. Hence at least initially after the war Taiwan was characterized neither by a government that was directly serving the interests of an entrenched bourgeoisie nor by a bourgeoisie whose fortunes were tied to foreign economic interests. Instead there was a mainlander-dominated government working to preserve its own position and making use of Taiwanese entrepreneurs, foreign firms and governments, and other actors for that purpose. And since this government came to see economic gains as a more effective prop of its shaky

legitimacy than its dubious goal of recapturing the mainland, overall economic gains have remained a preeminent concern. In general, then, Taiwan throughout the 20th century has been heavily dependent on foreign interests economically, but nonetheless has not developed a lopsided, dualistic economic growth pattern.

6. Dependence on foreign aid and credit reduces domestic capital formation, resulting in a lower rate of economic growth (see Frank 1969; Griffin and Enos 1970).

Authorities are divided on the impact of massive American aid in the 1950s and early 1960s; some see it as contributing to capital formation and economic growth (Jacoby 1966), while others feel it did not serve this purpose (Griffin 1973). Although the trend at any one time may be unclear, over the years it is easier to interpret. Initially, rates of gross domestic capital formation in Taiwan were relatively modest (14% of GNP in the early 1950s), but over time this rate tended to increase, until it reached quite high levels (28% of GNP in the period 1970-73). This change coincided with the phasing out of American aid, which has not been an important factor in capital formation in Taiwan since the mid-1960s.¹⁴ In other words, effective mechanisms for domestic capital formation were developed in spite of massive foreign aid, and these mechanisms made it possible to terminate the aid.

A complete account of the sources of domestic capital formation is not possible here, but we can see certain important processes at work. In early 20th-century China it was common for urban merchants and industrialists to use their profits to buy land in the countryside, for land was seen as a more secure and prestigious form of investment than was industry. On Taiwan after the 1950s the land-reform program placed a ceiling (of about three hectares) on the land that a family could own and gave remaining tenants long-term contractual rights to the land they were renting. Thus land was no longer an attractive investment opportunity. At the same time Taiwanese initially saw political and military careers blocked by the dominance of the mainlanders, while entrepreneurial careers were relatively wide open. In other words, the structure of mobility opportunities was changed so that there were strong incentives for ambitious Taiwanese to engage in entrepreneurial activities and to reinvest their profits in further expansion of industrial and commercial enterprises. We would argue that this restructuring, combined with the high rate of growth, and profits, of

¹⁴ Griffin makes much of the fact that, over time, U.S. aid decreases as domestic investment increases (Griffin 1973). He has reversed the causality: U.S. aid decreased because domestic investment and economic growth increased (or, as the USAID metaphor had it, "Taiwan graduated"). There appears to be a certain obtuseness among dependency theorists who fail to see that aid may be concentrated on countries which appear to need it and not lavished on states making good progress. As a result, there may appear to be a time-series correlation between lack of progress and receiving lots of aid.

the economy, was a prime factor promoting domestic capital formation, regardless of foreign aid shipments.

We have considered six different kinds of arguments why dependency might be expected to inhibit economic growth. In general, none of these arguments fits Taiwan. The reasons are complex and varied; they involve features of the Japanese colonial experience, the nature of mainlander rule in Taiwan, the labor-intensive growth strategy pursued, the lack of strong foreign investment and trading stakes in early postwar Taiwan, and other factors. To say that the dependency mechanisms have not operated in Taiwan is not the same as claiming that foreign investment and aid in Taiwan were consistently positive contributors to economic growth, or that by rejecting these arguments we have shown why Taiwan has been able to grow so remarkably fast.¹⁵ For the moment, we are simply concluding that a variety of factors made it possible for foreign economic dependency to exist without seriously retarding economic growth on Taiwan. Before considering further what damage this conclusion implies for dependency theory, we need to consider the other key hypothesis drawn from the theory, which is that dependency fosters economic inequality.

DEPENDENCY AND ECONOMIC INEQUALITY

In this section we will try to explain why Taiwan's income distribution has not become increasingly unequal, in spite of her dependence on foreign aid and investment. Here we will focus almost exclusively on the postwar period, as we do not have income-distribution data for the Japanese period. Because arguments about economic growth and income distribution are not completely separate matters, some of what we will cover here will be obvious from the previous section. What, then, are the various mechanisms by which dependency is seen as fostering economic inequality?

1. Dependency fosters unbalanced development, economic dualism, and privileged enclaves, all of which lead to a deterioration in income distribution, because traditional or new elites and labor aristocracies and urban

¹⁵ We have already noted the debate about the effects of foreign aid. Parallel issues could be raised about foreign investment. Such issues are difficult to resolve, since they involve hypothetical situations: "What if Taiwan had received no aid and no foreign investment?" Without going into detail here, our general feelings are (1) that Taiwan's growth rate has been so rapid that it is hard to imagine that it would have been faster without foreign aid and investment's supposedly "depressing" influence, and (2) that the reasons outlined in the text allowed foreign dependence to have a generally positive impact on growth. But other factors in Taiwan's growth have not been considered here, such as the economic spin-offs of American wars in Korea and later in Vietnam, and the cultural and family-organizational characteristics of Chinese and other "Sinic cultures" that may be conducive to economic growth (in view of the unusual number of "successful" cases of this cultural type—Japan, Taiwan, Hong Kong, Singapore, South and North Korea, and the People's Republic of China).

areas generally benefit disproportionately (see Frank 1969; Amin 1974; Robinson 1976).

We have already argued that, for a number of reasons, Taiwan did not experience this sort of unbalanced growth. Since the growth sectors were initially agriculture and later labor-intensive light industry, the benefits of growth were spread broadly and not confined to enclaves (see Ho 1980). This still cannot account for why inequality actually decreased. To do so fully would require more space than is available here, but a number of key developments should be stressed. First, land reform was carried out successfully in the years 1949–53, and the result was a more equal distribution of land and, hence, of agricultural income. Second, very rapid population growth in the countryside from 1949 until the mid-1960s, when the family-planning program began to take hold, meant increasing pressure on the land and greater need for farm families to look for nonagricultural sources of income. Because the poorest families with the least land felt the pinch first, they procured nonagricultural sources of income to a greater extent than did their neighbors who were better off. (The widespread education and literacy in rural Taiwan, legacies of Japanese colonial rule which were further advanced by the Nationalist government, made it possible for even the poor to qualify for such jobs.) As a consequence, non-agricultural earnings were distributed in a fashion contrary to agricultural earnings, and this tended to equalize the overall income distribution in rural areas.

Also, Taiwan's labor-intensive pattern of development meant that returns to labor increased more rapidly than did returns to capital. Any shift in this direction necessarily fosters income equality, because capital is generally more unequally distributed than labor power. This shift resulted, in part, from the fact that the rate of growth of the economy was so rapid and sustained that it soaked up most of the unemployment and underemployment, so that by the late 1960s a growing labor shortage was perceived. (The maintenance of an unusually large army, which effectively removed large numbers of able-bodied males from economic production activities, helped to hasten the labor shortage.)¹⁶ The labor shortage made it necessary for employers to compete with one another and to offer more attractive wages and fringe benefits, and so wages of even unskilled workers rose, fostering equality (see Fei, Ranis, and Kuó 1979; S. Ho 1979; Y. Ho 1980).

¹⁶ One other factor hastening the perceived labor shortage was ethnic conflict. Since most of the people employed in manual labor jobs in industry are Taiwanese, and Taiwanese is the language of daily communication among them, mainlanders tend to avoid taking such jobs, even when they cannot qualify for something better. This means that a significant portion of the potential labor force, the mainlanders who constitute about 15% of the population, is not generally available for industrial employment.

2. Indigenous elites are opposed to income redistribution, and they use their influence over the government and their clout with foreign interests to forestall any government redistributive efforts. Thus even where a strong state is present, it may work in the interests of the existing elites, rather than foster equality (see Sunkel 1973; Rubinson 1976; Bornschier and Ballmer-Cao 1979).

With regard to this argument, we have already seen that Japanese colonial policy did prevent the emergence of a strong urban bourgeoisie. In the countryside, rural landlords were influential, but the mainlander-dominated government after 1945 was not beholden to them, and so their interests could be sacrificed. Land reform was carried out successfully, and the rural landed class was, in effect, pensioned off, ceasing to be a strong political or economic force (see Yang 1970, pp. 230–60). However, the sacrifice of the rural landlords and the successful land-reform program cannot account for most of Taiwan's favorable trend in income distribution: the relatively high Gini coefficient of .576 in 1953 refers to a time *after* land reform had been essentially completed.

It can be argued that the government in Taiwan has not engaged in much direct redistribution of incomes in succeeding years. The welfare and social service systems are rudimentary (see Galenson 1979, pp. 442–47); taxes are mainly on sales, rather than incomes, and tend to be regressive. The government did continue to invest in rural infrastructure and other projects that might be said to foster more equality indirectly. But at the same time the government in the late 1950s felt that it had to offer various kinds of tax breaks and investment incentives to capitalists in order to foster economic growth; these measures limited the degree to which the government could soak funds away from firms, as had been typical of Chinese governments in the past. On balance the government clearly placed a much higher priority on economic growth than on income redistribution. To some extent, though, the mainlander-dominated government stood above the newly emerging Taiwanese entrepreneurial elite and could not be made to serve their narrow class interests. In the end it seems that the decentralized, labor-intensive pattern of growth itself, not government income policies, produced much of the redistribution that occurred. Studies of Taiwan and other countries argue increasingly, in fact, that the pattern of economic development is more important than governmental redistribution efforts in fostering equality (see Fei et al. 1979).

3. Dependency places labor in a weak position relative to employers, thereby fostering inequality. This can occur partly because a small labor aristocracy in modern firms can arise amid a depressed working class, thus preventing overall working-class solidarity and strong labor organizations from emerging. And it can occur because modern firms tend to be capital

intensive; hence they add to the structural surplus of labor, further depressing wage rates (see Robinson 1976; Bornschier and Ballmer-Cao 1979).

We noted earlier that Taiwan's economic development tended to be labor intensive, and that no enclave labor aristocracy arose, so neither version of this argument seems to fit the Taiwan case. However, we should note also that labor organizations in Taiwan are very weak, perhaps even in comparison with those in other Third World countries. For a number of years unions were moribund, and the government used the perpetual conflict with the Chinese Communists to declare strikes and other labor disturbances strictly illegal (violators of these regulations could be shot). Even in the later years when government-sponsored unions were revived (they had roots in pre-1949 mainland China), they cannot be said to have had much impact on the bargaining position of their members. So strong labor organizations have not been allowed to develop (see Galenson 1979, pp. 425-32). However, labor incomes have risen, owing to the increasing demand for labor already described. As with government redistribution, the position of labor in Taiwan leads one to question a tenet of dependency theory: in the Taiwan case, the role of working-class solidarity and trade unions in promoting equality seems less important than the role of the pattern of economic development and its effect on the demand for labor (see also Webb 1977).

4. Imported and domestically manufactured goods penetrate rural markets and undermine and displace household sideline activities. This deprives rural families of important sources of supplementary income and fosters inequality (see Chen 1936; Fei 1939; Moulder 1977).

With regard to this argument, we have already noted that the facts in Taiwan were quite different. Although traditional handicrafts did decline in importance, new sources of supplementary earnings more than made up for the difference. Several things were involved. New household manufacturing activities, such as the use of electric looms mentioned earlier, arose; members of rural families commuted to work in nearby rural factories and shops; and other family members went off to work in larger cities and remitted part of their earnings home. Over time, the percentage of rural family income derived from nonagricultural activities increased substantially, and as a result overall family income climbed. The share of nonagricultural income in total rural family income had reached a remarkable 53% by 1972 (Fei et al. 1979, p. 113; see also Chinn 1979; Wang and Apthorpe 1974). As noted earlier, the pattern of change fostered equality, because poorer rural families benefited first from these new sources of income.¹⁷

¹⁷ There is considerable evidence that the import-substitution strategy usually recommended to protect native handicrafts and industry generally *aggravates* inequality

5. Foreign firms keep important parts of their organizational structures overseas, or bring in their own technical and skilled personnel. Because of this they do not foster the development of many middle-class and mid-level managerial and bureaucratic occupations that would tend to even out the income distribution (see Bornschier and Ballmer-Cao 1979).¹⁸

We noted earlier that in the Japanese period growth of such jobs among the Taiwanese was severely stunted, as they tended to be monopolized by the Japanese. However, the departure of the Japanese, the arrival of the mainlanders, and the slow growth of direct foreign investment until the 1960s means that economic development went hand in hand with rapid expansion of the domestic middle classes. While the foreign-aid program brought in some foreign experts, they were generally phased out as soon as possible and their jobs were taken over by foreign or domestically trained natives.

Most of the foreign firms which operate in the export-processing zones or other areas of Taiwan run labor-intensive operations requiring little in the way of skilled technical personnel, foreign or Taiwanese. While the top managerial personnel tend to be foreigners, "local hires" of technical and middle-management people have become an increasingly attractive way of cutting personnel costs at little expense in terms of operating efficiency. On the supply side, as Taiwan has moved into an important position in world trade, local employees have grown increasingly familiar with the operating procedures of capitalist multinational firms. The growth of the export-processing zones does not seem to have stunted the burgeoning of Taiwan's new middle class.

We have considered five of the major arguments that lead dependency theorists to claim that reliance on foreign economic interests leads to economic inequality. Again, none of them fits the Taiwan case, and the reasons are complex. Factors such as the well-developed rural infrastructure, a high rate of literacy, labor-intensive development in firms without strong advantages of scale, the absence of powerful traditional elites dominating the government, and Taiwan's extraordinarily rapid rate of growth all helped to guarantee that income distribution would improve over time, regardless of the importance of foreign economic ties.

instead of reducing it (see Little, Scitovsky, and Scott 1970; Adelman and Robinson 1978). The fact that Taiwan did not suffer from increased inequality in this phase has been attributed to the fact that the import-substitution policies pursued there in the 1950s were relatively "mild" or moderate (Fei et al. 1979).

¹⁸ However, some dependency studies argue something like the opposite, that dependency leads to premature ballooning of the service sector and other middle-range occupations (see Evans and Timberlake 1980).

INTERPRETATIONS AND CONCLUSIONS

Our analysis suggests that there are understandable reasons why dependency has not led to slow growth or inequality in Taiwan, since none of the mechanisms described by the theory has operated there as the theory predicts. One case of this sort may not disprove the theory, but at the very least it forces us to question the degree of determinism implied in discussions of dependency. Sometimes dependence on foreign economic interests may be associated with harmful trends in the growth rate and income distribution in a country. But it is now apparent that there are also circumstances in which the effects may be neutral or beneficial. A variety of such circumstances have been uncovered in discussing the Taiwan case, but a fuller specification would require us to examine the experience of other countries. For the moment we simply argue that dependency need not ineluctably bring a full array of "malevolent mechanisms" in its wake. This conclusion implies that countries wishing to pursue growth with equity need not necessarily take the drastic steps, recommended by some writers, of nationalizing foreign firms and retreating into an autarkic shell (Zeylstra 1975; Chirot 1977; Moulder 1977).¹⁰

Several other implications for dependency theory arise out of our consideration of the Taiwan case. First, not all dependency theorists distinguish among various forms of dependency. They should do so, since the consequences of different forms will not all be alike. Foreign investment, foreign aid, and foreign trade all have different implications, and it may have been important in Taiwan's case to have had an interlude of heavy aid with minimal foreign investment (and to have received aid mostly in the form of grants, rather than loans, although the conventional wisdom of recent years is that loans are better because they encourage responsibility and savings). Furthermore, within foreign investment it seems important to distinguish among investment in agriculture, industry, commerce, or other branches of the economy, and within industry it is important to distinguish among such things as extractive industries, light industries, and heavy industry. For manufacturing, furthermore, it is important whether the products are meant primarily for domestic sale or for export. The particular forms of foreign economic penetration that develop are likely to have different consequences for capital formation, class relations, and other

¹⁰ Here it is interesting to note recent trends in the People's Republic of China across the Taiwan Straits, which has often been praised by dependency theorists as an example of what can be achieved by autarkic development (see Moulder 1977; Zeylstra 1975). Distressed about an insufficiently rapid rate of economic growth, China's leaders have recently begun a vigorous effort to attract foreign investment and have even set up special export-processing zones for this purpose that clearly seem modeled after those in Taiwan.

concerns. The forms that developed in Taiwan seem to have had consequences that are the opposite of those predicted by dependency theory.

Second, it is important not to assume automatically that either foreign or domestic governments are mere pawns serving the interests of economic elites or multinational corporations. In Taiwan after the war the governments of the Republic of China and of the United States both pursued policies that were not simply designed to serve their respective bourgeoisies. The presence in Taiwan of a strong state bureaucracy able to maintain separation from, and domination over, powerful economic interests seems to have been a major determinant of the form development took on that island. Third, we argue that both profits and power need to be thought about in much more complex ways than are usual among dependency theorists, who tend to see them in terms of a zero-sum competition in which foreign interests always win out. For instance, contrary to the claims of Frank (1969) and others, in Taiwan's case it was precisely during the 1960s, when interests in the "core" were experiencing unprecedented strength and expansion, that successes in economic growth and penetration of markets in the core countries were thrusting Taiwan from peripheral up to semi-peripheral status. Fourth, our study reaffirms the view that maximizing economic growth rates does not necessarily require concentrating on large, capital intensive industry (Lipton 1977). Finally, the Taiwan case calls our attention to the importance of labor-market phenomena in affecting the income distribution in a country and leads us to question the importance of direct governmental redistributive policies and trade union pressures in modifying inequality.

Implications of our case study for other poor countries can also be considered. Obviously the mix of factors responsible for Taiwan's relative success is unique to that country. And some of the factors that were important are obviously not ones that could or should be copied. For instance, it does not make much sense to suggest that poor countries today arrange to spend 50 years under Japanese colonial rule or open their doors to a massive influx of losers of a civil war. Nevertheless, certain aspects of Taiwan's experience seem relevant to some poor countries elsewhere. There are enough other places elsewhere in Asia (South Korea, Hong Kong, Singapore, perhaps Malaysia, and even in some ways Japan in an earlier period) whose experience is sufficiently similar to Taiwan's to suggest that there are generalizable lessons here.

The Taiwan experience does imply that both growth and equity are aided when a substantial period of agricultural development precedes the drive toward industrialization (see also Lipton 1977). During this phase substantial investments have to be made in the rural infrastructure—in schools, roads, electrification, credit facilities, and so forth—if later industrial growth is to provide broadly shared benefits. As industrialization proceeds

it seems clear that measures have to be taken, by the government or other agencies, to ensure that both domestic and foreign investments fit into the prevailing factor endowments and infrastructure. And a stress on decentralized, labor-intensive firms without important economies of scale seems important for achieving growth with equity. However, without some success over time in shifting industrial growth in order to penetrate export markets it is doubtful that favorable trends in both growth and income distribution can be maintained.

Clearly these lessons are not all applicable elsewhere, particularly to large, continental countries or to resource-rich, labor-poor countries. Even in relatively small, resource-poor, overpopulated places, a heritage of economic development taking other forms may be quite difficult to change, and change may be obstructed by domestic and foreign interests with a stake in the existing arrangements. So in the end the applicability of the Taiwan case remains uncertain, but at the very least the case proves that dependency does not inevitably and uniformly lead to economic stagnation and rising inequality. Indeed, it may provide clear benefits.

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Environmental Selection and Physician Supply¹

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A new conceptual framework for explaining physician distribution and forecasting regional physician supply is presented. Unlike existing efforts at explaining physician distribution, which rely on models of individual physicians making choices regarding location of practice (e.g., economic and psychological models), this paper develops an environmental selection model which views populations of physicians as the unit of analysis and identifies the environmental conditions under which these populations grow and decline. Nine dimensions of the environment affecting populations of physicians are described and hypotheses regarding the effect of four dimensions—residential population size, density, growth, and health system development—are posited. The model is dynamic; it allows predictions of change in the size of specialist and general practitioner populations over time. The model is empirically tested by integrating the appropriate differential equation and estimating the resulting linear model. Data are pooled cross-sectional and time-series data for counties in New York state. A modified generalized least-squares routine is used to estimate the coefficients. Results reveal substantial support for fundamental aspects of the model: a carrying capacity varying with density of resources, a finite response rate to deviations from carrying capacity, and differential selection of specialists and generalists. Future work should generate simultaneous differential equations estimating the extent to which generalists and specialists compete or cooperate in their efforts to provide services to residential populations.

The purpose of this paper is to propose a new conceptual framework for explaining the great variation observed in the geographic distribution and specialty mix of physicians, and to demonstrate how this new approach can be used to develop a dynamic model of physician supply at the regional level.

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It is well known that the ratio of physicians to population varies a great deal across geographic regions. With physicians located disproportionately in the northeast and western regions of the United States, the ratios are significantly higher in those areas than in the south and central states. In 1976, the District of Columbia had the highest ratio of non-federal patient care physicians to population in the nation (359 per 100,000 persons). States with more than 170 nonfederal patient care physicians per 100,000 persons are New York (198), Massachusetts (186), Connecticut (178), and California (172). States with fewer than 90 patient care physicians per 100,000 persons are Alabama (89), Arkansas (88), Mississippi (82), Alaska (79), and South Dakota (78) (Roback 1977).

Similar distribution patterns may be found within states. For example, in 1976, physicians per 100,000 population ratios for counties in New York ranged from a high of 350 in New York County to a low of 35 in Lewis County.

One important characteristic of counties that is often used to account for differences in physician-to-population ratios is population size or, more specifically, population density. It is well known that the number of physicians per capita tends to be higher in metropolitan than in rural counties. Upon closer inspection, however, the relationship between physician-to-population ratios and population density is more complicated. In particular, population density has opposing relationships with general practitioners and specialists. National data for 1975, displayed in table 1, show the proportion of specialists increasing as a region becomes more metropolitan, whereas for physicians in general practice the reverse is true (although the trend is nonlinear). These relationships are especially important because the health care system serving a given region is greatly influenced by the "mix" of specialists and general practitioners in the system. The simplest way of describing the mix is to indicate the proportion of specialists to general practitioners.

TABLE 1
PHYSICIAN-TO-POPULATION RATIOS BY TYPE OF PRACTICE AND TYPE OF AREA*
(United States, 1975)

Demographic County Classification	General Practice†	Specialized Practice
Counties in SMSAs with 5,000,000 or more residents.....	23	109
Counties in SMSAs with 500,000-999,999 residents.....	18	84
Counties considered potential SMSAs.....	21	72
Nonmetropolitan counties with 25,000-49,999 residents.....	26	40
Nonmetropolitan counties with 0-9,999 residents.....	30	9

* Includes only physicians in an active, office-based practice. Excludes physicians working for the federal government and osteopathic physicians. Ratios are physicians per 100,000 population.

† Includes family practice and general practice.

SPECIALTY INTENSITY

In any given geographic area, such as a county, the relative proportion of specialists to general practitioners is an important measure describing the health care delivery system. Access to physicians, availability of secondary and tertiary care, amount of capital investment in the health system, and the cost of the regional health care delivery system are all linked to the "specialty intensity" of the region. Specialty intensity (SI) is calculated as:

$$SI = \frac{N \text{ specialists}}{N \text{ general practitioners}},$$

with specialists of all types aggregated. The relationship between specialty intensity and county population density for the state of New York is presented in table 2. This table shows that density is positively related to specialty intensity.

Most demographic analyses go no further than to demonstrate such differences between urban and sparsely populated counties. Such analyses, however, are static and do not consider the effect of changes over time in the demographic structure of the community on the supply of physicians. In particular, they assume that the relationship between density and specialty intensity is the same for growing and declining communities. Figure 1, however, reveals substantial interaction effects for New York state among density, population growth, and specialty intensity. It is important to note that medium- and high-density declining counties have a higher specialty intensity than medium- and high-density growing counties, whereas the opposite is true in low-density counties. Thus, the question of what is (or will be) the available supply of specialists and general practitioners in a given region is difficult to answer without an understanding of how these different types of physicians respond to key characteristics of communities such as population density and rate of growth.

It is surprising how little attention the forecasting of regional physician supply has received to date. Estimating the regional supply of medical

TABLE 2
SPECIALTY INTENSITY BY COUNTY POPULATION
DENSITY (New York State, 1975)

Density of County	<i>N</i> (Total = 62)	Specialty Intensity (Average = 3.66)
<150/sq. mile.	38	2.37
150-300/sq. mile.	7	4.61
301-1,000/sq. mile.	6	5.79
>1,000/sq. mile.	11	6.33

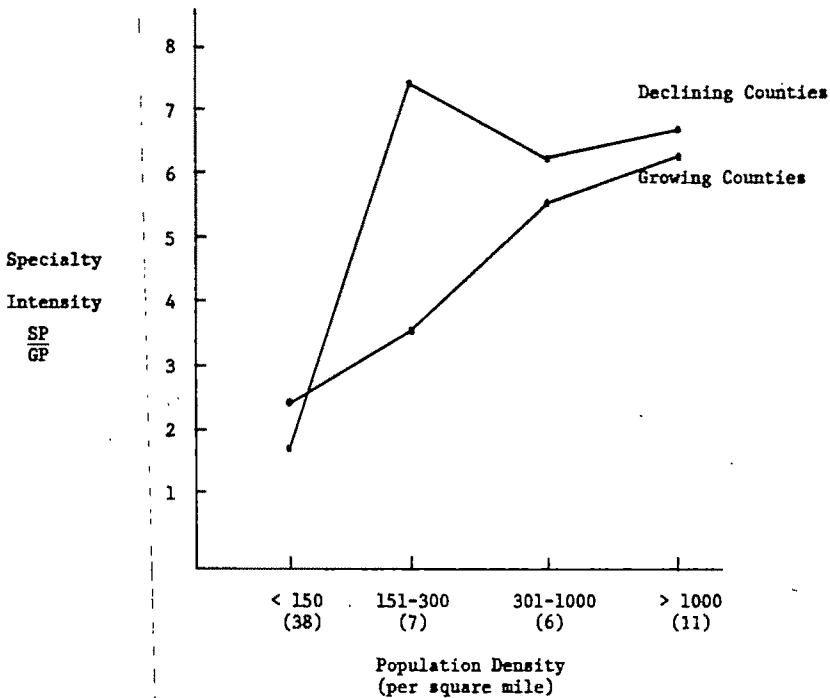


FIG. 1.—Specialty intensity in New York counties by population density for growing and declining county populations. Number of counties is in parentheses. Data are for 1975.

manpower at some future time is of great practical importance to health policymakers for several reasons. First, it is a necessary step in determining the extent to which future demand for health services will be adequately matched by the supply of health care providers. Second, it is widely recognized that the health care system is labor intensive. Therefore, estimates of the future costs of the system, and the monetary impact of various changes in the system, are heavily dependent on accurate estimates of manpower supply. Third, to the extent that the model behind the forecasts improves one's understanding of how populations of physicians respond to forces in their environment, new insights are developed as to how to affect the supply and distribution of physicians by geographic location and type (e.g., specialists vs. general practitioners).

PREVIOUS ATTEMPTS AT EXPLANATION: THE INDIVIDUAL CHOICE MODEL

The perspective often used to explain the maldistribution of physicians by area and specialty is one of individual choice. The individual choice model is really an orienting strategy which encompasses economic, social, and

psychological perspectives of practice location and specialty choice. Elements common to each approach include a focus on the physician as the unit of analysis and a strategy of evaluating the importance of various factors which affect his or her decision making.

McFarland's (1972) review of the literature identified five major categories of variables suggested by various researchers as important in physicians' choices of geographic location: (1) environmental factors such as cultural opportunities, climate, recreational facilities, and quality of the educational system; (2) prior exposure to the community through early residence or as a medical student, resident, or intern; (3) professional relationships through group practice, hospitals, continuing education programs, etc.; (4) economic factors such as potential income, costs of practice and of living; and (5) demand determinants such as population size, age, sex and racial composition, and education and degree of urbanization of community residents. The basic argument is that each physician uses one or more of these variables in a calculus as yet poorly understood, resulting in the selection of a preferred location of his or her practice.

Most researchers have focused on only a small subset of McFarland's variables at a time. For example, Rimlinger and Steele (1963) point to economic factors that motivate physicians. Lieberman (1958) and Elesh and Schollaert (1972) view the spatial distribution of physicians as a result of their attempts to establish a practice where clients will have status characteristics similar to their own. Research by Diehl (1951) and by Weiskotten et al. (1960) identifies the size of the physician's hometown community as a prime determinant of location preference. Still others have shown that physicians often locate near medical schools or close to institutions where they carried out residencies and internships (Bible 1970; Weiskotten et al. 1960). Perhaps the most comprehensive set of variables examined is found in the Rand-American Medical Association Survey (Cooper et al. 1975). It is typically argued that general practitioners and specialists attach different weights to the various decision factors (Cullison, Reid, and Colwil 1976). Hence, the goal of research within the individual choice framework is to determine the weights empirically by examining physicians' location decisions. For a discussion of the policy recommendations generated with the individual choice framework, see Scheffler et al. (1978).

Several limitations of individual choice models restrict their usefulness for theory building and for health policy development. First, although associations between certain personal or community characteristics and physician location decisions may be identified, they do not tell us much about the underlying causal process at work. For example, it has been found that general practitioners view a rural practice more favorably than do specialists. One common observation with regard to this finding is that general practitioners do not want to compete with specialists for patients,

but that if general practitioners could find an urban environment with a sufficient practice load, they too would opt for the city. An alternative interpretation, with some data to support it, is that individuals who have a personal preference for a rural practice select general practice in order to have such an opportunity. In order to sort out such issues, prospective longitudinal studies of practice decisions, following individuals from their entrance to medical school to actual practice, must be completed. Although some attempt at this is being made, the vast majority of research on individual choice models has not been of this type.

Second, at the regional level it is not clear how to use individual choice factors to predict future supply of medical manpower. One may be able to anticipate, for example, that a certain proportion of specialists will remain in the state where they have completed residencies, but we cannot say with any degree of confidence how many will set up practice in a given county.

PREVIOUS ATTEMPTS AT FORECASTING: TREND ANALYSIS

Traditionally, health care planners have focused on the problem of estimating the future supply of health manpower at the national level. To do this one measures the inflows and outflows from the current supply. The essential data are: (1) baseline data on the supply of health personnel, defined as the employed and the unemployed seeking work; (2) data on entrants as a result of the addition of new graduates, occupational and geographic mobility, and reentry of inactive professionals into the labor force; and (3) data on occupational losses reflecting deaths, retirement, and occupational and geographic transfers (Department of Health, Education, and Welfare 1976; Glasgow 1974).

Ideally, each of the three components of future supply is estimated separately. The estimates are aggregated to obtain a supply projection. However, this approach is practical only at the national level, where reasonable estimates of the in- and out-migration of physicians are available. At the regional level the estimates are likely to be unreliable: the absence of any system for monitoring interstate movements of physicians leads to faulty projections of supply. In the regional case, one has two alternatives: (1) to make assumptions about the size of in- and outflows; or (2) to adopt a methodology, such as trend analysis, which provides an estimate of the future manpower supply without requiring that each element be measurable. At the regional level the second of these options, trend analysis, is often used (Sorkin 1977).

In general, this method of estimating future manpower supply assumes that differences between one year's supply and the next represent the net effect of all the forces operating to increase and to reduce the supply stock (e.g., number of new licentiates, number of providers lost because of deaths

or out-migration, population size, attractiveness of the region, etc.). The net change in supply may be determined by a trend analysis of total (regional) supply over time. By far the most common method of estimating the future supply of physician manpower at the regional level is the physician-to-population ratio technique (Sorkin 1977). This forecasting method is of the form:

$$MD_t = POP_t R_{t-1}, \quad (1)$$

where MD_t = available manpower supply at some future time t , POP_t = estimated population size of the region at t , and R_{t-1} = physician-to-population ratio at the current time $t - 1$ (MD_{t-1}/POP_{t-1}). This model asserts that the physician-to-population ratio will remain constant for a region as the population changes. Physician forecasts are therefore based on a population forecast, obtained through well-established demographic techniques. The same formula is used to estimate the appropriate number of physicians for a region, by substituting a standard (i.e., normatively defined) value for R_t (see Department of Health, Education, and Welfare 1978).

Four important assumptions are implicit in this approach. First, it is assumed that rate of population growth has no independent effect on the supply of physicians: the numerator and denominator of R (the physician-to-population ratio) are assumed to react similarly in periods of population growth and decline. That is, physicians will continue to enter (or leave) the population at a rate roughly equal to the rate of increase (or decline) in the population. Second, it is assumed that population density per se is unrelated to the supply of physicians. Third, use of R_{t-1} implies that the physician-to-population ratio tends to remain constant, regardless of how out of balance it may be with respect to demand, regional averages, or some planning norm. Fourth, it is assumed that the components of the overall physician-to-population ratio, the specialist and general practitioner ratios, will not change as a function of population growth or decline.

In fact, the data summarized previously in tables 1 and 2 and in figure 1 seem to contradict each of these assumptions. Effects of growth and decline are suggested in figure 1. Density effects are apparent in all three summaries. Finally, projecting from these cross-sectional findings one would anticipate that R would change over time.

In the next section, we develop theoretical arguments that make predictions consistent with these cross-sectional data. They also predict longitudinal effects which have not been studied previously.

A NEW APPROACH TO EXPLANATION AND FORECASTING: ENVIRONMENTAL SELECTION OF PHYSICIAN POPULATIONS

An alternative approach to the individual choice models is to consider the population of a given type of physician (such as a general practitioner

or a specialist) as the unit of analysis and examine the environmental conditions under which one might expect that population to increase or decrease.

The environmental selection perspective developed here has its roots in the work of the biologists Darwin and Wallace. One of the first social scientists to apply ecological principles to the study of communities was Herbert Spencer. Unfortunately, much of his work was misinterpreted and misused by some who wished to argue that certain forms of society are naturally superior. Social Darwinism, as the movement came to be called, fell into disrepute until later scholars, most notably Park and Burgess at the University of Chicago during the first half of this century, returned the field of human ecology to the study of factors that influence the location, size, and physical organization of the community. By the time of the publication of *Human Ecology* in 1950 by Amos Hawley, the field was considered a major branch of American sociology.

Explicit environmental selection models, focusing on the growth and decline of populations of species, are relatively new developments in sociology. However, since the publication of Wilson's (1975) seminal book, *Sociobiology*, such models are being introduced increasingly in the social sciences. Hirshleifer (1977), for example, has argued that "the social sciences can fruitfully be regarded as the sociobiology of the human species" (p. 51). Hirshleifer's major contribution to this growing field is to trace the parallel analytical structures employed in economics and sociobiology. In a more limited vein, sociologists (Aldrich 1979; Hannan and Freeman 1977) have developed an environmental selection approach to the study of organizations. The following discussion will describe the general process of environmental selection and show how this process may be applied to the problem of physician supply at the regional level. For many of the concepts in this section we are indebted to the work of Marden (1966), Hannan and Freeman (1977), Aldrich and Pfeffer (1976), and Aldrich (1979).

The General Process of Environmental Selection

Environmental selection, as a general process, has three stages (Campbell 1969). The first is the occurrence of variations in organisms. Variations may occur for any reason. Biological variation occurs through genetic mutation within species. Variation in health manpower may involve formal recognition of new specialties or the emergence (or reemergence) of different forms for providing health care such as the use of physicians' assistants, nurse practitioners, and midwives. The second stage is the differential selection of some variations over others by the environment. The biological example is the differential survival of certain mutant forms that are better able to exploit the food supply. The type of physician best suited for a

locality depends on several environmental characteristics, described below. Location decisions of physicians, and to some extent decisions to retire early or to change one's specialty, are the analog of survival. The third stage is the operation of a retention mechanism; the positively selected variations are retained (preserved, duplicated, or reproduced) in the environment. With regard to the biological paradigm, positively selected variations survive and reproduce. In applying this concept to health manpower, we would expect positively selected forms of practice to be retained in the environment and to be reproduced and duplicated in the sense that new members of the "species" will enter the environment from the educational and cultural systems.

A key to application of environmental selection is to make some assumption about the permanence (or lack thereof) of the observed situation. When environmental elements that support a population undergo change, an imbalance occurs and changes in the population occur over time until an equilibrium is once again achieved. Thus it is necessary to discuss and measure not only the level achieved in equilibrium but also the rate of transient response. In the discussion that follows, the term "carrying capacity" is an equilibrium concept—the size of the population of physicians that a region would eventually support, given enough time with no changes in the environment. However, in our study, the environment is steadily changing, so that carrying capacity is not directly observable. We therefore investigate a *rate of response* to disequilibrium. (These key terms will be defined more fully below.) Simultaneous observation of many areas over time allows us to estimate both carrying capacity (equilibrium) and rate of response (transient) parameters.

Applying the three-stage environmental selection process to the distribution of health manpower requires us to identify the important dimensions of physicians' environment which may act as selective forces in stage two. Nine such dimensions are distinguished and briefly discussed below. The first six are adapted from Aldrich's (1979) work on environments of organizations. We have identified three additional dimensions.

1. *Quantity of natural resources* (the level of resources available to a population of physicians within its environment). Resources consist of the patient population (with the ability to pay) as well as physical necessities such as office space and hospital facilities. This dimension of the environment has fundamental importance for physicians since the greater the quantity of resources the larger the physician population the environment can support (carrying capacity).

2. *Environmental stability/instability* (the degree of turnover of elements in the environment). Environmental instability may be observed, for example, in regions with a high rate of in- and out-migration of residents. Such patterns of turnover are common in communities experiencing socioeconomic

transition. High rates of turnover in the residential population make it much more difficult for physicians to establish a set of regular clients. With environmental stability, on the other hand, physicians may develop on-going relationships with reliable clients.

3. *Environmental homogeneity/heterogeneity* (the degree of similarity or differentiation between elements of the environment). For example, physicians practicing in a retirement community are presented with a much more homogeneous set of clients than physicians in a typical residential area where the entire range of clients—including pregnant women, infants, adolescents, young adults, and the elderly—will be found.

4. *Environmental concentration/dispersion* (the degree to which resources contained in a geographic area are evenly distributed as against being concentrated in particular locations). For example, concentrated populations allow physicians easier access to clients (and vice versa), and therefore will support physicians to a greater extent than regions with dispersed populations.

5. *Domain consensus/dissensus* (the degree to which a population's claim to a specific domain—a type of diagnostic test, surgery, or other treatment, for example—is disputed or accepted by other populations). Domain dissensus suggests competition among health care providers for patients, while domain consensus implies a pattern of cooperation and referral of patients among providers.

6. *Environmental turbulence* (the extent to which environmental change is complicated by interconnections between the elements). Organisms that depend on highly structured environmental elements will be subject to a turbulent aftermath of environmental changes following a change in one element. This certainly describes the health care system. For example, a change in an employee insurance package by one company can have a lingering effect as other employers respond. Turbulence may obscure the actual structure of the health care system and the relations of clients to the system, thus making it more difficult for physicians to do their work.

In applying environmental selection to health manpower, we have identified three additional dimensions of physicians' environments: environmental growth, environmental density, and system development.

7. *Environmental growth/decline* (the rate of growth or decline of resources in the environment upon which physicians depend for survival). The most obvious example, and the one used in this analysis, is the rate of growth of the residential population. Environmental growth affects the distribution of physicians by creating an expectation for future carrying capacity. It is hypothesized that optimism about the future will increase the inflow of physicians. Conversely, population decline often engenders a pessimistic attitude which may accelerate departures. Thus, unlike the other charac-

teristics, the effects of environmental growth and decline are due to the tendency or ability of a species to forecast by extrapolating a trend.

8. *Environmental density*: (the extent to which resources are spatially close together or dispersed). This is related to concentration of resources. Whereas the latter refers to lack of uniformity of distribution over an area, density refers to an average over the area. Thus, environmental density is the interregional counterpart of environmental concentration.

9. *System development* (the extent of development of supportive resources in the environment). Supportive resources represent a modification of the environment to improve a species's capability to obtain and utilize natural resources. For physicians, they include facilities, laboratory services, referral patterns, etc. System development is particularly important to the rate of response to environmental change, since organisms that depend on it may require more time to adjust.

So far we have argued that environmental selection is a general process which has three stages: variation, selection, and retention. We have identified nine characteristics of physicians' environments which may contribute to selecting for or against physician populations. In this paper we focus on four of these dimensions: quantity of natural resources, growth, density, and system development. The reasons for selecting these dimensions are twofold. First, the data on these dimensions are readily available to health planners and policymakers; thus a model of physician supply and distribution using these dimensions will be more useful to planners than a model using dimensions of the environment for which data are not available. Second, there are well-developed arguments in the ecological and organizational literatures linking these dimensions to the selection of different species of organisms and organizations. When the dimensions are appropriately modified and applied to the problem of physician distribution, we will be able to derive hypotheses about the effect of these environmental characteristics not only on the overall supply of physicians but also, as the following section argues, on the differential selection of specialists and general practitioners.

Environmental Selection of Specialists and Generalists

A key concept in the environmental selection model of physician distribution is the idea of "fitness." That is, a given type of medical practice (e.g., general or specialty practice) will persist and thrive in certain environments and wither under certain other conditions.

In biological applications of this concept, it is argued that adaptation and selection are both responses of species to the driving force of survival through fitness. For the purposes of this paper we will follow the work of Hannan and Freeman (1977) and focus on the idea of environmental selection of forms of medical practice, setting aside, for the moment, the

fact that these forms (e.g., medical specialties) may evolve to meet environmental demands.

One of the basic propositions of our model is that physicians who have specialized will thrive in a plentiful environment (high quantity of natural resources, environmental growth or density) but will not survive as well as generalists in an environment where the level of natural resources is low, declining, or sparse. This is so because specialists exhibit efficiency in abundant environments—they exploit a narrow range of the environmental spectrum (i.e., see patients with only certain types of disorders for which they have specialized training). In a lean environment, however, the ability of generalists to draw on resources from a wide range of the environment gives them a selective advantage over specialists (general practitioners may see patients with a wide variety of disorders).

The importance of the system development dimension of the environment derives from the fact that certain resources (especially capital equipment) in a community are severely constrained. For example, a hospital cannot be moved or laboratory services developed easily. The availability of medical colleagues for referral, partnerships, and consultation is, in the short run, limited to what is currently available, and time is required to establish these relationships. Thus, to the extent that physicians are dependent on these community resources, they will be reluctant to move to areas which do not have them, and will be encouraged to move to (or stay in) areas where they exist.

Specialists, to a much greater extent than general practitioners, are constrained by needing hospital, laboratory, consultation, and other services; therefore, in the short run they are less free than general practitioners to respond to changes in carrying capacity. In a sense, specialists are anchored to an area by their need for support services. If the community experiences a negative rate of growth, the special relationship between the specialists and the "system" will provide a buffer in the form of referrals, consulting, administration, research, and other activities which mediate the effect of population decline. On the other hand, specialists may find it difficult to move into a growing region to exploit the new abundance of resources until a certain amount of development of the health care system has been completed. Hence, the mobility of specialists, and therefore their ability to respond to deviations from their carrying capacity, is more limited than that of general practitioners.

Another way to state this effect is that the carrying capacity is measured, in part, by system development, and that specialists may (or may not) respond to a change in population by additional system development. This implies that some independent measure of system development must be included in a description of the environment. This possibility (suggested by a referee) remains for future research.

Hypotheses

The hypotheses we derive from the environmental selection perspective of physician supply are divided into two groups: (1) hypotheses applicable to all types of physicians and concerning both a region's carrying capacity and physicians' rate of change as a response to disequilibrium; and (2) hypotheses concerning the differential impact of environmental change on specialists and generalists. This section states these hypotheses formally and indicates how they are derived from our environmental selection arguments. Symbols for variables are included in parentheses to enable the reader to see clearly the relationships between our verbal formulations and the mathematical specification in the following section.

Throughout this paper we have assumed that clients are the primary natural resources upon which physicians practicing patient care depend for survival. It was argued above that quantity of natural resources (environmental dimension number 1) is positively related to carrying capacity. Since the size of the potential client population is directly related to the size of a region's residential population, we hypothesize:

- H1.1: The physician carrying capacity (K) of a region increases with the size of its residential population (P).

For a given region, an increase in residential population also means an increase in population density (environmental dimension number 8). As argued above, density often has an effect on carrying capacity; cross-sectional evidence in table 1 supports this argument. Hence, we hypothesize:

- H1.2: The effect of the residential population on carrying capacity depends on population density (D).

(This is made more explicit in H2.1.) We have also argued that population growth (environmental dimension number 7) has an effect on a region's carrying capacity. Growth may contribute to the perceived richness of an environment at any time and establishes expectations among populations of physicians for the future carrying capacity of the region. In a sense, current carrying capacity is "artificially" inflated or deflated by growth as physicians respond rationally to their expectations for the future. This hypothesis is stated as follows:

- H1.3: A geographic area with increasing population will support more physicians, *ceteris paribus*, than an area with no population growth. Similarly, population decline decreases carrying capacity.

Although we do not have any strong theoretical arguments regarding the relative strengths of the effects of growth and decline, it seems reasonable

to assume they are qualitatively different types of population change. In particular, population decline may create a "panic" climate among physicians which would not be created by population growth. We also have cross-sectional evidence displayed in figure 1 consistent with the notion that the effects of growth and decline may not be symmetrical. We therefore hypothesize:

H1.4: Population growth and decline may not have equal and opposite effects on carrying capacity.

Carrying capacity is an equilibrium concept. However, data are collected in environments undergoing change, and the rate of change of physicians is limited. The carrying capacity must therefore be viewed as an ideal, toward which the number of physicians moves.

H1.5: The number of physicians (M) changes through time in the direction of the carrying capacity.

Deviation from the carrying capacity is a measure of environmental pressure for change. There are a number of methods to translate this observation into a hypothesis, and the simplest is:

H1.6: The rate of change in the number of physicians is proportional to the deviation from carrying capacity ($M - K$).

We have argued above that species which exhibit characteristics of specialism and generalism are differentially selected by certain dimensions of the environment. It is important to the validity of the environmental selection perspective that we be able to observe instances of this selection process among types of physicians. As a first attempt at defining different populations of physicians we have elected to use a simple typology common among health systems studies: specialists and general practitioners. No doubt refinements of this typology, classifying specialists into various subtypes that may compete with each other for resources, are possible. But for our initial efforts to observe selective forces empirically we believe the dichotomy to be adequate.

The environmental selection argument posits that environmental richness favors specialists in the long run owing to superior efficiency in utilizing resources. In the hypotheses above, population density and growth have been identified as measures of environmental richness. Thus, the following hypotheses are consistent with theory, as well as with the cross-sectional evidence of table 1 and figure 1:

H2.1: As a function of density of residential population, the carrying capacity decreases for generalists and increases for specialists.

H2.2: For specialists, in comparison with general practitioners, the carrying capacity is more strongly increased by population growth and more strongly decreased by population decline.

Our final hypothesis is derived from the system-development argument. We do not test the effect of health system development directly; instead we hypothesize that the mobility of specialists and generalists will be differentially affected by system development. That is, since specialists are constrained to a greater extent than general practitioners by the availability of support systems, they are less free to respond to deviations in carrying capacity. Hence, we will test indirectly the validity of our assumption about health system development with the following hypothesis.

H2.3: The response rate (c) of specialists to deviations from carrying capacity is lower (i.e., slower) than that of general practitioners.

Hypotheses H2.2 and H2.3 represent countervailing forces. Whereas H2.2 says that specialists have an advantage when a community grows, H2.3 says that it takes longer for them to avail themselves of that advantage. Thus, to test these hypotheses, we need a model that can distinguish between a change in carrying capacity and the rapidity with which physicians can respond to such a change.

SPECIFICATION OF AN ENVIRONMENTAL SELECTION MODEL

The following equation is used to test hypotheses about carrying capacity K as a function of residential population P , its density D , and its rate of change dP/dt :

$$K = \left(r - \frac{s}{D} \right) P + g \frac{dP}{dt}. \quad (2)$$

Hypothesis H1.1 asserts that carrying capacity K increases with residential population P ; this is supported (*ceteris paribus*) if $r > 0$. Hypothesis H1.2 suggests a density effect ($s \neq 0$), and H2.1 is supported if $s > 0$ for specialists and $s < 0$ for generalists. Because density (D) is in the denominator, the equilibrium physician-to-population ratio (K/P) is affected by density, but the effect disappears as density increases. This is in keeping with empirical evidence (e.g., table 1).

Similarly, the effects of population growth, stated as H1.3, H1.4, and H2.2, are tested through the parameter g , comparing its values across growing versus declining counties, and with respect to generalists versus specialists.

Thus the equilibrium of the system is described by equation (2). However, as indicated above, the carrying capacity cannot be observed directly. Its influence on the actual number of physicians is expressed through the

following relationship:

$$\frac{dM}{dt} = -c(M - K), \quad (3)$$

in which M and dM/dt represent the number and the time rate of change of physicians. In other words, the farther M deviates from the carrying capacity, the faster physicians move to rectify the imbalance. The parameter c is termed the response rate, since it measures the rate of inflow or outflow of physicians for each one-unit deviation of M from the carrying capacity. Hypothesis H1.5 implies $c > 0$. Hypothesis H1.6 implies that the linearity of equation (3) is correct. There are other ways to specify dM/dt , most notably as $-cM(M - K)$, but they are not pursued in this paper. Hence hypothesis H1.6 can be given only a weak test (Does eq. [3] seem to represent the data?) since no specific alternative is given. Hypothesis H2.3 implies that c is larger for generalists than specialists.

Unfortunately the parameters of this model cannot be estimated by direct methods, since the instantaneous growth rates cannot be observed. However, following the general approach of Coleman (1968), we can integrate equation (3) over time. First, we rewrite the equation by substituting (2) for K and eliminating population density by using the identity $D \equiv P/A$ (population divided by area). The result is

$$\frac{dM}{dt} = -cM + crP - csA + cg \frac{dP}{dt}, \quad (4)$$

which is a first-order linear differential equation in the variables M (physicians), P (resident population), and A (area of the region). The area of a given county is a constant. Moreover, if we observe the variables at suitably short intervals, it is reasonable to assume that dP/dt is constant between observations. Under these assumptions, the solution to (4) is

$$M_t = K_t - (r/c) \frac{dP}{dt} + e^{-c\Delta t} \left[M_{t'} - K_{t'} + (r/c) \frac{dP}{dt} \right], \quad (5)$$

in which the subscripts t and t' indicate any two points in time between which the preceding assumptions are valid, and $\Delta t = t - t'$.

One way to make sense of this equation is to consider the long run (that is, $\Delta t \rightarrow \infty$). Because of the negative exponential term, when $\Delta t \rightarrow \infty$ the bracketed term can be neglected. The remaining terms indicate that a gap of $(r/c)dP/dt$ will exist between the carrying capacity K_t and the actual number of physicians M_t . However, the faster the response rate (the larger the value of c) the smaller the gap.

In the short run, the bracketed term in (5) cannot be ignored. As Δt grows, the number of physicians moves away from its initial value $M_{t'}$ and decays exponentially toward the carrying capacity minus the lag discussed above.

Methodology for Testing Hypotheses

According to our previous assumption, dP/dt is constant over the interval Δt , so we can replace it by $(P_t - P_{t-1})/\Delta t$. After considerable manipulation, the result is a linear equation in the observable variables:

$$M_t = b_1 M_{t-1} + b_2 P_{t-1} + b_3 (P_t - P_{t-1}) + b_4 A. \quad (6)$$

The coefficients of (6) are related to the model parameters as follows:

$$b_1 = e^{-c\Delta t} \quad (7)$$

$$b_2 = r(1 - e^{-c\Delta t}) \quad (8)$$

$$b_3 = g(1 - e^{-c\Delta t})/\Delta t + r[1 - (1 - e^{-c\Delta t})/c\Delta t] \quad (9)$$

$$b_4 = -s(1 - e^{-c\Delta t}). \quad (10)$$

The b coefficients may be estimated by statistical methods. However, their relationships to the research hypotheses are complicated. Of the four basic parameters (r , s , g , and c), only c is uniquely determined by one b coefficient. Equation (7) implies

$$c = -(\ln b_1)/\Delta t. \quad (11)$$

Thus H1.5, supported if $c > 0$, may be tested by $b_1 < 1.0$, and H2.3 implies a smaller b_1 for generalists than for specialists.

The parameter r is a function of both b_1 and b_2 , as may be seen from (7) and (8):

$$r = b_2/(1 - b_1). \quad (12)$$

The properties of this estimator are unknown, but r and b_2 have the same sign whenever $b_1 < 1.0$ as hypothesized. Thus, H1.1, supported if $r > 0$, may be tested by $b_2 > 0$.

Similarly, for the parameter s ,

$$s = b_4/(1 - b_1); \quad (13)$$

that is, H1.2 is supported if $b_4 \neq 0$, and H2.1 predicts that $b_4 > 0$ for generalists and $b_4 < 0$ for specialists, again assuming that $b_1 < 1.0$.

The estimate of g involves three of the b coefficients:

$$g = \frac{\Delta t}{1 - b_1} \left[b_3 - b_2 \left(\frac{1}{1 - b_1} + \frac{1}{\ln b_1} \right) \right]. \quad (14)$$

This complicated expression makes it difficult to test hypotheses concerning g . However, for values of b_1 in the range of 0.5 to 0.999 (which includes all of the observed values in this study), the term in parentheses lies between 0.557 and 0.500. Thus, H1.3, $g > 0$, could be roughly approximated as " $b_3 > 0.6b_2$."

Hypothesis H1.4 requires a test of the g values for growing versus declin-

ing counties. These are estimated by using dummy variables. Hence, each state has a pair of b_3 values but only one value of b_1 , b_2 , and b_4 . Therefore the only difference between b_3 values within a state is due to differing g values for growing versus declining counties, and H1.3 can be expressed as b_3 (growing) \neq b_3 (declining).

Hypothesis H2.2 is that g is greater for specialists than for generalists. The statistical requirements of this hypothesis are awesome, owing to the complexity of equation (14). We shall look at each case to see whether a logical argument can be made, but no general rule can be given.

The hypothesis tests presented above are summarized in table 3.

Estimation and Hypothesis Testing

The data drawn on for our initial analyses of New York state are annual estimates of numbers of physicians in office-based patient care practice by type of specialty published by the American Medical Association for each county in the United States (Roback 1971, 1973, 1975, 1977), county geographical size (Rand McNally 1978), and annual census estimates (U.S. Bureau of the Census) also by county. There is some disagreement among researchers over the proper geographical units for analyses of the health system (see the exchange between Brooks and Anderson [Brooks 1978; Anderson 1979]). We have selected counties over SMSAs for two reasons. First, we are interested in the effect of population decline, and many states which have counties declining in population have very few SMSAs. Second, we prefer counties because they are the basic unit around which local medical associations are organized. We believe that information regarding regional growth, system development, etc., is transmitted through these county organizations. The time period covered, 1970-76, was selected because data on county population more recent than 1976 are not yet available and because the methods used for aggregating and reporting AMA data in the 1960s differ significantly from current methods. New York was selected for our initial tests of the model because it clearly satisfied two important conditions: the state tested must have a reasonably large number

TABLE 3

Hypotheses		Tests
H1.1	$r > 0$ (population is a resource)	$b_3 > 0$ (if $1 - b_1$ is positive)
H1.2	$s \neq 0$ (density effect)	$b_4 \neq 0$ (if $1 - b_1$ is positive)
H1.3	$g > 0$ (growth effect)	$b_3 > 0.6b_2$
H1.4	g (growth) \neq g (decline)	b_3 (growth) \neq b_3 (decline)
H1.5	$c > 0$ (response rate)	$b_1 < 1.0$
H2.1	s (specialists) > 0	b_4 (specialists) < 0 (if $1 - b_1$ is positive)
	s (generalists) < 0	b_4 (generalists) > 0 (if $1 - b_1$ is positive)
H2.2	g (specialists) $> g$ (generalists)	Ad hoc
H2.3	c (generalists) $> c$ (specialists)	b_1 (generalists) $< b_1$ (specialists)

of counties, and a significant proportion of those counties must have experienced population decline during the 1970-76 time period.

The data set consists of a time series of cross-sectional observations. This fact causes a number of estimation complexities, but one needs both time-series data (since the model explicitly includes time-dependent "motion" toward the carrying capacity) and cross-sectional data (to increase the number of observations and the amount of variation in dependent and independent variables).

Hannan and Young (1977) describe a series of simulation experiments which test a variety of estimation procedures for data sets such as this. The general conclusion is that a two-stage procedure termed Modified Generalized Least Squares (MGLS) is preferable to available alternatives.² In particular, their study "cast doubt on the many published panel studies which do not correct for autocorrelation" (Hannan and Young 1977, p. 80).

The first stage of this procedure estimates a factor ρ which measures the redundancy in the time series due to autocorrelation. The second stage uses this estimate of ρ to improve the estimates of the parameters of the linear operations. The superiority of this method is at least partly due to the relative insensitivity of the second-stage estimates to bias in ρ . Thus, the "ballpark" estimate of ρ from the first stage yields estimates very nearly as good as can be obtained when ρ is known. The second stage also reports standard errors for each parameter, but these can only be taken as approximate as they do not take into account the two-stage procedure.

After some preliminary work, several decisions were made: (1) Observations were weighted by dividing by population in order to eliminate heteroscedasticity in the residuals. (2) A time interval of $\Delta t = 2$ years between observations was chosen as a compromise between increased unreliability of $(P_t - P_{t-1})/\Delta t$ as an estimate of dP/dt for short time periods and increased unreliability of the assumption that dP/dt is actually constant if Δt is too long. This resulted in three "waves" of data (1970-72, 1972-74, and 1974-76). (3) A dummy variable δ was introduced to allow separate estimates of the effect of dP/dt in growing versus declining counties. However, the remaining parameters were assumed to apply statewide. The resulting equation submitted to MGLS estimation³ was:

$$M_t = b_1 M_{t-1} + b_2 P_{t-1} + b_{3g} \text{INC} + b_{3d} \text{DEC} + b_4 A, \quad (15)$$

in which $\text{INC} = \delta(P_t - P_{t-1})$, $\text{DEC} = (1 - \delta)(P_t - P_{t-1})$, and δ is 1.0 when $P_t > P_{t-1}$ and zero otherwise.

² We are indebted to Michael Hannan for allowing us to use the estimation package and to Glenn Carroll for helping us to sort through the many options that were programmed in for their experimental work.

³ The estimation package had to be modified slightly to allow estimation of a zero intercept model.

Results

The results of applying MGLS to data for New York are shown in table 4, with standard errors in parentheses. The hypothesis tests presented above will now be discussed.

Hypotheses H1.1 and H1.5 establish the nature of the model—if $r > 0$ and $c > 0$, then there is a carrying capacity to which physicians are drawn. This is confirmed for both specialists and generalists. As expected, generalists have a significantly higher response rate (lower b_1) than specialists (H2.3).

Hypothesis H1.2, the density effect, is confirmed only for specialists. Nevertheless, the favorable selection of specialists suggested by H2.1 is consistent with the data, since b_4 is positive for generalists (but insignificant) and significantly negative for specialists. Scatter plots (not presented here) of physician-to-population ratios versus density confirm the general shape posited by the model. Hence, we conclude that specialists are favorably selected as density increases, although the decrease in generalists (if any) is less than the increase in specialists.

The three remaining hypotheses concern the b_3 coefficients, the influence of growth and decline. Hypothesis H1.3 suggests that both growth and decline should have significant independent effects, but the data do not bear this out. Decline does significantly reduce carrying capacity, but the effect of growth is not significant.

The pattern of coefficients supports H1.4, since growth and decline clearly do not have equal weight. The data also lend weak support to H2.2 since specialists receive the greater impact from population decline. (In fact, using eq. [14] to solve for g for declining counties in New York, we obtain 6,789 for specialists and 568 for generalists.) On balance, we conclude that the coefficients for population growth-decline demonstrate effects which seem to be consistent with the environmental selection hypotheses.

SUMMARY AND CONCLUSIONS

Environmental selection has been shown to provide a new conceptual framework for studying the locational patterns of physicians through time. Hypotheses formulated from this point of view were tested by specifying a mathematical model typical of population dynamics, integrating it with respect to time, and estimating the resulting coefficients by using a regression technique specifically developed to handle a time series of cross-sectional data. Counties were the unit of analysis, and data were drawn from the state of New York.

Fundamental aspects of the model (a carrying capacity varying with density of resources, and a finite response rate to deviations from carrying

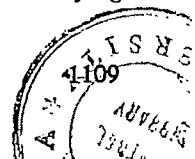


TABLE 4
ESTIMATED COEFFICIENTS FROM MODIFIED GENERALIZED LEAST-SQUARES (MGLS)
REGRESSION WITH $T = 3$ WAVES; 1970-72, 1972-74, 1974-76
(For 62 Counties in New York State)

TYPES OF PHYSICIANS	R^2	ESTIMATED EQUATIONS									
		$M_t = b_1$	M_{t-1}	b_2	P_{t-1}	b_3	b_4	INC†	b_5	DEC†	b_6
General practice.....	.979	$M_t = .865^* M_{t-1}$ (.035)	$+ 1.55^* P_{t-1}$ (.810)	$+ 8.71$ INC (10.9)	$+ 77.5^*$ DEC (26.1)	$+ .028$ (.029)					
Specialists.....	.995	$M_t = .981^* M_{t-1}$ (.011)	$+ 5.81^* P_{t-1}$ (1.26)	$- 3.69$ INC (19.7)	$+ 131.9^*$ DEC (45.9)	$- .114^*$ (.069)					

NOTE.—Standard deviations in parentheses.
* These are consistent with the hypotheses concerning the model parameters, at a 95% level of confidence.
† INC and DEC are population increase and decrease, respectively.

capacity) were supported. The theory also predicted certain differences between specialists and generalists. Most of these were also supported.

The eventual goal of such studies as this is to increase understanding of the causes of physician distribution patterns, and thereby to improve physician supply forecasting and planning. This study represents a first step in applying the concept of environmental analysis to this problem. The theory described here is rich in hypotheses that we have not tested. Many hypotheses can be developed, for example, by considering the list of nine environmental characteristics salient in the dynamics of physician populations. However, one must be careful to distinguish between long-run effects on carrying capacity and short-run responses to deviations therefrom. For example, the response rate of specialists was found to be very slow. We suspect that this slow response makes it difficult to distinguish between "slow movement toward carrying capacity" and "no movement because capacity has been reached." Thus, it is particularly important to collect data over a longer time interval and to continue to use the best available methods to distinguish time-series effects from cross-sectional ones.

A major next step will be to model the competition-cooperation aspects of generalists and specialists by specifying simultaneous differential equations in which the number of specialists influences the carrying capacity for generalists and conversely. This should produce a more accurate picture of the relationship between the two; do they support each other, do they compete, or does one exhibit unreciprocated dependence on the other?

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Race and Juvenile Justice Processing in Court and Police Agencies¹

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Studies of race bias in the juvenile justice system have yielded contradictory and inconclusive findings. The diversity of findings, though due in part to inadequacies in the methods used in previous studies, is also attributable in part to the differential possibilities for bias in different settings. This paper develops and tests hypotheses that specify two conditions which affect the likelihood of bias: the characteristics and procedural constraints of processing agencies and the characteristics of their social environments. Log-linear analysis is used, to allow simultaneous control for the influence of prior record, type of allegation, family type, sex, race, and county in analyzing data from police and court records in a populous eastern state. Consistent with the hypotheses, the findings indicate that racial bias is more apparent in police dispositions than in judicial decisions. In the more urban of the two social settings studied, minorities constitute a relatively high proportion of the population; police bias is especially pronounced there. In the same setting, however, this bias may be compensated for, to some extent, by the courts.

The issue of racial and other forms of bias in the processing of juvenile offenders continues to be a controversial question that is of significance to both practitioners and students of the criminal justice system (Chambliss 1973; Quinney 1970; Chesney-Lind 1977). The body of published research on the question of bias has grown steadily over the past decade,

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but the findings are contradictory and inconclusive. For example, Cohen and Kluegel (1978) and Chused (1973) found no evidence of bias in juvenile court processing, but Arnold (1971) did. Carter and Clelland (1979) found evidence of racial bias in court processing, but only for "moral" or victimless offenses. Terry (1967) found no bias in dispositions by either police or court agencies in a midwestern city of 100,000, while Thornberry (1973) concluded that bias operated in both kinds of agencies in the city of Philadelphia. Weiner and Willie (1971) found no evidence of bias in police processing in Syracuse, New York. Wilson (1968) compared two police departments, a traditional one and a modern "professionalized" one, and found bias in the former but not the latter. Ferdinand and Luchterhand (1970) found evidence of bias in processing by police, but not by the juvenile court, in an eastern city of 150,000.

Methodological problems may account for some of these conflicting findings. With the exception of Cohen and Kluegel (1978), all previous studies have relied on tabular analysis or statistical techniques that permit only one or two control variables at a time. In addition, Arnold's work has been criticized for relying on arbitrarily constructed quantitative indices, and Thornberry's conclusions have been called into question because of his reliance on percentage differences rather than measures of association (Wellford 1975).

Given the complexity of the decision-making processes and the diversity of the practice of juvenile justice across jurisdictions and from agency to agency within a given geographical area, it seems unwarranted to assume that all the differences in findings are due to such methodological problems. It seems unlikely that bias is present or absent uniformly over a wide range of social settings. This paper develops and tests hypotheses that attempt to specify some of the conditions under which bias may be expected. We suggest that the presence or extent of bias will vary with (1) characteristics of the social environment and (2) the type of agency studied.

THE SOCIAL ENVIRONMENT

If the application of the law by the police or the courts reflects the influence of social, nonlegal factors, outcomes should vary with the social features of environments. To our knowledge, only three empirical comparisons of juvenile justice processing across jurisdictions have been reported, however, and with varying results. Wilson's (1968) study found that the degree of race bias varies inversely with the professionalization of police, while neither Cohen and Kluegel's (1978) study of dispositions by courts with differing ideologies nor Chused's (1973) study of three New Jersey

counties found evidence of race bias in any setting. None of these studies directly measures or discusses variations in the social environments of the agencies which might operate to produce biased outcomes.

One feature of the social environment that is associated with the quality of race relations and with a number of indicators of bias is the proportion of the population that belongs to minority groups. As that proportion increases, majority group members' fears of economic competition and threats to their power are likely to increase, producing "motivation for discrimination" (Blalock 1967, p. 29; see also Allport 1954, pp. 227-29).² The results appear to include higher levels of discrimination (Cutright 1965; Parcel 1979; Spilerman and Miller 1976) and of racial hostility (Morgan and Clark 1973; Spilerman 1976). In addition, Vines's (1964) study of civil rights cases in southern courts found that as the proportion of blacks in a county's population increased the likelihood of favorable outcomes for members of minorities decreased.

Although it has not been tested directly, the expectation of a positive relation between bias in police decisions and a high percentage of minority residents is consistent with Groves and Rossi's (1971, p. 17) finding that white officers who patrolled black communities tended to view them as hostile, or, at best, indifferent. This tendency appeared to result from projection by police of their own fears of blacks "getting out of their place" (also see National Advisory Commission 1972, p. 141) as well as from a lack of knowledge of black residents. We would expect to find similar perceptions of blacks among white officers who patrol white communities adjacent to black ones. However, in communities with a low proportion of black residents, the bases for such perceptions, and hence their potential as sources of bias, would be reduced.

In a study of a national sample that focused on citizen attitudes, Feagin (1971) found systematic variation in attitudes toward the police that is consistent with our expectations. Although he did not characterize communities in terms of percentage black, he used a closely related variable, community size. For whites, Feagin (1971, p. 105) reported, willingness to rely on police for defense of their homes rose with community size, while among blacks, reliance on police was greatest in communities with populations between 2,500 and 100,000 (the size of communities in our setting with a low percentage of blacks). Among blacks in cities of more than 100,000 inhabitants (the size of the large city in our county with a high percentage of blacks), reliance on the police was much less common (Feagin 1971, p. 105).

² As Allport states (1954, p. 229), proportion minority is not in itself sufficient as an explanation of differential treatment. Lack of knowledge, prejudice, or other factors must contribute to or intervene in the causal path from ecological characteristics to individual behavior.

As applied to juvenile justice processing, Blalock's (1967) theory and the findings of the studies reviewed suggest the following hypothesis: The larger the proportion of minority group members in a population, the greater the likelihood of discrimination by official agencies against juvenile justice offenders who belong to minorities.

This hypothesis receives initial support in that it appears largely consistent with findings of the studies reported above. Discrimination was found in Philadelphia (Thornberry 1973), in Arnold's (1971) study of a southern city, and for some offenses in Carter and Clelland's study of a "metropolitan area in the southeastern U.S." On the other hand, no discrimination was found in Syracuse, New York (Weiner and Willie 1971); in Denver (Cohen and Kluegel 1978); in Montgomery County, Pennsylvania (Cohen 1975); or in Terry's (1967) midwestern city of 100,000.⁸ The only clear contradiction to this hypothesis in previous work is Cohen and Kluegel's (1978) finding of no evidence of discrimination in the Memphis-Shelby County, Tennessee, court.⁴

AGENCY DIFFERENCES IN THE OPPORTUNITY FOR BIAS

Another important kind of variability exists between agencies in the same geographical space. After juveniles are arrested, they may or may not be referred by the police to the juvenile court. The action of officials of the juvenile court is thus dependent on the decisions made by the police—both the arresting patrolman and the juvenile officer who ultimately decides whether the accused juvenile should be referred to court. Most studies (Cohen and Kluegel 1978; Arnold 1971; Chused 1973; Weiner and Willie 1971; Wilson 1968) have examined processing only at one stage or the other, not at both simultaneously. It is clear, however, that bias at earlier stages of processing would affect outcomes at later stages, even if no bias occurs at the later stages (Farrell and Swigert 1978).

There are strong theoretical reasons for suspecting that bias (in any direction) is more likely to occur at the police stage of processing than at the juvenile court. First of all, a relatively large amount of discretion is endemic to police work, and action must often be taken without ade-

⁸ According to U.S. census data, Philadelphia's population was 26.4% black in 1970. The juvenile population of Arnold's study is said to have been 12%–13% black, above the national average. We assume that Carter and Clelland's southeastern metropolitan area also has a substantial black population. In contrast, the percentage black in settings where no discrimination was found is as follows: Denver 9.1 in 1970; Syracuse 10.8 in 1970; Montgomery County, Pa., 3.6 in 1970. Presumably Terry's midwestern city of 100,000 was also well below the national average.

⁴ While our discussion of bias in the treatment of minority groups relies largely on studies that have compared blacks and whites, we assume that our expectations for bias against blacks will apply also to Hispanics.

quate knowledge of the relevant facts. Reiss states that "unlike the lawyer or judge, who may take a long time gathering information to make a diagnosis or reviewing the decisions that lead up to a fate decision, a line officer must often make a quick fate decision. This creates, in many ways, a paradoxical situation for the police. To be professional about the decision often means that more information and more time is required. However, to protect the interests of the client and the public, and to satisfy the interests of operating efficiency, a quick decision is required" (1971, p. 130).

Thus, the police officer must often rely on his or her own judgment in making the decision to arrest and then the decision to refer a case to court. When essential pieces of information about an alleged deviant act are missing, the officer must "fill them in" by constructing an informed conjecture based on experience and general background knowledge. Inevitably, such a process must rely on the perceptual and evaluative constraints of the officer's own biography, including typifications of some juveniles as more likely than others to be probably in need of control or assistance by the juvenile court. In contrast to the speed with which the officer must reach a "verdict," the nature of court decision making both permits and requires more careful deliberation before adjudicating and disposing of a case. Although the decision to refer a case to court can be made with more deliberation than the street arrest, each involves a relatively quick decision based on limited information. In contrast, the court can and must await information that should provide more objective criteria for decision making.

A second factor distinguishing police and court practice is the extent of formalization and bureaucratization of procedures. Especially since certain Supreme Court decisions of the past 15 years,⁵ which have had the effect of replacing the traditional *parens patriae* concept with formal due-process proceedings for accused juveniles, the court's discretion relative to that of police has been reduced. The court is now required by law to consider such factors as the establishment of guilt beyond a reasonable doubt and the presence of defense counsel. In addition, in a number of states the range of permissible dispositions for many offenses has been narrowed, and probable-cause hearings may be required. Some observers (e.g., Packer 1966) have suggested that one consequence of these reductions in judicial discretion would be the reduction of the possibility of bias in juvenile court processing. While standardization and formalization of police procedures has also been a policy issue, no comparable legal restrictions have been imposed on police processing, and some changes in procedures have been relatively minor and jurisdictionally confined. Indeed, as noted above, re-

⁵ The decisions referred to are as follows: *in re Gault*, 387 U.S. 1967; *Kent v. United States*, 383 U.S. 541, 1966; and *in re Winship*, 397 U.S. 538, 1970.

moval of a comparable amount of discretion from police decision making is largely precluded by the nature of the work itself.

Finally, and related to the degree of formalization of procedures, police decisions are less visible than court decisions (Reiss 1971, pp. 185-206; Goldstein 1960, pp. 543-94). Many police-citizen encounters that involve a police decision go unrecorded. This is often true in the station house as well as on the street. The police disposition is made without opportunity of appeal or formal adversarial representation. By definition, these are functions of the judicial stage of processing.

In sum, the police are legally and bureaucratically permitted, and functionally required, to exercise a wide range of discretion for which there is relatively little review or accountability, or opportunity for appeal on the part of the accused party. Responses obtained during semistructured interviews with police officers, police juvenile bureau directors, court intake officers, judges, and probation officers in each setting studied suggest that these characteristics are true of the sampled jurisdictions. Thus, our second hypothesis is that racial bias is more likely to occur in police than in juvenile court processing. This hypothesis is consistent with the findings of Ferdinand and Luchterhand (1970) that race had a stronger effect than demeanor in determining the outcome of police encounters but made no difference in court dispositions. Two other studies (Terry 1967; Thornberry 1973) also examined police and court processing jointly but found no differences in bias between them. In addition to the methodological limitations of the studies reviewed earlier, it should be noted that all three of the studies just mentioned utilized data from the early 1960s, before the impact of the Supreme Court decisions noted above. Although it compares only police departments, Wilson's (1968) study provides a comparison of discretion levels that supports our hypothesis: he reports that bias existed in a traditional department but not in a professionalized one that involved a higher level of bureaucratization and formalization.

THE DATA

The Setting

Data for the analysis of dispositions of cases by the police are drawn from two New Jersey counties: River County, one of the most populous and heavily urban counties in the state, and Woodlawn County, a nearby, largely suburban county. (The names of both counties are fictitious.) River County is dominated by a blighted older city of about 300,000 which is surrounded by smaller cities and large suburban towns, increasing in affluence as distance from the center city increases. Its population is 30% black and 9% Hispanic. The extent to which the county is burdened

with urban problems is reflected in high rates of school dropouts and unemployment and by the proportions of unskilled blue-collar workers and families below the poverty line. River County has a high crime rate and a high volume of juvenile complaints. Minority population is concentrated in the center city.

Woodlawn County is largely suburban, with the county seat located in an old city of about 40,000, now outsize by sprawling suburban municipalities and outlying older towns. Only 5% of Woodlawn's population is black, and 5% is Hispanic. In contrast to River County, Woodlawn County has far fewer of the social problems mentioned above; when all counties for the state are ranked, it occupies an intermediate position.

Three police juvenile bureaus were selected from each county: one from the county seat, one from a large town, and one from a small town. Location in the same state and county meant a helpful degree of standardization and consistency in record keeping and procedures. Because of the importance previous work has attributed to the race of the police, we checked the racial composition and policies of the police agencies studied. We found that in each municipality, blacks and Hispanics were substantially under-represented in the force in comparison with the proportion of blacks residing in the municipality. In the smaller towns, no members of minorities were on the force. Police sources in municipalities having black officers report that no attempts are made to match race of the officers to the racial composition of neighborhoods to which they are assigned.

While demographic characteristics of these two counties differed greatly, the operation of their juvenile courts was quite similar. Some similarities are structural and procedural, as both were required by the state to use some of the same forms and procedures. Beyond this, interviews with judges and other personnel indicated that the philosophy of the head judges and other staff was quite similar in the two courts. Both judges had several years of experience in juvenile court and tended to emphasize the parental model of the court's role, although they endorsed and claimed respect for the recent emphasis on due process. The most frequently noted difference was the repeated claim by other juvenile justice and social service professionals that the Woodlawn judge was consistently overly permissive.

In our log-linear analysis of court dispositions, we include data from the juvenile courts of four additional counties: one additional urban county with a relatively high percentage of minority residents (adjacent to River County) and three nonurban counties with a low percentage of black residents. Tabular analysis of data from each of these counties taken separately produced findings similar to those for River and Woodlawn Counties (see table 2)—in no county was the bivariate relation between race and disposition substantively significant. Inclusion of these additional cases enables us

to perform a higher-order multivariate analysis by increasing the cell sizes in the n -way cross-tabulations.

Sampling

Delinquency cases processed in the first eight months of 1973 and 1975 were selected randomly from official records of individual cases in the county courts and in each of the six municipalities.⁶ In the two small towns, data were recorded for all cases processed. The resultant samples consist of 1,271 police juvenile bureau cases, 519 juvenile court cases from River and Woodlawn Counties, and a total of 1,553 juvenile court cases from all six counties. The samples were stratified by sex to insure adequate numbers of females for multivariate analysis; about a third of the cases in each sample are girls.

Measurement and Analysis Procedures

Case data were coded from official police and juvenile court records. At the juvenile court, some pertinent information was found in other documents in the case files, such as reports from schools and welfare workers, and psychiatric and investigative reports. Table 1 lists the variables included in the present analysis. In most cases, coding is straightforward. The legal variables of offense type and prior record, and the extralegal variables of race, sex, and family configuration are employed in both samples. The dependent variables are specific to the police and court samples.

The "release" category of police disposition includes "conditional release," which involves the imposition of certain conditions such as making restitution or compensation, or participating in some kind of "voluntary" social service program. Since there are relatively few such cases, since there appears to be little subsequent checking up on enforcement of the sanctions imposed, and since the young persons are in fact released, it seems most reasonable to collapse these cases into the release category. For court disposition, dismissal includes "adjourned" cases, since they stand as dismissed unless the juveniles are returned to court on subsequent allegations. The intermediate category consists mainly of probation cases but also includes suspended sentences and those few cases waived to adult court.⁷ The severe

⁶ Two distinct time periods were used because the study was also designed to measure the impact of legal change. Processing patterns in the two years manifest only random variation, however, and therefore they are collapsed in this analysis. For a detailed analysis comparing findings from the two years, see Dannefer and DeJames (1979).

⁷ It could be argued that waiver to adult court is a severe disposition. However, incarceration is only one of a number of outcomes of waiver to adult court. More-

category contains only those juveniles actually placed in a correctional facility.

Log-linear analysis is used to test the effects of variables on dispositions. This form of analysis allows assessment of effects of categorical variables on other categorical variables and is thus suited to our data (see Burke and Turk 1975; Cohen and Kluegel 1978). Since a causal structure is assumed for the analysis (i.e., disposition is considered as dependent on the other variables), a "modified multiple-regression" approach (Goodman 1972*b*) is used to develop a model. In this approach, a term representing all interactions of the independent variables with each other is included in each

TABLE 1
INVENTORY OF VARIABLES FOR THE ANALYSIS

Variables	Categories
Dependent:	
Police disposition.....	<ol style="list-style-type: none"> 1. Release (including conditional release) 2. Court: forward case to juvenile court
Juvenile court disposition..	<ol style="list-style-type: none"> 1. Dismissed: includes adjournment (dismissed contingent on good behavior) and placement under care of human service agency 2. Probation or other sanction: consists mainly of cases placed on probation, but also includes those given suspended sentences or waived to adult courts 3. Incarceration: placement in postdispositional correctional facility
Legal:	
Offense type.....	<ol style="list-style-type: none"> 1. Violent offenses: murder, manslaughter, forcible rape, atrocious assault and battery, kidnapping, assault and battery 2. Property offenses: breaking and entering, larceny, auto theft, stolen property offenses 3. Drug offenses: possession or usage of marijuana, narcotics, or controlled dangerous substances 4. Minor offenses: vandalism, failure to give good account, disorderly conduct, trespassing, intoxication, loitering, escape, miscellaneous
Prior record.....	Number of prior offenses: coded as none or as one or more according to the evidence contained in the file
Extralegal:	
Race.....	<ol style="list-style-type: none"> 1. Black 2. Hispanic 3. White
Family configuration.....	<ol style="list-style-type: none"> 1. Two parents 2. One parent, other
Sex.....	<ol style="list-style-type: none"> 1. Male 2. Female
County.....	<ol style="list-style-type: none"> 1. River (and one adjacent county in the court data) 2. Woodlawn (and three similar counties in the court data)

over, the law provides that the case may be waived to adult court at the request of the accused juvenile. Since we did not know the eventual outcome for these few ($N = 9$) cases, we chose to place them in the intermediate category.

model. In consequence, only the log of the odds of cell frequencies that vary on the dependent variable are subject to "explanation." Therefore, the model is actually equivalent to a logit equation.⁸

Our model-testing procedure begins with an equation involving all main effects. We also test for all effects due to the interaction of two independent variables. We use a combination of forward selection and backward elimination to determine which interaction terms to include in our final models (Goodman 1971). In evaluating models, we rely on χ^2 tests and the coefficients of partial determination. The latter are suggested by Goodman (1972a, pp. 1056-58) for measuring the magnitude of the contribution of variables in a log-linear equation.

FINDINGS

The cross-tabulations in table 2 provide a preliminary view of the association between race and disposition, prior to controlling for the legal variables in the analysis. In River County, there was a very strong association at the zero-order level between race and disposition by the police. While fewer than half of the black and Hispanic juveniles were released, 79% of the whites were released. In Woodlawn County, however, the association was weaker. While almost the same percentage of blacks were released as in

TABLE 2
POLICE AND COURT DISPOSITIONS BY RACE AND COUNTY

DISPOSITION	RIVER COUNTY			WOODLAWN COUNTY		
	Black	Hispanic	White	Black	Hispanic	White
By Police (%)						
Released.....	40	50	79	34	62	54
Referred to court...	59	50	21	66	38	46
Total.....	99 (404)	100 (32)	100 (212)	100 (202)	100 (89)	100 (286)
$\chi^2 = 80.35, df = 2, P \leq .001$				$\chi^2 = 25.62, df = 2, P \leq .001$		
By Court (%)						
Dismissed.....	57	71	55	64	47	57
Probation.....	29	11	31	23	47	36
Institutionalized....	14	18	14	13	5	6
Total.....	100 (231)	100 (28)	100 (64)	100 (31)	99 (19)	99 (110)
$\chi^2 = 4.54, df = 4, N.S.$				$\chi^2 = 4.38, df = 4, N.S.$		

NOTE.—Some percentages do not add to 100 owing to rounding error. *N*'s shown in parentheses.

⁸ See Garrison (1979) for a similar use of log-linear techniques.

River County, only 54% of whites were released. Hispanics in Woodlawn County were released somewhat more frequently than whites.

In contrast to its impact on police dispositions, race was only weakly associated with court disposition. Patterns for blacks and whites were almost identical in River County; Hispanic juveniles were dismissed somewhat more often. In Woodlawn County, the blacks were slightly more likely to have their cases dismissed than whites, who were, in turn, more likely to have their cases dismissed than Hispanics. Blacks were slightly more likely to be institutionalized than whites or Hispanics. In neither county were the associations significant.

This table provides preliminary support for the two hypotheses. Race has a strong effect on police dispositions, but not on court dispositions. Social environment is also related to disposition in the predicted pattern for police dispositions: the racial difference in dispositions is greater in River County than in Woodlawn County. However, black and white juveniles are treated similarly by the courts in both River and Woodlawn Counties. Moreover, Hispanic juveniles are treated more severely than blacks and whites in Woodlawn, and less severely in River County. If this pattern of effects for court dispositions is maintained in our final model, it will suggest a need to specify our social context argument.

Log-linear analyses of the main effects of race and the other variables on dispositions by the police and the courts are presented in table 3. Race is the most important predictor of police dispositions, as indicated by its coefficient of partial determination (.27). The two legal variables, allegation and prior record, also have strong effects. The effects of these three variables are all significant at the .001 level. County and family configu-

TABLE 3

LIKELIHOOD RATIO χ^2 AND COEFFICIENT OF PARTIAL DETERMINATION (CPD)
FOR MAIN EFFECTS* ON POLICE AND COURT DISPOSITIONS

VARIABLE	Police			Court		
	χ^2	df	CPD	χ^2	df	CPD
Race.....	59.91	2***	.270	13.85	4**	.065
Sex.....	.34	1	.002	.84	2	.004
Family configuration..	4.65	1*	.028	3.41	2	.017
Allegation.....	34.90	3***	.177	14.31	6*	.067
Prior offense.....	27.78	1***	.146	66.80	2***	.252
County.....	9.68	1**	.056	.15	2	.001
Model.....	162.28	182	...	198.46	364	...

* All models in this table and succeeding tables include term representing interaction of all independent variables.

* $P \leq .05$.

** $P \leq .01$.

*** $P \leq .001$.

ration both have much weaker effects on police dispositions, but sex is the only variable which has no significant impact. Court dispositions are influenced by fewer of the variables studied, and the effect of race on court dispositions is much weaker than its effect on police dispositions. Neither sex, family configuration, nor county has a main effect on court dispositions.

Construction of final models is based on the evaluation of two-way interactions in tables 4 and 6. The effects of all two-way interaction terms were tested, and only those which added significantly to the model of all main effects were eligible for inclusion in the final model. There are three such interaction effects on police disposition, as reported in table 4: race by allegation, race by county, and family configuration by county. Since these interaction effects involved terms in common, it is possible to achieve a

TABLE 4
LIKELIHOOD RATIO χ^2 FOR TWO-WAY INTERACTION
EFFECTS OF RACE (R), SEX (S), FAMILY CONFIGURATION (F), ALLEGATION (A), PRIOR OFFENSE (P), COUNTY (C) ON POLICE DISPOSITIONS

Reduction of χ^2 from All- Main-Effects Model for:	χ^2	df	P
By Forward Selection			
RS.....	5.35	2	< .10
RA.....	20.11	6	< .01
RP.....	4.00	2	< .20
RF.....	1.51	2	> .20
RC.....	15.60	2	< .001
SA.....	6.24	3	< .20
SP.....	3.12	1	< .10
SF.....	1.44	1	> .20
SC.....	.61	1	> .30
AP.....	3.19	3	> .30
AF.....	.17	3	> .5
AC.....	3.26	3	> .5
PF.....	.18	1	> .5
PC.....	3.68	1	< .10
FC.....	5.20	1	< .05
By Backward Elimination: Addition to χ^2 due to Dropping RA, RC, or FC from Model Containing All Three and Main Effects			
RA.....	22.07	6	< .01
RC.....	15.18	2	< .001
FC.....	2.27	1	< .20
By Backward Elimination: Reduction of χ^2 due to Dropping RA from Model with Main Effects and RC			
.....	4.51	4	> .20

more parsimonious model by removing those effects which do not make a significant independent contribution. As indicated in the bottom portion of table 4, neither the interaction of family type and county nor that of race and allegation adds significantly to the explanatory power of the model when the interaction of race and county is included. Only the interaction of race and county is, therefore, left in the final model.⁹

The final model of police dispositions includes all variables in the original specification except sex, and also the interaction of race and county. In table 5, λ coefficients are presented to show the pattern of effects, together with the χ^2 tests which show the significance of these effects. The interaction of race and county adds significantly to the main-effects model presented in table 3, while the removal of sex has no effect.

TABLE 5
EFFECT PARAMETERS (λ) AND SIGNIFICANCE LEVELS
(Likelihood Ratio χ^2) FOR THE FINAL MODEL
OF POLICE DISPOSITIONS

VARIABLE	λ BY POLICE DISPOSITION	
	Release	Court
Race ($P < .001$):		
Black.....	-.278	.278
Hispanic.....	.044	-.044
White.....	.233	-.233
Family configuration ($P < .05$):		
Two parents.....	.069	-.069
Allegation ($P < .001$):		
Violent.....	.006	-.006
Property.....	-.067	.067
Drug.....	-.201	.201
Minor.....	.262	-.262
Prior offense ($P < .001$):		
None.....	.167	-.167
County ($P < .01$):		
Urban.....	.054	-.054
Race by county, urban ($P < .001$):		
Black.....	-.023	.023
Hispanic.....	-.155	.155
White.....	.179	-.179

NOTE.—Model: $\chi^2 = 147.14$, $df = 181$, $P > .5$.

⁹ In their study of a juvenile court, Carter and Clelland (1979) provide evidence to support the general hypothesis that bias would be more likely in cases involving minor ("victimless/status") offenses than in cases involving "crimes against persons and property." Although our data are not directly comparable since we do not include status offenders, the direction of the RA (race-allegation) interaction was, as indicated by the λ coefficients (data not shown), consistent with their hypothesis. In the court data, however, the race by allegation interaction effect was not significant, even when it was the only interaction term included in the model (see table 6).

The pattern of effects of race is consistent with the corresponding percentages in table 2. Blacks were less likely to be released than whites; Hispanics occupied an intermediate position. The coefficients for the interaction of race and county show that this differential treatment was particularly strong in River County, where Hispanics were especially disadvantaged. Property and drug crimes were treated more harshly than minor crimes. (The relatively lenient treatment of cases involving violence may reflect the nature of the many juvenile cases which involve fights and other conflicts between peers.) Juveniles without a prior record were much more likely to be released than those with a record, and cases in River County were treated somewhat more leniently by the police than those in Woodlawn County.

Two-way interaction effects on court dispositions are evaluated in table 6. Four two-way interactions are significant: race by county, family by prior offense, family type by county, and allegation by county. As with the analysis of effects on police dispositions, all terms which do not add significantly to the predictive power of the model when other terms are included are deleted in constructing the final model. The combination of forward inclusion and backward elimination of effects yields a final specification that includes the interaction of allegation and county and of family type and county.

The final log-linear model of court dispositions is presented in table 7. Sex, which again has no significant main or interactive effects, has not been included in this model. The main effects of family type and county were included in the specification but are not presented because their significant effects were limited to their role in interactions. The pattern of effects of race on disposition indicates that white juveniles were somewhat more likely to have their cases dismissed by the courts and were less likely to be institutionalized than black or Hispanic juveniles. The group that was treated most harshly, however, is Hispanics. Both black and white juveniles had their cases dismissed more often and were institutionalized less frequently than Hispanics. As has been consistently found in research on juvenile justice, juveniles with no prior offenses had a substantial advantage in court.

Because of the importance for our theory of the race-by-county interaction term, we have also examined the λ coefficients of that term when included in the model of court dispositions in table 3, even though its effect is not significant independent of the other terms in the final model (see table 8). These coefficients reveal an unexpected pattern. In comparison with their treatment in Woodlawn, black juveniles in River County are more likely to be dismissed and less likely to be institutionalized than are white juveniles. Hispanics occupy an intermediate position in this interaction effect.

DISCUSSION

The two hypotheses proposed are largely supported by this analysis of police and court dispositions. The race of a suspect is much more important in police than in court dispositions. For police dispositions, the effect of race is much stronger in River County, with its high percentage of black residents, than in Woodlawn County. Our findings also provide some evidence of better treatment of white than black juveniles in court. As our hypothesis

TABLE 6
LIKELIHOOD RATIO χ^2 FOR TWO-WAY INTERACTION
EFFECTS OF RACE (R), SEX (S), FAMILY CONFIGURATION
(F), ALLEGATION (A), PRIOR OFFENSE (P), COUNTY (C)
ON COURT DISPOSITIONS

Reduction of χ^2 from All- Main-Effects Model for:	χ^2	df	P
By Forward Selection			
RS.....	3.55	4	> .5
RA.....	7.66	12	> .5
RP.....	8.83	4	< .10
RF.....	5.35	4	> .20
RC.....	9.55	4	< .05
SA.....	10.02	6	< .20
SP.....	.72	2	> .5
SF.....	.02	2	> .5
SC.....	.26	2	> .5
AP.....	7.86	6	> .20
AF.....	7.40	6	> .20
AC.....	16.43	6	< .02
PF.....	6.52	2	< .05
PC.....	1.26	2	> .20
FC.....	7.98	2	< .02
By Backward Elimination: Addition to χ^2 due to Dropping RC, AC, PF, FC from Model Containing All Four and Main Effects			
RC.....	5.99	4	< .20
AC.....	15.25	6	< .02
PF.....	4.00	2	< .20
FC.....	4.68	2	< .10
By Forward Selection: Reduction of χ^2 from All-Main-Effects Model + AC for			
RC.....	7.92	4	< .10
PF.....	6.73	2	< .05
FC.....	8.13	2	< .02
By Forward Selection: Reduction of χ^2 from Adding PF to All-Main-Effects Model + AC + FC			
PF.....	4.39	2	< .20

suggested, the difference is not large. Hispanics, a group combined with blacks by Cohen and Kluegel (1978), are treated more harshly by the courts than are white or black juveniles. Contrary to our initial hypothesis, the courts in River County dispose of the cases of black juveniles with less severity than do the courts in Woodlawn County. While this interaction effect is weak, its direction suggests an important specification of the relation between minority percentage and likelihood of bias—an issue we address below. Consistent with other studies, legal variables of allegation and

TABLE 7
EFFECT PARAMETERS (λ) AND SIGNIFICANCE LEVELS (Likelihood Ratio χ^2)
FOR THE FINAL MODEL OF COURT DISPOSITIONS

VARIABLE	λ BY COURT DISPOSITION		
	Dismiss	Probation	Institutionalize
Race ($P < .01$):			
Black.....	.050	-.072	.022
Hispanic.....	-.141	-.104	.245
White.....	.091	.176	-.267
Allegation ($P < .05$):			
Violent.....	.020	-.052	.032
Property.....	-.073	.148	-.074
Drug.....	-.164	-.014	.178
Minor.....	.217	-.082	-.135
Prior offense ($P < .001$):			
None.....	.336	-.066	-.271
Allegation by county, urban ($P < .05$):			
Violent.....	.178	-.059	-.119
Property.....	.002	.098	-.100
Drug.....	-.180	-.188	.368
Minor.....	.001	.148	-.149
Family configuration by county ($P < .05$):			
Urban.....	-.115	.042	.073

NOTE.—Model: $\chi^2 = 174.70$, $df = 358$, $P > .5$.

TABLE 8
EFFECT PARAMETERS (λ) FOR TWO-WAY
INTERACTION EFFECT OF RACE AND
COUNTY, IN MAIN-EFFECTS MODEL

RACE BY URBAN COUNTY	COURT DISPOSITION		
	Dismiss	Probation	Institutionalize
Black.....	.096	.048	-.144
Hispanic.....	.060	-.066	.006
White.....	-.156	.018	.138

NOTE.—This model also includes the two-way interaction term for race and allegation. This term does not have a significant effect. The coefficients for the main effect of race on disposition for this model are very close to those reported in table 7.

prior record are very important in dispositions at both levels of the juvenile justice system studied. Although female juvenile cases have been neglected in many previous studies, our results indicate that their inclusion did not alter the race-disposition relationship that would have obtained for males only.

CONCLUSIONS

Our basic hypothesis was that bias in official decision making varies with social-structural factors. As specified in terms of (1) ecological and (2) organizational variables, it has received strong support in this analysis. Moreover, the social-structural dynamics suggested by our findings provide some basis for ordering the conflicting findings of previous research. While there was evidence of substantial bias in the likelihood of the police sending juveniles to court, there was much less evidence of bias in court decisions. The degree of such bias apparent in police dispositions varied greatly across the two social contexts studied, as suggested by our theory. Thus the findings suggest that the importance of social context in producing differences in findings between studies should be given more systematic attention.

Although we found less evidence of bias in the juvenile court than among the police, court decision making is not independent of police decisions. If there is bias at the point of police dispositions, it will ultimately translate into differences in prior record—a variable which had a stronger effect on court dispositions than any other variable studied. The effect of prior record may, in other words, include a component due to police bias. This general problem of “bias amplification,” which has been more fully discussed by Farrell and Swigert (1978) and by Liska and Tausig (1979), cannot be resolved in studies of bias without more detailed information on cases which are followed from initial contact with the police to final disposition in court. However, our evidence of more favorable dispositions of cases of black juveniles than of whites in River County, compared with Woodlawn, suggests another connection between police and judicial decisions: judges may actually compensate for differential treatment by the police, to some extent, rather than simply reinforce it.¹⁰ The greater formalization of the decision-making process, and perhaps the greater insulation of judges from direct community contact, may account for this complex interaction between environment and degree of bias in the police data.

Our finding of more severe court dispositions for Hispanic than for black juveniles also has relevance for a social-structural approach to the explana-

¹⁰ This possibility suggests a process whereby decisions made at later stages in the system correct for bias exercised at earlier stages, and thus contrasts with the “bias amplification” findings of Farrell and Swigert (1978). However, it is clear that both processes may occur simultaneously; our findings *do* indicate some bias in the juvenile court, but less in jurisdictions where bias had been more pronounced in earlier stages.

tion of bias in decision making. The differences in the social situations of Hispanics, blacks, and other minority groups should be assessed in future studies before making an a priori decision to use only a minority-non-minority classification.

We do not have the measures of complainant preference and of offender behavior which have been found to have an impact on police dispositions in other settings (Black and Reiss 1970; Piliavin and Briar 1964). Inclusion of these variables might have altered the findings obtained. We did run separate analyses for models of police dispositions which substituted age for sex and a core/periphery variable (within counties) for sex. Neither age nor the core/periphery variable changed the effect of race on police dispositions.

Our findings clearly indicate the importance of attention to social environment. Nonetheless, although the sampled counties differed on a key dimension (percentage minority), the limitation of our samples to one state means that we did not compare maximally different social environments. While our test for bias by the police in the two environments is therefore a relatively stringent test of the potential for environmental effects, our hypotheses need to be tested again in other settings.

On the basis of our findings, we suggest that future research in this area should concentrate on comparative studies in which social environments are sampled and on direct measures of characteristics of social environments. The theoretically important question which research should address is not whether there is bias in the juvenile justice system but, rather, under what conditions it is more likely or less likely to occur. An answer to this question has been suggested in this study in terms of basic features of the social environment, of the criminal justice process itself, and of an interaction between the two.

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Age, Housing Choice, and Neighborhood Age Structure¹

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Prior studies of age segregation have been restricted to the use of aggregate data for blocks or census tracts, and to an aggregate assumption that the actual behavior of individuals was being observed. This study, however, focuses on the process whereby individual differences in age are transformed into age segregation; it does so through observation of the housing and location choices of individuals. A large sample of the population of all urbanized areas is used to test how persons of all ages fit into patterns of age segregation. In a path model, age is found related to neighborhood age structure, but its effects are considerably outweighed by those of housing and location choices. Elaboration of this general path model by age group shows that the individual model of age segregation operates least well among the elderly. Further exploration yields evidence that, among the elderly population, the traditional routes to age segregation have been replaced in recent years by an alternate process. It is suggested that the individual approach to age segregation would, if applied to other forms of segregation, advance knowledge of the phenomena.

One of the central assumptions in social research is that aggregate differences necessarily reflect underlying social differences. What was implicit in Durkheim's methodology of studying "realities external to the individual" (1951, pp. 37-38) has suffused much of sociology, and nowhere is this assumption more evident than in the study of age segregation. While the aggregate assumption has served well both sociology in general and human ecology in particular, the aggregate approach to age segregation leaves unresolved the problem of the individual. That is, research attempts to explain individual involvement in age segregation have yielded only tentative findings because connections between individual involvement and aggregate data require correlational inferences about the behavior of individuals. How individual differences in age are transformed into a spatial ordering on age

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is the central question addressed here. In this research the relationship between the age structure of neighborhoods and individual characteristics which locate their residents in the social order are explored through a path model applied to the 1970 urbanized area population.

MACRO- AND MICROTHEORIES OF AGE SEGREGATION

In the past many human ecologists have depicted residential patterns as the outcome of impersonal social and economic market forces. Residential space is allocated through a competitive process which pits housing against other potential land uses. The most valued land is given over to those organizations and groups able to mobilize the necessary resources. Other groups find their ineluctable places in the remaining space. Urban structure takes shape as a result of the natural working out of competitive and symbiotic relationships among land uses, in a process that is essentially economic. Residential segregation in all forms—racial, ethnic, social class, family type, and age—is one of the more important outcomes of this process. For racial and ethnic segregation, and in part for social class segregation, additional competitive advantage may accrue to those who can influence institutions such as zoning boards, lending agencies, building firms, real estate agencies, and school boards. In contrast, the distribution of age and family groups cannot be attributed to the domination of these market institutions, for age and family-type segregation are usually not focal points for any group; thus the strictly economic interpretation holds sway in these areas.

From this ecological interpretation of segregation it follows that age and family-type segregation are caused by aggregate morphological features of cities and neighborhoods. For example, Gûest (1977, pp. 285–86) finds that the distribution of family types is attributable to housing features and general land-use patterns of neighborhoods. Global SMSA-wide measures were used by Cowgill (1978) to predict age segregation, and aggregate measures were used by Pampel and Choldin (1978) to predict age structure for city blocks. These and other efforts to explain age and family-type segregation have noted the ways in which individuals and families mesh with existing patterns of segregation. Typically, housing needs accompanying the family life cycle have been acknowledged as the key to understanding how families come to be segregated. Thus, a theoretically unified picture of age and family-type segregation has been presented, one in which the housing choices of families are matched to the morphological characteristics of neighborhoods. From the ecological viewpoint, age segregation occurs because areas vary in the dominant characteristics of their housing stock, while from the behavioral viewpoint the major determinant of age segregation is the difference in housing choices of households according to their life-cycle stage. All research to date has tested directly the ecological expla-

nation of age segregation, and has tested only by implication the behavioral explanation. Using a model based on the housing choices of individuals, this study tests directly the behavioral explanation by observing housing and location decisions in relation to neighborhood age structure.

PROPOSED MODEL

Individuals, insofar as they play a role in segregation, have been seen in past research as responding to urban processes. However, it is potentially instructive to take a different perspective and hypothesize that individuals initiate a sequence of decisions leading to age segregation. Theories of age segregation at the individual level have been clearly enunciated in the literature (Golant 1972; Pampel and Choldin 1978). Figure 1 reflects some of these ideas and illustrates the proposed path model. In conformity with the present approach, that of explaining age segregation at the individual level, all variables prior to age structure represent individual or family measures, and the dependent variable, age structure, is an aggregate measure. As a dependent variable, however, age structure is not cast in the usual causal sense, because no individual acting alone can cause the age structure of an area to differ from that of another area. Neighborhood age structure is a consequence, for the most part, of the housing and location choices families make. Families with similar social or economic backgrounds have similar housing needs and location preferences. Thus age segregation, as depicted

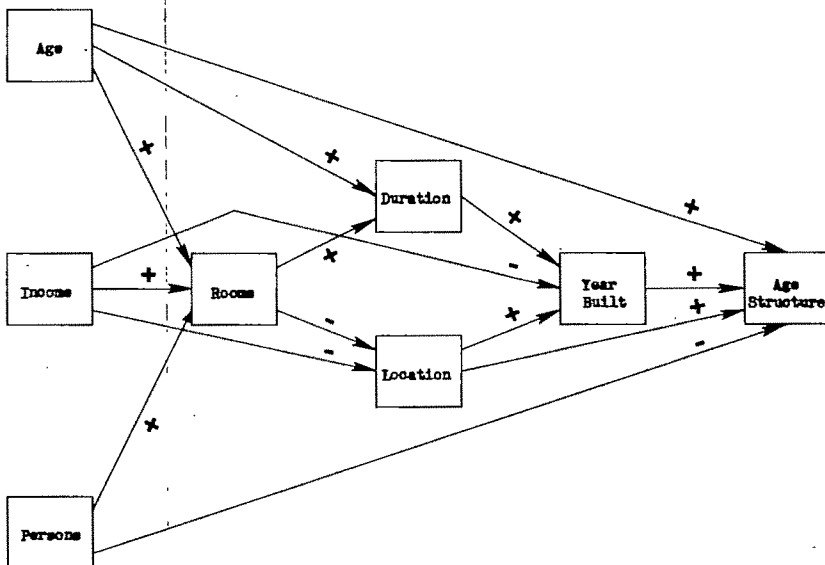


FIG. 1.—Proposed path model of determinants of neighborhood age structure

in figure 1, is the result of the unplanned coalescing of families with parallel housing and location choices.

A sense of hierarchy of choices pervades the housing and location aspects of the model. No time-ordered sequence of events that leads to location and number of rooms can be demonstrated easily. Indeed, current family income and age may have been acquired years after the original choice of housing and location. Furthermore, the dependent variable, neighborhood age structure, also changes over time; thus the 1970 level may differ considerably from the level at the time when prior variables in the model attained their current values. In short, the model is reasonable only if a rational, ongoing evaluation process is allowed. It is realistic to assume a continuous decision process with respect to housing, a process culminating from time to time in residential mobility. Few moves are capricious decisions, as Rossi (1955, pp. 113-17) has demonstrated, and a cross-sectional model tested with a sufficiently large sample will capture households across the spectrum of constraints under which families operate. Families continually review the congruence between housing desires and current residence, taking into account various location factors and feasible housing alternatives.

If we assume a continuous rational decision process with respect to housing, the demand for housing space takes priority among potential housing variables such as housing type and tenure status. Rossi (1955, p. 153) has shown that space is probably both the most important quality of a home and the housing need most closely attuned to life-cycle changes. Other authors (Foote et al. 1960; Speare 1970; Johnston 1971; Golant 1972; Guest 1977) have linked the family life cycle to residential choice. For example, families are more sensitive to space needs during the formative years than in later years when the relationship between family size and housing space tends to be less elastic. Although many older families remain too long in homes too large for their family size, income, or ability to maintain them, such factors apply increasing strain on the inertia of these families. In general, age and family size represent major components in the family life cycle, and along with family income they affect the demand for housing space.

Dwelling size has two important consequences: it affects where a family lives and how long the family remains there. Small families orient their search for smaller quarters toward a central city location, whereas those seeking more space tend to direct their search toward the metropolitan fringe. At this point, the model assumes that families respond to the urban housing structure, with its apartments concentrated in the central city and its single-family housing in the suburbs. Guest (1977, p. 284) explicitly relates age and family type to location within the urban housing structure, although he sees older couples as neither centralized nor decentralized. Favoring the central city or suburbs is a derived value based on considera-

tions of housing, site, physical environment, and, not infrequently, neighborhood amenities and composition. Income plays a major part in metropolitan area location, regardless of the number of rooms occupied.

Families occupying larger homes tend to remain in them longer than families in smaller apartments. A family will usually move to a large home at a time when it is approaching or has reached its maximum size, with the intention of remaining there at least until family size starts to diminish. In contrast, smaller apartments tend to be occupied by smaller families for shorter periods, largely because levels of housing satisfaction are considerably lower but also because less tangible factors centering on home ownership and prestige are involved.

Age and residential mobility are inexorably linked, usually because of age-related processes involving marriage and family formation, and career and job mobility (Goldscheider 1971). In the absence of life-cycle variables, the present model hypothesizes a direct path between age and duration of residence.

Choosing to remain long in one dwelling and choosing to locate in the central city lead to the same result—living in older housing. Income too is a factor, since most families probably prefer newer housing. Hence, occupying older housing is a consequence of tenure, location within the urbanized area, and income. Finally, the model allows four direct influences on the age structure of a household's neighborhood: age of the dwelling unit, location within the urbanized area, number of persons in the household, and age of the household head.

The path from individual age to area age structure is theoretically decisive in evaluating the model. In any sample of individuals who are age segregated, no matter what the reason may be, there will always be a marginal relationship between the age of individuals in an area and an aggregate measure of their age. If age segregation were based on a preference to reside with age peers, the path between individual age and neighborhood age structure would represent a redundant and large path. Prior conceptions of age segregation have not entertained the possibility of segregation based on age preferences alone, independent of housing—in part because the ecological model has dominated thinking on the subject, but also because such preferences may be a recent development. The present individual version of the ecological model will hold when other variables intervene between age and age structure, and either fully explain or dominate that relationship. In the model, the housing and location variables, taken as a whole, act as intervening variables between neighborhood age structure and the variables representing family and social status; but direct paths remain from individual age and family size to neighborhood age structure. Family income, number of persons in the household, and age are also related to

neighborhood age structure because the choice of housing and location in urban areas is contingent on these three variables.

DATA AND METHODS

As part of its program for providing data from the 1970 census the Bureau of the Census released a public use sample (U.S. Bureau of the Census 1972) containing data at the individual, household, and neighborhood levels. One datum in this file is the percentage of persons aged 65 and over who live in the neighborhood; this percentage is the dependent variable in my analysis. Because the unit of analysis is the individual, it is best to think of this variable as an individual measure of age segregation. It does not represent the degree of segregation between old and young for an entire urban area, as would a measure of age segregation. From an individual perspective it simply describes the probability of finding a person aged 65 or over within a specific neighborhood, without reference to the situation in the larger urban area of which the neighborhood is a part. It is a measure of local age structure or concentration. At the individual level of analysis, the percentage of the population aged 65 and over is a more realistic measure of actual experience than is an abstract measure such as an age-segregation index.

Neighborhoods are geographic areas with an average population of about 4,000 (Zeisset 1972). But a small and unknown number of neighborhoods consist of noncontiguous segments (a fact which probably explains why this file has gone relatively unused). The Census Bureau has designated neighborhoods by following the sequential order in which enumeration districts and blocks fell on its tape file. For the most part, blocks and enumeration districts were numbered in contiguous sequence. But occasionally they were not. It was possible to identify some of these cases from misformed neighborhoods. That is, households reported within the same neighborhood but with different size-of-place codes were omitted from the sample. The misformed neighborhoods contained about 9% of the urbanized area population and were concentrated in the fringe areas where it was most difficult to attain the minimum population required to form a neighborhood. As a result of eliminating these cases from the sample file of one in 1,000, the remaining 34,659 sample households which reside in 18,005 neighborhoods somewhat overrepresent the central city and the elderly population. Tests of the model using only eliminated households yielded results less clear-cut than those from acceptable neighborhoods—a fact which corroborates the irregularity of these misformed areas.

Yet another drawback of the data is the fact that neighborhoods are not centered on sample households, so that the actual neighborhood encountered by a household probably differs from its census neighborhood. On the

other hand, census neighborhoods have the virtue of being large enough so that the ages of persons in any one household cannot seriously distort the value of the dependent variable, but not so large as to submerge local variation in the fundamental heterogeneity of the larger community. Neighborhoods are used in this study in much the same way that census tracts are used—as statistical artifacts; the term “neighborhood” is retained only to conform to usage in Census Bureau documentation.

One consequence of sampling individuals instead of using aggregate data for geographic areas is that traditional approaches to measuring segregation through segregation indexes are inapplicable. Operationally this means that the measurement of segregation must rest on the relationship between the characteristics of individuals and the aggregate characteristics of their neighborhoods. This is all to the good, for segregation may be defined as the nonrandom distribution of individuals, and, therefore, the greater the degree of segregation, the larger the association between individual and area measures of the same characteristic. Table 1 illustrates this point by presenting three sets of correlations: between the ages of individuals in a neighborhood and the percentage of persons aged 65 and over; between family income in 1969 and the percentage of families with income below \$5,000; and between individual race (black or nonblack) and the percentage of black persons. Counts of the number of neighborhoods used are shown for general interest. These measures are presented for the entire urbanized area population and for the six urbanized areas identifiable on the neighborhood file.

As expected, the correlations for age segregation are weak, whereas they are comparatively strong for race segregation, with correlations for income falling between the two. There is considerable range in correlations between urbanized areas; but the lack of overlap between age, income, and race segregation makes it clear they are of a different order. Except for the issue

TABLE 1
CORRELATIONS BETWEEN NEIGHBORHOOD AND
INDIVIDUAL CHARACTERISTICS

AREA	CORRELATIONS			PERSONS (N)	NEIGHBOR- HOODS (N)
	Age	Income	Race		
All U.S. neighborhoods.....	.217	— .348	.751	104,780	18,005
Boston.....	.134	— .321	.768	23,450	508
Rochester.....	.156	— .332	.644	4,340	107
Denver.....	.224	— .330	.728	9,578	197
Phoenix.....	.242	— .290	.603	8,038	162
Seattle.....	.222	— .291	.587	10,938	235
Portland.....	.167	— .244	.622	6,908	147

SOURCE.—Data for all neighborhoods are from the 1/1,000 public use sample; data for all six cities are from the 1/100 public use sample (U.S. Bureau of the Census, *Public Use Samples of Basic Records from the 1970 Census: Description and Technical Documentation* [Washington, D.C.: U.S. Department of Commerce, 1972]).

of choosing appropriate areal units, the individual approach to measuring segregation seems free of some of the methodological difficulties attendant upon aggregate measures (Duncan and Duncan 1955; Van Valey and Roof 1976).

In tests of the model shown in figure 1, the analytical tool, path analysis, solves a set of equations from the correlations between variables in the model. Furthermore, the correlations estimated by the model may be tested against the actual correlations using a technique outlined by Specht (1975). An advantage of this technique is that it yields a coefficient of fit for the entire model, Q , which ranges between zero and one, with one representing a perfect fit. The following list further specifies the variables entering the model:

Path model	Description
Age	Age of household head
Persons	Number of persons in household
Income	Total family income in 1969
Rooms	Number of rooms in dwelling unit
Location	Residence in central city = 1; residence in fringe = 0
Duration	Duration of residence
Year built	Built 1949 or before = 1; built after 1949 = 0
Age structure	Log of the percentage of neighborhood population aged 65+

Data for households are used, and age of the household head is selected. Age structure is represented by the log of the percentage of the population aged 65 and over; a log transformation was chosen because of the high degree of skew in neighborhood age structure, which ranges from less than 1% to 80% with a mean of 11%.

INITIAL RESULTS

Turning now to the test of the model, figure 2 shows results for the sample households. Most important, the model fits the data quite well, with Q equal to .957; this indicates that the paths assumed to be zero contribute little to the model. Without exception, all coefficients are strong and many times their standard errors. Age of the dwelling unit is the dominant path to neighborhood age structure. If age were the only influence on age structure, its standardized coefficient would be .247; but the introduction of other considerations dominates the raw age effect and reduces it by almost half. Nevertheless a considerable age effect remains. Moreover, although the model provides several indirect paths through which age may affect age structure, the direct path is many times the size of the indirect paths. Further comment is warranted because it is the direct path between age and age structure that is critical in interpreting the model.

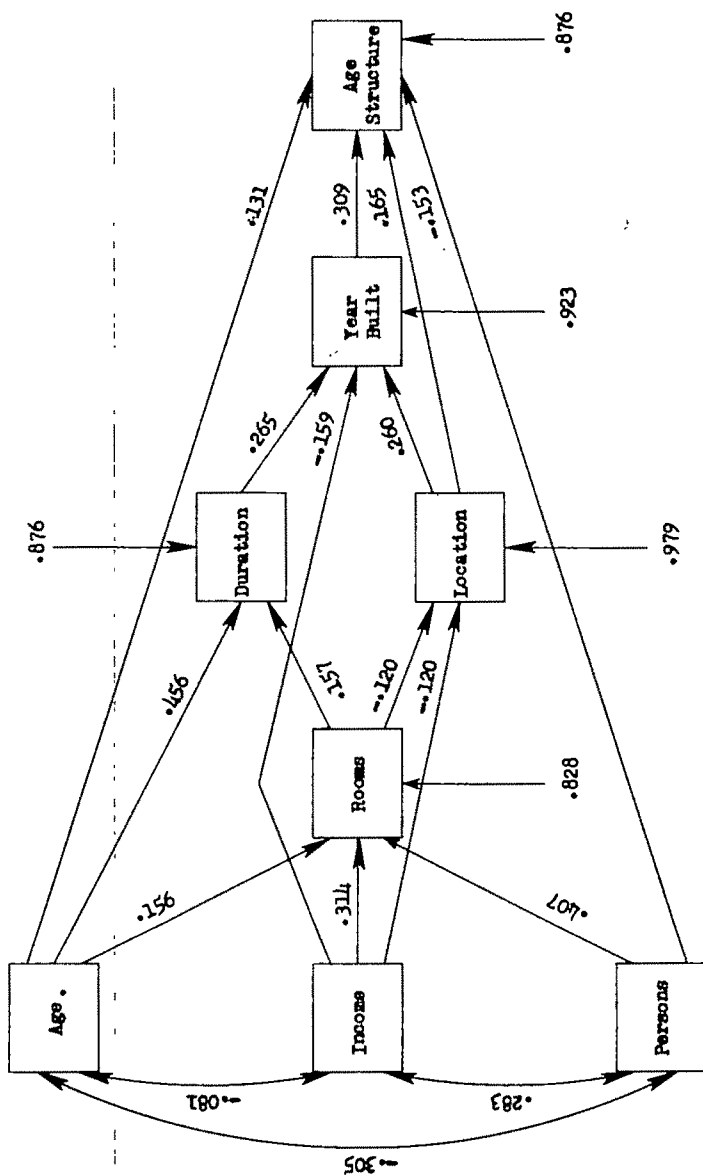


FIG. 2.—Path model of determinants of neighborhood age structure: results for sample households

Unless there are unspecified independent housing or location variables which might explain more of the direct age effect, some process associated with age alone is indicated. For example, there may be neighborhoods near special services such as schools, medical facilities, or churches, that attract one age group or another disproportionately. Some neighborhoods may be home for an ethnic or racial group which has a young or old age structure, and what may appear to be age segregation may in reality be age segregation superimposed on a pattern of ethnic or racial segregation. Under some circumstances a preference for age peers plays a part in age segregation. It is easy enough to point to retirement centers where direct selection by age replaces the indirect hand of the marketplace. Direct selection by age is also found in public and private housing for the elderly. This housing would produce a reduced or negative coefficient between age of the dwelling unit and neighborhood age structure because most housing for the elderly has been constructed since 1950.

RESULTS BY AGE

It is something of a paradox that the age of individuals is not central to age segregation. However, the comparatively small coefficient for age in figure 2 does not rule out the possibility of another kind of age effect. With the passage of years, or alternatively, in different cohorts, it is possible that the model operates differently. For example, young families respond quickly to changes in family size whereas older families are relatively insensitive to changes in family size. Such a situation could foreshorten the age domain of the general model and dilute the actual effect of age. If age segregation is the result of a deliberate process engaged in by the elderly, we may expect the model to work less well among the elderly. It will, therefore, be useful to apply the general model to age groups and compare its fit among them.

It is well known that path coefficients may not be compared between samples (Kim and Mueller 1976) and, although unstandardized regression coefficients may be compared, they are not particularly informative. Hotchkiss (1976) has justified the use of standard scores, calculated from means and standard deviations, over all samples being compared. This transformation yields Standardized Path Regression Coefficients (SPRCs), which are standardized to the same target values and which may safely be compared across subgroups and variables. In table 2, SPRCs for the general model are presented for three broad age groups: ages 18 to 44, 45 to 64, and 65 and over. These age groups correspond roughly to broad phases of the family life cycle. Notwithstanding the division of the sample into age groups, age is retained as a variable in the model because of the potential

effects of age within age group. By retaining the percentage of population aged 65 and over as the dependent variable, segregation of the two younger groups vis-à-vis each other is ignored. This course is taken on the assumption that segregation of the elderly from other age groups is the most important form of age segregation to be addressed now, and that it will probably increase in importance.

At all ages, the model reproduces the observed correlations with almost equal precision; and at ages 18 to 44 the nearly perfect Q value indicates that the general model captures almost all covariation in the data. Of the 14 coefficients estimated by the model, only one—that between location and age structure—approaches equality across all ages, although three others are similar in two age groups. A test for the equality of coefficients (Specht and Warren 1976) confirms that no coefficient is statistically the same in all three age groups. In short, although the general model holds up well at each age, it is conditioned by age.

TABLE 2
STANDARDIZED PATH REGRESSION COEFFICIENTS FOR MODEL OF AGE
SEGREGATION IN THREE AGE GROUPS

DEPENDENT VARIABLES	INDEPENDENT VARIABLES							R ²
	Age	Persons	Income	Rooms	Duration	Location	Year Built	
Ages 18-44*								
Rooms.....	.122	.414	.284369
Duration.....	.374077166
Location.....	-.143	-.132053
Year built.....	-.162113	.211090
Age structure..	.022	-.157163	.377	.227
Ages 45-65†								
Rooms.....	.011	.348	.329293
Duration.....	.178182055
Location.....	-.102	-.123036
Year built.....	-.157264	.208146
Age structure..	.086	-.104178	.305	.190
Ages 65+‡								
Rooms.....	.010	.263	.227162
Duration.....	.092282085
Location.....	-.080	-.073016
Year built.....	-.084324	.149136
Age structure..	.072	-.104153	.090	.056

SOURCE.—Data are from the 1/1,000 public use sample (U.S. Bureau of the Census, *Public Use Samples of Basic Records from the 1970 Census: Description and Technical Documentation* [Washington, D.C.: U.S. Department of Commerce, 1972]).

* $Q = .989$; $Rm^2 = .654$.

† $Q = .963$; $Rm^2 = .566$.

‡ $Q = .939$; $Rm^2 = .405$.

In general, coefficients are stronger at younger ages. Only two coefficients—that between number of rooms and duration of residence and that between duration of residence and age of dwelling unit—are larger in the elderly population. This is understandable, since recent elderly movers are less likely to move to a large home than are recent young movers, and the elderly with large homes tend to retain them. Housing characteristics and, in particular, age of housing are more clearly associated with neighborhood age structure among the young than among the elderly, for whom choosing either newer or older housing may lead to living in neighborhoods with an older age structure.

Among the elderly the model is fitted to a weaker set of correlations resulting in smaller path coefficients. This situation is confirmed by means of the generalized multiple correlation coefficient (Rm^2), which is used to detect external specification errors in path models (Specht 1975). In the present context, Rm^2 is employed to summarize multiple correlation coefficients over all equations for an age group and to compare among age groups for the relative strength of SPRCs. For the elderly, Rm^2 is .405; whereas it is .566 at ages 45 to 64, and .654 at ages 18 to 44. Thus, although the model captures the covariation in the data at all ages, as shown by high Q values, there is less covariation to capture at successively higher ages.

Apparently, then, the general model of age segregation is least applicable to those most intimately involved in the process—the elderly. Three explanations for this finding suggest themselves: (1) The process is truly random among the elderly; (2) age segregation is tied to some unspecified variables that affect the elderly more than persons of other ages—as, for example, attitudes; (3) an alternate model operates among some part of the elderly population, and the lower coefficients shown in table 2 result from counterbalanced effects. Of these possibilities, the third appears most plausible and may be investigated with the data at hand. Housing for which only the elderly are eligible has become fairly prevalent. Such direct selection by age largely involves newer housing and, to the extent that it offsets the more traditional route to age segregation through older housing, would yield weaker results only for the elderly population.

Testing models that operate simultaneously on a single population is unwieldy and, unless one can specify beforehand the exact population subject to each model, it seems wiser to turn to a less confirmatory and more exploratory technique. Automatic Interaction Detection (AID) provides a method for ferreting out alternate paths to neighborhoods with old and young age structures. The AID cannot test alternate models, but it can assist in establishing the presence and outline of such models if they exist. In the AID analysis there is no need to transform the dependent variable, and the independent variables are coded to the following categories:

AID analysis	Coding
Persons	1; 2; 3; 4; 5+
Income	\$0-\$2,499; \$2,500-\$4,999; \$5,000-\$7,499; \$7,500-\$9,999; \$10,000-\$14,999; \$15,000+
Rooms	1; 2; 3; 4; 5-6; 7+
Location	Central city; fringe
Duration	Moved in: 1965-70; 1960-64; 1950-59; before 1950
Year built	Built: 1965-70; 1960-64; 1950-59; before 1950
Age structure	Percentage of neighborhood population aged 65+

Figure 3 portrays the result of an AID analysis for 6,361 household heads aged 65 and over, with the splitting routine halted at the point where a potential split would involve less than 1% of the total sum of squares of the dependent variable.

Only in broadest outline is the general model of age segregation captured in the AID shown in figure 3. In the initial split, the general model is supported by the selection of the number of rooms occupied as the key variable which differentiates between those living among low versus high concentrations of elderly residents. Subsequent to the split on rooms, interactions within the AID and noteworthy deviations from the general model pointedly suggest alternate paths to age segregation for the elderly. For those elderly people who reside in small apartments (one to three rooms), a high income, residence in the urbanized area fringe, and occupancy of recently constructed housing are associated with high concentrations of elderly residents. The general model specified quite the opposite effects, either directly (as in the case of the paths from location and age of the dwelling unit) or indirectly (as in the case of income through location and age of the dwelling unit). Living in the fringe in a small apartment is identified with the highest level of elderly concentration. If any group of the elderly follows the path predicted in the general model, it is the elderly living in larger quarters in the central city. But even among this group a high income is associated with a high level of elderly concentration.

The idea that the most highly segregated elderly are the poor living in old, central city housing needs refinement. On the contrary; it is the high-income elderly in newer housing on the fringe of urbanized areas who are most age segregated. It appears that recent housing programs aimed at constructing small apartments for the elderly have succeeded in concentrating them to an even greater extent than the normal housing market processes.

There are two other groups among the elderly whose housing choices explain partially why the general model of age segregation does not work well at older ages. First, before 1970 cooperative and condominium housing accounted for less than 2% of the older households in the sample. This housing is fairly small, recently constructed, and draws higher-income

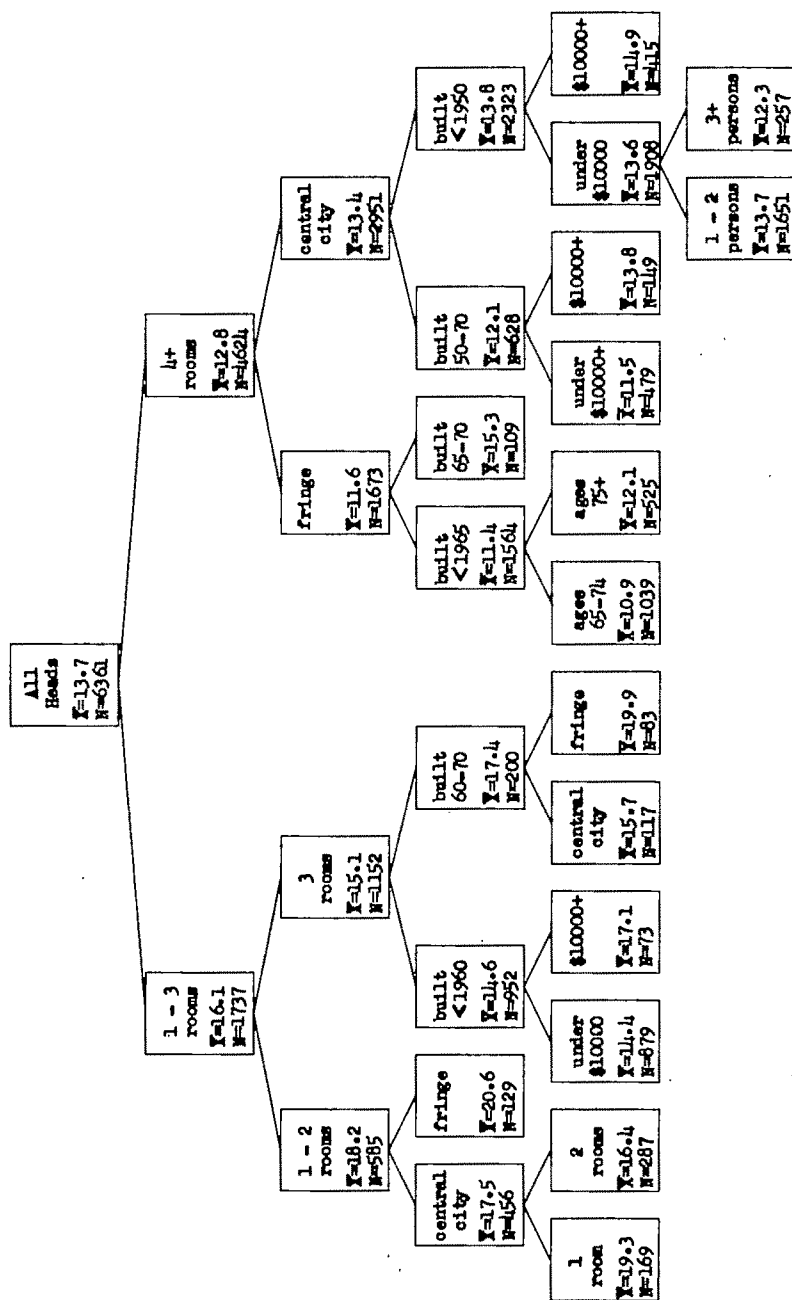


FIG. 3.—The AID analysis of neighborhood age structure for household heads aged 65 and over

elderly. Older occupants of cooperatives and condominiums are likely to find their neighbors similar to themselves in age, for here the mean neighborhood percentage of persons aged 65 and over is 24.1%, as compared with 12.3% for older homeowners and 15.3% for renters.

Residential mobility identifies the second anomalous group. Although this factor did not appear in the AID analysis, if mobility had been measured by distance instead of time, one group, interstate migrants, would have stood out sharply. For older household heads who were interstate migrants during the period 1965 to 1970, the mean neighborhood percentage of population aged 65 and over is 18.5. This compares with 13.3% for non-movers and 14.2% for movers within the county or state of residence. At this level of analysis, it is clear that mobility rather than stability precedes living in a neighborhood with a high concentration of elderly people.

Age segregation can mean either low or high concentrations of the elderly, depending on the framework used. For example, focusing on the housing decisions of household heads makes it possible to miss one group of older persons, parents, whose housing decisions lead them to live in areas where they find relatively few age peers. In urban areas, about one out of 10 non-institutionalized older persons lives with a son or daughter. To the person choosing this living arrangement, the age structure of the neighborhood is probably of secondary importance. Nevertheless, the effect of this decision is probably to amplify the importance of family ties, since elderly people living in the homes of their children are in neighborhoods whose mean percentage of elderly residents is 10.6%, as compared with 13.6% for older heads and their spouses. This form of age segregation of the elderly is strongest in fringe areas, where the mean percentage aged 65 and over is 8.9% for parents in the home of a son or daughter.

SUMMARY AND CONCLUSION

As they grow, metropolitan areas become more differentiated internally. One facet of this process is the differentiation of areas by age of their residents. This study indicates that there are important individual components which contribute to the process. Construction and testing of a model of neighborhood age structure for the urbanized area population of the United States in 1970 has yielded evidence about the process of age segregation as it is experienced by individuals. Age has been found linked to neighborhood age structure in two ways: both directly, through a process in which consonance is achieved between individual age and neighborhood age structure; and indirectly, through the housing and location decisions made by individuals of various social and economic backgrounds. However, the characteristics of a person's housing are considerably more important than is the person's age in determining the choice of a neighborhood. Age segrega-

tion exists primarily because people of similar social and economic backgrounds make parallel location and housing decisions, and only secondarily because of any desire to live near one age group or away from another.

Age segregation is a more complicated process for the elderly than the general ecological model tested here would have us believe. There is no logical reason why the process leading to age segregation need be uniform across or within age groups. It is no longer the poor, central city elderly in older housing who are the most age segregated. Some older people choose to live in housing built for the elderly, much of it newly constructed in the urbanized area fringe. High income facilitates occupancy of this housing, and some of these elderly people are interstate migrants. In view of the high levels of elderly concentration revealed in the AID analysis, future age segregation seems insured under a continuation of current public and private housing programs for the elderly. Past conceptions of age segregation are in need of revision. After a fashion this research does confirm the premise of human ecologists that urban morphological features structure housing decisions; but for the elderly living in age-segregated neighborhoods, there is more than one route to these areas. Most people, young and old, are not living in neighborhoods with an old age structure, and it is clear that social characteristics in conjunction with morphological features determine who will be age segregated.

In asking why families are age segregated, it has been neither possible nor desirable to avoid the question of why areas are age segregated. In the past, both of these questions have been answered from an aggregate stance, a stance most appropriate for finding out why areas are age segregated. But a research design embodying aggregate observations can do no more than suggest inferences about ecologically relevant behaviors; it cannot draw these inferences directly. The approach used here has demonstrated how individuals relate to age segregation. This study has barely pried open the door to the discovery of how and why age segregation is experienced differently in different areas. Nevertheless, this type of question is easier to generate and answer with individual data of the sort I have used. Application of this research design to other problems in human ecology, particularly to racial and social class segregation, would enrich the field. Furthermore, relationships among the various forms of segregation constitute important issues which need attention. Such a course would address structural issues beyond the field of human ecology.

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Economic Development and Status Change of the Aged¹

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There is an assumed negative relationship between the status of the aged and both the level and the rate of economic development in a society. It is hypothesized that status decline among the aged is caused by their relative displacement from high-status occupational groups. This change in the occupational distributions of age groups results from both a decline in economic activity among the aged and changes in age-specific occupational distributions among the economically active. In a sample of 30 countries, differences are examined in the age concentration of professional-technical and administrative-managerial occupational groups, by both level and rate of economic development. The paper concludes by noting that the evident decline in status of the aged with economic development may be a temporary phenomenon, subject to the dynamic relationship between economic development and the changing age structure of the population.

In his review of cross-cultural research on aging, Cowgill (1972, p. 8) states: "As a general principle, it may be affirmed that the status of the aged in a community is inversely proportional to the degree of modernization of the society." Similarly, Rosow (1965) and Hauser (1976, p. 81) argue that technological change and changes in occupational, residential, and familial structures have a negative effect on the status of the aged. The underlying premise is that social and economic change has such an effect because it represents a displacement process whereby the young assume the societal and economically powerful roles formerly held by the aged.

Although this hypothesis has been generally accepted by social gerontologists (but see Laslett [1976], Quadagno [1980], and Dowd [1980] for exceptions), the empirical evidence for the relationship is sparse.² I have tested

¹ This research note is the analytic section of a paper presented at the annual meeting of the American Sociological Association, New York, August 1980 (Cohn 1980). I acknowledge the useful comments on an earlier version of the paper by E. M. Beck, D. J. Colasanto, R. E. Cole, D. O. Cowgill, C. N. Halaby, and R. E. Klein. Responsibility for any errors remains mine. Requests for reprints should be sent to Richard M. Cohn, Center for Demography and Ecology, University of Wisconsin—Madison, Madison, Wisconsin 53706.

² The research of Palmore and his colleagues (Palmore and Whittington 1972; Palmore and Manton 1974) has become the standard citation for the relationship between modernization and the status of the aged (cf. Cowgill 1974, pp. 120–22; Maddox and Wiley 1976, p. 10; Neugarten and Hagestad 1976, p. 38; Streib 1976, p. 168). In these analyses, the

the hypothesis by examining the relationship between the occupational status of the aged and one aspect of modernization, economic development. Economic development is one of four dimensions of modernization included in Cowgill's (1974) descriptive model of the relationship between modernization and age-group status.³ Implicit in Cowgill's (1974) discussion is a general process of change in the relative position of age-group members in a hierarchical status system common to all members of society, for example, the occupational or income distributions.⁴ During the process of modernization, a group's status changes when its members become more or less visible in the upper levels of the hierarchy.

ECONOMIC DEVELOPMENT AND THE OCCUPATIONAL DISTRIBUTIONS OF AGE GROUPS

In the social gerontological literature, the relationship between economic development and the status of the aged has been described in both static and dynamic terms. For example, Cowgill (1972, 1974) posits a negative relationship between the *level* of development and the status of the aged. Rosow (1965), Cowgill and Holmes (1972), and Palmore and Manton (1974) discuss the relationship between *rate* of economic development and the status of the aged. While both higher levels and higher rates of economic development are hypothesized to be associated with a disproportionately small number of high-status occupational incumbents from older age groups, the two dimensions of economic development are hypothesized to be related to disproportionate age representation for different reasons.

First, societies with higher levels of economic development are expected to have a disproportionately small number of the aged in upper-level occupations because in these societies the aged have lower levels of economic activity than the young. Durand's (1975) analysis indicates a negative

measure used to indicate the relative status of the aged is a variant of the index of dissimilarity, a measure which indicates the degree of similarity between the distributions of the aged and nonaged on discrete measures of socioeconomic status, e.g., educational attainment, occupation, income classes. Palmore and his colleagues interpret relationships between this measure and an indicator of socioeconomic development, e.g., gross national product per capita, as indicating change in the status of the aged. However, Palmore's measure does not indicate relative status; the measure indicates only whether the aged and nonaged have similar distributions. For other criticisms of the index, see Johnson (1973) and Clemente and Summers (1973). The measure used in this analysis, the age-group concentration ratio, does indicate the relative concentration of age groups in upper-level occupations and is therefore a more appropriate indicator of relative status.

³ Cohn (1980) includes a review of Cowgill's (1974) complete model and corroborative empirical research.

⁴ Strictly speaking, it is improper to refer to a group's position in a common hierarchy, since individuals from all groups may be found at each level of the hierarchy. We conceive of a group's position as being the level at which there is a relatively greater proportional concentration of group members.

relationship between level of economic development and rates of economic activity among males over 45, and no relationship between level of development and activity rates among males 20-44. A society's level of economic development indicates its ability to afford the retirement of older workers. Only when there is a surplus of economic resources whose production is not dependent on the labor of the entire population can the aged be allowed to leave the labor force. Thus we expect differences in the relative concentration of age groups in upper-level occupations by level of economic development to be due to differences in economic activity rates among age groups.

Second, the rate of economic development is expected to influence the relative concentration of age groups in upper-level occupations. It is a central thesis of the economic development literature that the process of development produces a shift in the relative size of the agricultural, manufacturing, and service sectors of the economy, with a concomitant shift in the relative size of occupational groups. For example, Clark's (1940) analysis of the shift, with economic development, from emphasis on primary sector employment (agriculture, raw material exploitation) to secondary sector (manufacturing) and then to tertiary sector (services) employment is the classic description of Western historical experience.⁵ Kerr et al. (1964) argue that a consequence of development is technological change that restructures the occupational hierarchy to expand upper-middle-level occupations.

If a major effect of the process of economic development is the reshaping of the occupational hierarchy, it is important to consider which age groups are involved in this expansion of upper-level occupations. If each age group contributed an equal proportion to this growth, the visibility of different age groups at various levels of the hierarchy would remain unchanged. In this case, the hypothesized mechanism by which the rate of development affects the status of the aged—that is, the disproportionality of age groups in levels of the occupational hierarchy—would not be evident. However, if economic development distorts the age distribution of the occupational hierarchy, filling the expanded upper-level occupations with younger workers, then there is evidence of the hypothesized mechanism.

The rapid expansion of particular upper-level occupations is expected to produce a lower concentration of aged workers due to a difference between age groups in their ability to supply the requisite human capital, that is, workers with the latest specialized training. Even disregarding such exogenous factors as age discrimination by employers, we would expect them to

⁵ However, Singelmann's (1977) analysis suggests that the time period in which industrialization occurred affected the pattern of sector growth. Also, the applicability of these descriptions of industrial and occupational redistribution during development to countries currently in the early stages of development has been questioned. Myrdal (1968, p. 1113) argues that in South Asia a large tertiary sector is an indication of economic stagnation, a result of surplus labor in low capital-intensive economies.

find their newly and highly trained employees largely among younger workers. However, this effect may be expected to vary among occupations. To illustrate this, two occupational groups are distinguished in the analysis.

The first group, professional and technical workers, represents economic activities for which the amount and quality of training are particularly affected by the technological change implicit in modern economic growth. The changing technological skill requirements of this group are typically met through additional schooling. When it expands, this group is likely to recruit from among the young, since they have received more recent and more extensive education. The second occupational group, administrative and managerial workers, represents economic activity for which the increased demands of a developing society can more readily be filled through reallocation of the existing work force, including the aged. Compared with professional and technical workers, administrative and managerial workers require less school-specific training and are therefore more available through occupational mobility than initial job recruitment.⁶

This difference in recruitment channels for expansion between the two upper-level occupational groups implies a difference in the effect of the rate of economic development on the relative occupational distributions of age groups. The difference should be evident in the proportions of economically active workers in each of the two occupational groups. That is, we expect the rate of economic development to be related to a difference in the proportions of aged and younger workers in professional and technical occupations, but not in administrative and managerial occupations.

MEASURES OF RELATIVE OCCUPATIONAL DISTRIBUTIONS AND ECONOMIC DEVELOPMENT

In the following analysis, the measure of relative occupational distributions of age groups is an age-group concentration ratio (A_{ij}^k), the ratio of the proportions of particular age groups employed in an upper-level occupation. The age-group concentration ratio is defined as

$$A_{ij}^k = [(O_i^k/P_i)/(O_j^k/P_j)], \quad (1)$$

where k is occupation (professional-technical or administrative-managerial); O_i^k and O_j^k are the number of workers in the k th occupation from age group i and j , respectively; and P_i and P_j are the population sizes of age groups i and j , respectively.

These ratios are used to test two hypotheses. They are: (1) that the effect of the level of economic development on the relative occupational distributions of age groups is a result of development-level differences in age

⁶ See Kaufman and Spilerman (1982) for a discussion of how recruitment channels for different occupations can affect the age distribution of the occupations.

group economic activity rates, and (2) that the effect of the rate of economic development on age-group differences in the distributions of these occupations is a result of economic growth rate differences in age-specific occupational distributions among the economically active. In order to test these hypotheses, the age-group concentration ratio is decomposed in the following manner:

$$\begin{aligned} [(O_i^k/P_i)/(O_j^k/P_j)] &= [(O_i^k/E_i)/(O_j^k/E_j)][(E_i/P_i)/(E_j/P_j)] \\ [A_{ij}^k] &= [OC_{ij}^k][EC_{ij}^k], \end{aligned} \quad (2)$$

where E_i and E_j are the number of workers age i and age j who are economically active; component OC_{ij}^k is the occupational component, the ratio of the proportions of economically active workers who are in the particular occupation k ; component EC_{ij} is the economic activity component, the ratio of the proportions of the age groups who are economically active.

Age distributions for major occupational groups, as well as age-specific rates of economic activity, are available for 30 countries for the period 1962-71 (United Nations 1967, 1970, 1973, 1974). Although Palmore and Manton (1974, p. 207) discount the importance of sex differences in the proportion who are economically active by age, Durand (1975), using a continuous rate measure of economic activity, notes important interactions of sex, age, and level of economic development. Therefore, to simplify the analysis, age differences in male occupational distributions only are used.

Gross national product per capita (GNPCAP) is used as the measure of level of economic development. The rate of growth in gross national product per capita (Δ GNPCAP), during an approximate 15-year period prior to the year of the occupational and economic activity data, is used as the measure of rate of economic development. Data on both measures are taken from the two editions of the *World Handbook of Political and Social Indicators* (Russett et al. 1964; Taylor and Hudson 1972) and the *1977 Statistical Yearbook* (United Nations 1978).⁷

In the following analysis, three age groups are defined: prime age males, age 15-54 (group 1); older males, age 55-64 (group 2); and aged males, age 65 and over (group 3).⁸ Two age-group concentration ratios for each occupational group were constructed to compare aged males with prime

⁷ The correlation between GNPCAP and Δ GNPCAP in the sample is $-.002$. GNPCAP and Δ GNPCAP are net material product per capita for Hungary, Poland, and Yugoslavia and gross domestic product per capita for Algeria, Cambodia, Indonesia, Tanzania, and Thailand. These measures, though not directly comparable to the gross national product measures, appear to be reasonable surrogates to indicate relative level and rate of economic development. The standard caveat concerning the uneven quality of international socioeconomic data applies to this research. Taylor and Hudson (1972) provide a good discussion of the shortcomings of these data.

⁸ Owing to differences in reported age categories, prime age males are age 20-54 in Sweden and age 5-54 in Sri Lanka.

age males and with older males: A_{31} is the ratio of the proportion of males 65 and over and the proportion of males 15-54; A_{32} is the ratio of proportion of males 65 and over and the proportion of males 55-64.⁹

The technique used to decompose a ratio, and to analyze the effect of exogenous variables on the ratio through its components, is described in Duncan (1966) and Schuessler (1973). The multiplicative relationship of the ratio and its components (eq. [2]) is additive in logarithmic form:

$$\ln A_{ij}^k = \ln OC_{ij}^k + \ln EC_{ij}^k. \quad (3)$$

The effects of the level and rate of economic development on the age-group concentration ratio and its components are obtained from the following equations:

$$\ln A_{ij}^k = b_0 + b_1 \text{GNPCAP} + b_2 \Delta \text{GNPCAP} \quad (4)$$

$$\ln OC_{ij}^k = c_0 + c_1 \text{GNPCAP} + c_2 \Delta \text{GNPCAP} \quad (5)$$

$$\ln EC_{ij}^k = d_0 + d_1 \text{GNPCAP} + d_2 \Delta \text{GNPCAP}, \quad (6)$$

where $b_0 = c_0 + d_0$, $b_1 = c_1 + d_1$, and $b_2 = c_2 + d_2$.

Using the above notation, we may state formally hypotheses 1 and 2 and the evidence required to support them:

H1: There is a negative relationship between level of economic development and the proportional representation of the aged in upper-level occupational groups, that is, $b_1 < 0$.

H1a: This proportional underrepresentation of the aged in upper-level occupations is a result of lower rates of economic activity among the aged, that is, $d_1 < 0$.

H2: There is a negative relationship between rate of economic development and the proportional representation of the aged in upper-level occupational groups, that is, $b_2 < 0$.

H2a: This proportional underrepresentation of the aged in upper-level occupational groups at higher rates of economic development is a result of higher growth in those occupations requiring newly and highly trained younger workers. This hypothesis is supported by a negative relationship between rate of economic development and the occupational component (OC_{ij}^k) of the age-group concentration ratio, that is, $c_2 < 0$. Because of the above noted occupational differences in human capital requirements and recruitment practices, we expect to see this relationship for professional-technical occupations, but not for administrative-managerial occupations.

⁹ An analysis of the effects of level and rate of economic development on the ratio A_{31} , i.e., the ratio of the proportions of older and prime age males, indicates that neither measure is related to the relative concentrations of these age groups for both occupational groups. Therefore, only comparisons between the aged and the two younger groups are reported.

RESULTS OF THE ANALYSIS

To test the hypothesized relationships between level and rate of economic development and age-group concentration in upper-level occupations, equations (4) through (6) were estimated using ordinary least squares. The results are reported in table 1.

The upper portion of table 1 shows the effects of level of economic development on age-group differences in occupational distribution. In professional and technical occupations, the hypothesized negative relationship between level of economic development and age-group concentration ratio ($H1: b_1 < 0$) is evident. The source of this effect lies in the rate of economic activity. That is, level of development affects age-group concentration in these occupations by causing differences in the relative rates of economic activity between aged and younger males (EC_{21} and EC_{32}). This observation supports the specific hypothesis that the lower status of the aged at higher levels of economic development is a result of their lower rate of participation in the status-providing occupational hierarchy ($H1a: d_1 < 0$).

In administrative and managerial occupations, there are no statistically significant differences in age-group concentration ratios by level of economic development, that is, $H1$ is not supported.¹⁰ Here the level of development appears not to affect relative concentrations of aged and younger males, but this is because development has contrasting effects on the two components of the ratio. On one hand, the economic activity component (EC_{21} and EC_{32}) is negatively related to level of economic development ($GNPCAP$); as noted above, $H1a$ is supported. On the other hand, although it is not statistically significant, the relationship between $GNPCAP$ and the occupational component (OC_{21} and OC_{32}) is positive. Thus the positive effect of level of economic development on the occupational component of the ratio offsets the negative effect on the economic activity component. The result is a total effect for administrative-managerial occupations which does not support hypothesis $H1$.

The lower portion of table 1 reports the effects of rate of economic development on age-group differences in occupational distributions. For all occupations tested, the negative coefficient of $\Delta GNPCAP$ indicates that the faster the economic development, the greater the proportional underrepresentation of aged males compared with prime age males. In the comparison between age concentrations of aged and older males, the effect of the rate of economic development is marginally statistically significant ($t = 1.96$). Thus there is support for hypothesis $H2$, which proposed a negative effect of rapid economic development on the relative status of the aged.

¹⁰ Given the nonrandom basis of sample selection, i.e., all countries with complete data are included in the sample, the caveat concerning the formal inference of statistical significance tests is appropriate.

TABLE 1
EFFECTS OF LEVEL (GNPCAP) AND RATE (AGNPCAP) OF ECONOMIC DEVELOPMENT ON AGE-GROUP CONCENTRATIONS
IN PROFESSIONAL-TECHNICAL AND ADMINISTRATIVE-MANAGERIAL OCCUPATIONS

	PROFESSIONAL-TECHNICAL OCCUPATIONS			ADMINISTRATIVE-MANAGERIAL OCCUPATIONS		
	Group 3/Group 1 Coefficient	Group 3/Group 1 S.E.	Group 3/Group 2 Coefficient	Group 3/Group 1 Coefficient	Group 3/Group 1 S.E.	Group 3/Group 2 Coefficient
Effect of GNPCAP:						
Total effect (<i>b</i>)	-.033	.012*	-.036	-.013	.018	-.021
Via components:						
OC _{ij} (<i>c</i>)	.003	.011	.000	.023	.016	.015
EC _{ij} (<i>d</i>)	-.036	.008*	-.036	-.036	.008*	-.036
Effect of ΔGNPCAP:						
Total effect (<i>b</i>)	-1.256	.577*	-.971	-1.392	.862	-1.550
Via components:						
OC _{ij} (<i>c</i>)517*768	...
EC _{ij} (<i>d</i>)	-1.248	.366	-1.016	-1.384	.366	-1.595
Intercept (<i>b</i>)	-.558	...	-.263	-.521	...	-.594
R ²	.308361	.104272

NOTE.—Group 1 = prime age males (age 15–54); group 2 = older males (age 55–64); group 3 = aged males (age 65+). Metric coefficients and their standard errors for GNPCAP and AGNPCAP are multiplied by 100 and 1,000, respectively.
* |Coefficient/SE| > 2.

As hypothesized, the source of this effect is age-group differences in occupational distributions. That is, it is the occupational components (OC_{81} and OC_{82}) of the age-group concentration ratios which are negatively related to the rate of growth in gross national product per capita.

According to hypothesis H2a, we expect no effect of the rate of development on the proportional representation of age groups in administrative-managerial positions. This lack of effect is evident when the comparison is between aged and prime age males. However, when aged males are compared with older males, there is a growth-rate effect which is due to the negative relationship between growth rate and the occupational component of this ratio.¹¹

In summary, the results of the analysis support not only the basic hypotheses concerning the effects of level and rate of economic development on the relative concentration of the aged in upper-level occupations, but also the specific hypotheses about the influence of level and rate of economic development on age-group concentration. Both level and rate of economic development are negatively related to the relative concentration of the aged in high-status occupations. The source of the effect of level of development is age differences in rate of economic activity. The source of the effect of rate of economic development is age differences in occupational distributions, differences which are evident primarily when professional-technical occupations are distinguished from other occupations. This is consistent with the explanation that recruitment for this occupational group during rapid economic growth requires newly trained younger workers.

DISCUSSION

The analysis provides evidence for the hypothesized mechanism by which development lowers the status of the aged. Both level and rate of economic development are associated with a decline in the proportional representation of the aged in high-status occupational positions. However, an assumption concerning "contextual variables" deserves mention. That is, certain institutional aspects of the labor market may be significant, as suggested by the case of Japan. Japan has an institutional system of "permanent employment" in which workers stay with the same employer until retirement age (see Cole [1979] for a discussion of the origins of the system).

¹¹ The observed effect of $\Delta GNPCAP$ on A_{21} as disconfirming evidence of hypothesis H2a is of questionable importance. The measure of the rate of economic development ($\Delta GNPCAP$) covers the previous 15-year period. Cohort differences in educational attainment between aged and older males, the hypothesized cause of a growth rate effect, would have had their effect during a much earlier period. The more appropriate test of hypothesis H2a is the effect of $\Delta GNPCAP$ on A_{11} . The results of this test support the hypothesis.

Since it retains workers only until age 55, we would not expect the Japanese system to produce a particularly high economic activity component (EC_{ij}) among older and aged people. However, the system is expected to affect the age-group concentration ratio via the occupational component (OC_{ij}) by preventing the displacement of "tenured" older workers from high-status occupations. The permanent employment system, which has been operational since World War I (Cole 1979, p. 11), should have produced relatively similar occupational distributions among the three age groups.

An inspection of the residuals from equation (4) indicates that Japan is indeed an exception to the general pattern. Japan is more than one standard deviation above its predicted age-group concentration ratio for professional-technical occupations and more than two standard deviations above the corresponding predicted value for administrative-managerial occupations. Examination of the residuals from equations (5) and (6) indicates that the source of Japan's deviant position is the occupational component of the ratio. The proportion of aged workers in high-status occupations is closer to the proportions of prime age and older workers than one would predict, given Japan's rate and level of economic development. The relative level of economic activity among the aged in Japan, although slightly higher than one would predict, is not an important source of Japan's deviant position. It appears that the institution of permanent employment in Japan, while not ensuring employment of the aged, has had the effect of helping the aged maintain their status.¹²

Where no institutionalized employment security counteracts the displacement of aged workers, it appears that one of the negative consequences of economic development is loss of status among the aged. However, it is unlikely that this loss of status continues indefinitely with economic development. The present analysis indicates that the primary effect of level of economic development occurs via the economic activity component of the age-group concentration ratio. As Hauser (1976, p. 73) notes, by the end of this century there will be a slight decline (3%) in the total dependency ratio in the more developed countries. The stability of the total dependency ratio is a result of a predicted 11% decline in the youth dependency ratio, offsetting a 20% increase in the aged dependency ratio. This increase in the aged dependency ratio of developed countries can be expected to reduce

¹² Cole (personal communication) suggests that the deviant position of Japan results from Japan's idiosyncratic occupational classifications. Cole and Tominaga (1976) note that in Japan professional and technical occupations have grown relatively slowly during the nation's rapid postwar development. Cole and Tominaga (1976, p. 74) conclude that professional and technical activity is being carried out by an unusually large group of male clerical employees. Cole also notes that in Japan "professionalism" is less well developed as a distinct set of occupational activities. Thus, Cole argues that Japan's "outlier" status in this analysis is more a result of vagueness of occupational classification than it is the specific consequence of the "permanent employment" institution.

the economic surplus which allows the aged to have relatively lower rates of economic activity. If economic and demographic development reach a stage at which the economically inactive aged become such a burden that they are forced to resume economic activity, we may expect an end to their status decline. Thus there may exist a dynamic equilibrium among level of economic development, age structure of the population, and status of the aged. If so, the decline in status of the aged as explored here is only a temporary phenomenon.

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Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

SOME COMMENTS ON REFLECTION THEORY IN GRISWOLD'S STUDY OF AMERICAN NOVELS

To a reader working in the sociology of literature, it was interesting to come across Griswold's study of some of the social conditions that forged the American novel in the late 19th and early 20th centuries ("American Character and the American Novel: An Expansion of Reflection Theory in the Sociology of Literature," *AJS* 86 [January 1981]: 740-65). That she identified a major change in the U.S. copyright law as having a crucial effect on the development of American fiction was an important point. It was also interesting to compare the generalizations that can be made about the novel on the basis of a sample reading of all the novels published in the period with those based on selective or impressionistic readings characteristic of a more literary approach. Her findings and arguments open new and interesting possibilities in the attempt to come to a proper understanding of the nature of American literature and culture.

However, Griswold's paper presents a number of problems. She glosses over lack of evidence with speculative assertions. She minimizes the methodological problems that her sampling procedure creates. She establishes a quite gratuitous connection between her empirical study and her theoretical position. What I want to deal with here is the theoretical position she espouses—reflection theory—and its significance for the sociology of literature.

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Given the existing and serious criticisms of this theory (see Eagleton 1976b; Laslett 1976; Williams 1977), it was surprising to see Griswold try to resurrect reflection theory, proposing it as a useful means for understanding her findings and as the theoretical orientation for the sociology of literature. Presenting it first in an astonishingly crude form—"national literature reflects national character"—she advocates refining it. But this is a "straw man" argument: the majority of reflection theories are far more complex than she asserts, and some of them can be ingenious.

However, her argument for reflection theory is also unnecessary. If her demonstration of the role of changes in the copyright law was not enough to put the national character thesis to rest, her other point—that novels are bourgeois in nature, involved with some of the issues that their predominantly middle-class (and often upwardly mobile) readers like to read about—should have shown that it is the concerns of the middle class, not national character, that novels express.

By the end of the paper, she has not elaborated reflection theory at all; she has, rather, widened the scope or reference of "reflection" to include any condition or event that might be said to have influenced a novel. Turning a theory into a catchall term indicates conceptual confusion.

But there are more fundamental issues involved in Griswold's attempt to use reflection theory. The statement that literature reflects national character or society is vacuous: it is one of those statements that, by virtue of their generality, serve to orient rather than explain, and seldom even describe accurately. The statement recommends a distinctive approach to literature. It expresses a model and a methodology. The assumption that literature reflects society requires the sociologist to look to fiction as documentation of social life. It is one thing to illustrate sociological ideas by reference to fiction; this is a useful teaching device. But as an approach to the sociology of literature, it raises a multitude of problems. It is a lazy procedure, because it allows sociologists to find what they are looking for largely through an act of definition. Does Dickens's fiction "reflect" Victorian society? Well, this might or might not be wholly or partly true (even if we could agree on what "reflect" means), but—accuracy and semantics aside—what would we achieve in saying this? What is the point in asserting or proving such a statement, or in claiming that Orwell's fiction reflects British class structure or that Kafka's reflects bureaucracy? A pleasant enough pastime, but if there is to be a serious sociological understanding of literature, it cannot go in pursuit of the infinite number of "reflections" that literature can be said to provide; and when Griswold says that the differing depictions of race, social class, domestic settings, and national settings in European and American novels reflect the differences between Eu-

ropean and American national character, one is left aghast as much at the superficiality of thought as at the sheer pointlessness of the statement.

In discussing the empirical problems that reflection theory raises, the historical sociologist Peter Laslett (1976, p. 340) has rightly concluded that "the word, the concept, the process of *reflection* has obfuscated and misled: it ought to go." My claim is that the sociology of literature consigns itself to vacuity and inconsequentiality if it accepts the project that reflection theory recommends. But this is the very theory and project that Griswold would like to exhume, tart up, and put back on the streets. Rather than go in trivial and innocuous search of reflections, sociologists can proceed in other ways. They can recognize that literature is a productive process. The texts that writers create are the products of intellectual and physical labor, and that labor goes on, as it does for all of us, under specific social conditions. Novelists work under physical, financial, and ideological conditions, in relation to the requirements of book and magazine production, and within contexts of individual objectives, literary traditions, movements, schools, and genres—and copyright laws!

If sociologists accept these claims as accurate, it becomes interesting and important to inquire into how the conditions of literary labor affect the product (the text) and what versions of the world result. Dickens, it then appears, provides us with *one* version of Victorian life, not to be taken as a reflection of Victorian England (unless one wishes, following the metaphor, to consider also refractions, distortions, inversions, opacities, and deformations), but to be related to the circumstances of Dickens's life, and then compared with other versions of his times.

What is interesting to see is which versions of the world gain the widest currency: which sell, receive the greatest critical acclaim, are turned into movies, receive prizes, or create images that encapsulate abiding meanings. In other words, what are the most pervasive literary images of society at any given time, and, further, how do these images articulate with other institutions and processes? In particular, how do literary and other versions of the world support, transform, or oppose the prevailing images of economic and political life?

In this formulation, literature is part of the total cultural apparatus through which people live and, rightly or wrongly, make sense of their lives. Literature is no reflection of anything; it is ingrained in the very texture of life. Consequently, the sociology of literature leads inexorably to a sociology of culture which itself resists being separated—as a reflection, as an epiphenomenon—from the analysis of society as a whole. The sociologist's task, then, is to read those time-consuming texts and relate them to history and to culture. Counting has its place, but an immersion in the texts as well as in history and culture is the only way to retrieve a sociology of

literature from the simpleminded and intellectually regressive formulations of reflection theory. Along with others in the field, Griswold should know not only that there are serious, if not irrefutable, criticisms of reflection theory, but that there are alternative approaches as well (see Eagleton 1975, 1976a; Jameson 1979a, 1979b; Macherey 1978).

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MIRRORS AND TARTS AND PROVOCATIVE METAPHORS: A REPLY TO ALAN SEGAL

A novel, gentlemen, is a mirror carried along a highway. Sometimes it reflects to your view the azure of the sky, sometimes the mire of the puddles in the road. And the man who carries the mirror on his back will be accused by you of immorality! His mirror shows the mire and you blame the mirror! Blame, rather, the road in which the puddle lies, and still more the road inspector who lets the water stagnate and the puddle form. [Stendhal (1830) 1958, p. 363]

There goes Stendhal, putting that tart back on the streets again, to use Alan Segal's grating metaphor. Well, she marches in good company. Ever since Plato's artist began "turning a mirror round and round," the dominant mode of understanding the relationship of art to society has been to see art as reflecting some aspects of social reality (Abrams 1953; Auerbach [1946] 1968; Albrecht 1954; Peterson 1979). This reflection is imperfect, for circumstances of production influence which and whose views of what is real will be embodied in cultural works. It is this process of selective re-

lection, rooted in both productive processes and cultural systems of meaning, that the sociologist of culture attempts to untangle.

I am not sure whether Segal read beyond the title of my paper, for much of his comment seems based on a misapprehension that I am defending simple reflection theory of the national-character-produces-national-literature sort. But it is precisely this "astónishingly crude" oversimplification against which I am contending. Such factors as publishing circumstances, including copyright laws, the composition of the readership, and the characteristics of the authors who manage to get published all serve to distort any simple literary reflection, and it is the nature of that distortion that I have analyzed in the case of late 19th-century American novels. My findings, in brief, were: (1) American novels were not very different from European novels, thus throwing a challenge to the argument that literary distinctiveness reflects national social pathologies, and (2) most of the differences that do exist can be accounted for by copyright laws and other features of the publishing industry system of the time. But some distinctive properties of American novels, such as the propensity of American authors to locate their novels in institutions other than the home, did not yield to the latter type of explanation. Such findings suggest something unusual about the "American character" that requires further empirical investigation. Far from being "pointless," these American fictional oddities are provocative to the sociologist interested in finding literary variations that may, yes, reflect different cross-cultural social meanings.

But Segal has different fish to fry. He is interested in promoting a dialectical critical approach to literature, whereby culture and society are understood not as mutually influential but as two aspects of the same whole. Thus he cites Pierre Macherey, who has advocated that we look less at what literature says than at what it does not say for its social meaning, and Fredric Jameson, who argues that dialectical criticism can have no pre-established categories, for "to the degree that each work is the end result of a kind of inner logic or development, it evolves its own categories and dictates the specific terms of its own interpretation" (Jameson 1971, p. 333). This type of thinking allows for rich and evocative readings of individual texts, but it does not lend itself to the theory building based on proposition construction and empirical verification typically attempted by the sociologist, for the variability of categories is infinite and the controls are nonexistent. Thus the acceptance of such theoretically intriguing readings is more a matter of faith, based on a fixed notion of causality, than one of conviction through scientific persuasion.

This is why research using such models has been slim and disappointing. Instead, empirical investigation has recently concentrated on artists and art worlds, audiences, the production of culture within an industrial system,

or—in the case of my paper—an expanded reflection model which attempts to take all of these into account without losing sight of either the imperatives of cultural genres or the possibility that different national interests, ideas, or values may indeed be reflected in their respective literatures. Further comparative empirical work along these lines may allow complex reflection to move from metaphor to substantiated theory. If Segal is undertaking a similar effort of empirical verification of the dialectical model, I look forward to seeing the fruits of his research.

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THE OPERATIONAL APPROACH: JUST HOW NEW IS IT?

In Michel Verdon's article "Kinship, Marriage, and the Family: An Operational Approach" (*AJS* 86 [January 1981]: 796–818), we are treated to an interesting journey through the area that has been explored in comparative analysis of kinship, marriage, and the family. The goal was to lead us to an advance in our conceptualization; but, from my perspective, we are given instead a new set of problems and a stereotyping of the approaches of other writers on this topic.

First, we are presented with a gratuitous set of criticisms of the structural-functionalist approach and its teleological aspects and its paradigm. I agree with Goode and others that if one storms the fortress of structural functionalism, one will find that the castle is empty. Building up the importance of such labels and using them as weapons by stereotyping beliefs seem unnecessary. Verdon frequently uses the concepts of "purpose" and "teleology" and "function" in ways that are surely not in line with, for

example, Merton's usage or that of all people whom Verdon would label structural-functionalists.

More important is the author's attempt at a new conceptualization. What Verdon means by operational definition of a group seems to be the attempt to define a particular group in terms of a specific type of activity or function (p. 804). His basic criticism of other approaches seems to be that they use interpersonal behavior and norms as parts of the definition of groups. Such practices lead to "ontological variability" and to the mixing of psychology and culture with the purely social. Again I fear that, although this statement is interesting and arguable, it does not present accurately the state of affairs that exists in comparative definitions. Since I know my own definitions best and since Verdon referred to them (p. 799, n. 3), I will use them to illustrate my points.

Verdon defines a conjugal group as "formed around two distinct sets of activities, namely, sex and reproduction, with the emphasis on the latter" (p. 811). This use of more than one activity seems to violate Verdon's own desire to use only one activity and to avoid terms like emphasis or degree of stress on one activity versus another. But let us set that aside: Verdon goes on to assert that marriage is the criterion of membership in conjugal groups formed around the reproductive activities of one woman with the conditions that the child not disrupt the group and that the child be given membership in religious and political groups of the mother or other group members. My definition of marriage is in line with the views of Malinowski, Gough, Davis, and Radcliffe-Brown and asserts that marriage is "a socially accepted union of individuals in husband and wife roles with the key function of legitimation of parenthood" (1965, p. 452; 1980, p. 50). My definition, like Verdon's, stresses the reproductive outcome. The fulfillment of reproductive goals is the key aspect of husband and wife roles as I define them. The stress on the legitimation function points to the acceptability of that outcome. The notion of legitimation also fits with Verdon's stress on the membership of the child in religious and political groups and on the lack of disruption of the group caused by the birth. Thus I fail to see what of significance has been added by this new approach and why the similarity to my definition and those of others was not explicitly noted by Verdon.

It is, perhaps, in the definition of the family that some capriciousness and inconsistency in Verdon's approach are most apparent. The family is arbitrarily defined as the conjugal group plus the child born to it. Then it is asserted that all of these individuals do not partake of any single activity, and, therefore, the family has no function. One can ask whether all the members must perform an activity for the family group to be considered involved in that activity. In marriage, the reproductive process may belong much more to the females than to others in the conjugal group, but that

does not remove the conjugal group from having shared activity. Also, that group exists during times of no reproduction. If we take the newborn as the center of the family group—which is reasonable since the child establishes the family by Verdon's definition—then we can see whether the child and anyone else engage in a particular activity which would establish them as a group. My research has led me to the judgment that nurturant socialization of the child by one or more others is universally present. In this way the family has a function and we avoid negating that fact, as Verdon did when he simply looked for a combined activity of the conjugal group and the child. The others may be just the mother and her brother, or the father and others too may be involved, but this approach does define the family in terms of a key activity and thus exemplifies Verdon's "operational approach." I believe I did just this in my 1965 definition, which read: "The family is a small kinship-structured group with the key function of nurturant socialization of the newborn" (p. 449). I use "kinship-structured group" to mean a descent group; this usage too fits Verdon's approach. Thus, my major question to Verdon is, How does my approach (which was cited as one of the many that were deficient) differ from the one recommended in the article? Were my approach and others carefully analyzed? Perhaps our difficulty in settling on a cross-cultural set of definitions lies in our communication and emotions more than in our science.

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THE OPERATIONAL APPROACH IS AS NEW AS NONTELEOLOGICAL DEFINITIONS

In answer to Reiss, I would like to emphasize that I was careful to suggest in my article that *all* (not only the structural-functionalist) current models viewed groups (1) as usually involved in more than one type of activity, and (2) as always implying a set of norms regulating interpersonal behavior (or social relationships). In the article I mentioned, Reiss (1965) depicts the family as a *structure* performing a *functional* prerequisite, adding that it forms a *system* because it is an "institution," that is, "an

integrated set of *norms* and *relationships* which are socially defined. . . ." (italics mine; Reiss [1965], p. 448). We find there the main ingredients of the models mentioned above, save model 1. Indeed, Reiss defines the family as having only one functional prerequisite. Another important point: in both publications mentioned (1965, 1980), Reiss merges conceptually "family" and "residential group"; stem or extended "families," for instance, are in fact stem or extended "residential groups" (Verdon 1979*b*), and "matrifocal families" are female-headed residential groups of specific composition. It is to clear up this kind of conceptual confusion that I have proposed an operational framework (Verdon 1979*a*, 1980).

The same conceptual confusion leads Reiss to define the family (translating his definition into my terminology) as a group ("structure," to him) involved in the activity ("function," to him) of nurturant socialization and recruiting primary kin who share some kind of right of possession ("descent," to him). Two minor points: (1) the family is not a kinship-structured group because husband and wife are not kin; and (2) his use of "descent" does not correspond to anything in the social anthropological literature. Major point: the analytical value of the definition. Among the Abutia Ewe, the wife accompanies her husband on his labor migrations (which last over 20 years) but, at her first pregnancy, she is sent back to her native village, where she joins a residential group headed by her father, her brother, or her mother. The father of her newborn may not show up for two or three years, and, if he does, his visits rarely exceed two or three weeks in a year. The mother will socialize her child with the nurturant help of kith and kin, including friends and more distant relatives living in neighboring houses. Also, many women leave their children in fosterage. Hence: (1) What psychological test will enable us to measure the nurturance which defines membership of the group when a vast number of individuals are involved? (2) Given that some miraculous test would enable us to delineate the "group of nurturant socialization," it would reveal a baffling diversity in composition within the same small village. (3) Its composition would also change with great frequency, as Abutia adults are extremely mobile. But, above all, (4) *This definition would exclude fathers on labor migrations and mothers whose children are fostered*, although we intuitively see them as part of the family. The position is untenable. Reiss and I thus differ on this major point, namely, that he sees the family as involved in one activity only, whereas I see it involved in none. I have shown the problems his definition creates.

Now, what is the analytical value of his definition of marriage, which implies (1) the legitimization of parenthood and hence of children, and (2) the assumption of socially accepted roles of husband and wife? Note also that his definition of marriage fills one paragraph in an article devoted

entirely to the family, suggesting strongly that marriage is defined with the family as its purpose (1965, p. 452). This he stresses further in his comment by writing that "the fulfillment of reproductive *goals* [italics mine] is the key aspect of husband and wife roles as I define them." According to Reiss, people get married in order to have children; the definition is explicitly teleological.

First, there are scores of societies in which parenthood needs no legitimation because children always belong to the group of either their genitor or genitrix, whatever the link between them (the Abutia Ewe represent an ideal case). In the same society (Abutia Ewe), the "socially accepted union of individuals in husband and wife roles" (Reiss 1965, p. 452) disappeared 30 years ago, together with the traditional marriage ceremony. Now, men and women do not get together for reproductive goals but for the enjoyment of sex. Many of these sexual unions result in pregnancies which, if they cannot be aborted, have to be made public. At this juncture, the genitor might accept the paternity and wish to maintain the union, which will then outlast the birth of the child (and possibly of many children). But nothing special happens beyond the fact that the pregnancy publicizes the sexual involvement. Since there was never a "socially accepted union of individuals in husband and wife roles with the key function of legitimation of parenthood" in those many instances which simply started as sexual affairs, the quasi totality of heterosexual unions outlasting the birth of a child in Abutia would have to be excluded from the category of married people. As a field-worker, however, I need the conceptual tools to classify as married those numerous individuals who remain together after the birth of a child but would not be regarded as married according to Reiss's definition. Similarly, many middle-class Americans decide to cohabit without wanting children or being registered as married in either the law or the church. In the event of an accidental conception (or a belated decision to have a child), the couple may decide not to register their union in court or in church. If their union persists beyond the birth of the child, I would regard them as married, although they were not united in the socially accepted roles of husband and wife. Furthermore, Reiss's introduction of "reproductive goals" makes it difficult to deal with individuals who do get married but do not want children! Also, How shall we define the role of the Nayar husband, who has neither rights nor duties? And shall we treat as nonmarried those husbands who do not perform their roles as they are "socially accepted"? The definition of marriage in terms of goals and roles is thus of doubtful value.

In my definition of marriage, I reverse the order of Reiss's reasoning. Instead of starting with the purposive behavior of individuals and the manner in which society endorses it, I start with constituted groups or

sets and focus, not on a cause, but on an *effect*, namely, what the birth of a child does to the partnership responsible for the birth of that child. If the birth disrupts or transforms the relationship (because of extrinsic reasons, i.e., social pressure), there was no marriage; if it does not, I assume that the partnership was formed around the potential reproduction of a woman, and that the set thus formed was a conjugal set. I then postulate that the element used for the formation of that set (its criterion of membership, so to speak) had this one effect that the birth of a child did not disrupt the set, and I call this element marriage. *But note:* this effect can occur only if one precondition is fulfilled, namely, that the child born is placed socially with either the genitrix or her partner(s). In many societies this social placement is automatic (independent of marriage), but in others it is not. In the latter instances, whatever serves to form conjugal sets will, by definition, serve for social placement. This I regard as a non-teleological definition of marriage; as a conceptual tool, it enables me to include all the cases which Reiss's definition embraces plus many others that his definition excludes but which should nonetheless be regarded as marriages, such as the Abutia, Nayar, or Caribbean unions. In line with generations of theorists, furthermore, I define the family as "the conjugal set plus the offspring born to them," claiming, however, that this reproductive set is not engaged in any activity (reproduction being internal to women).

Reiss also points out (1) that the family remains in times of no reproduction, and (2) that individuals in conjugal groups are not equally involved in reproduction. First, the family also remains in times of "no nurturant socialization," a fact more difficult to understand. Indeed, if the family as a biological set remains after reproduction, it is because the fact of having been reproduced does not stop since it defines parents and children and genealogical connections (i.e., kinship). With respect to the second point, I did define the family as a biological set because I consider reproduction to be a process internal to women and therefore not an activity to be engaged in. Therefore I should have written of conjugal sets (I suggest "set" but would welcome any alternative more appropriate) and not conjugal groups. Despite this terminological inconsistency, which Reiss kindly brought to my attention, the argument remains the same. Finally, I would like to stress that operationalism is not an attempt to find an activity for every group but a means of defining our concepts with less ambiguity by using activities and ownership as points of reference to distinguish among groups, corporations, categories, crowds, aggregated groups, and so on.

I had thus carefully assessed Reiss's definition before writing my article, and I still contend that our two approaches diverge significantly. There-

fore I would be tempted to return the accusation and suggest that Reiss's mode of communication and emotions may have stood in the way of a clear appreciation of the differences between our definitions.

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Review Essay: Who Controls the News?

The Powers That Be. By David Halberstam. New York: Dell, 1979. Pp. 1071. \$3.50 (paper).

Deciding What's News. By Herbert Gans. New York: Pantheon Books, 1979. Pp. xvii+393. \$12.95 (cloth). New York: Vintage Books, 1980. Pp. xvii+393. \$5.95 (paper).

Making News: A Study in the Construction of Reality. By Gaye Tuchman. New York: Free Press, 1978. Pp. xi+244. \$12.95 (cloth); \$8.95 (paper).

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How are we to understand the forces that shape contemporary news? Media analysts agree that there is no simple answer to this question but differ on which among a complex of influences is primary. Three explanations are currently popular. According to the first, primary control rests with the working press. Despite other influences, the news is shaped first and foremost by the values, judgments, and news-gathering routines and conventions of front-line journalists as they transform information selected from the external world into news stories. A second explanation often locates primary control in the gatekeeping apparatus of news organizations, where tiers of subeditors, writers, and deskmen process incoming information to fit organizational standards and interests. An alternative form of this explanation emphasizes the role that powerful owners, publishers, and chief executives play in defining which topics and persons will be given attention in the news and which ignored. A third explanation locates the source of primary power in forces external to the media per se. This explanation focuses on the legal, political, and economic constraints within which the modern news media operate. In this view, sources control the news. By means of both direct and indirect pressure, plus an elaborate information-gathering machinery of their own, powerful political and economic interests manage the flow of information to the media and, in so doing, control the attention or lack of attention they themselves get.

All three books under review here are concerned either explicitly or implicitly with these dynamics, and in each one we get a somewhat different interpretation of the locus of ultimate control. In *The Powers That Be*, journalist David Halberstam shows us the view from the very top: the news system of elite sources, elite media executives, and the elite press corps described, appropriately, by a member of one of those elites. Halberstam is

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concerned with the evolution of the modern news organization and takes us on a historical survey of *Time* magazine, "CBS News," the *Washington Post*, and the *Los Angeles Times*—four undisputed giants of the contemporary news scene. In an account of epic proportions, we come face to face with the men who founded them, guided their corporate growth, shaped them editorially, and reported for them, and whose careers in public life were both developed and demolished by them. Although at specific times both sources and journalists become the dominant force, real power over the news resides most often for Halberstam with the modern media barons.

In *Deciding What's News*, Herbert Gans also focuses on major league journalism, as we revisit *Time* and CBS and are introduced to *Newsweek* (covered by Halberstam only after its acquisition by the *Post*) and to the "NBC Nightly News" operation. Gans is concerned with how journalists select and report domestic news, with what gets left out, and with the kinds of people journalists are. Although we are not told much about the latter, we do learn a great deal about the nature of national news media and the dynamics of news selection and preparation. Here again is an inside account of the top tier of the news industry, though this time observed by an outsider and from a vantage point a rung or two lower than Halberstam's. Even so, Gans ends up feeling that he has written not so much about journalists as about the "political and economic underpinnings" of the dominant culture in America (p. xv). For Gans, news is influenced by many considerations—sources, audiences, and organizational standards—but in the final analysis he views source power as primary. In the perpetual tug-of-war, sources outmaneuver the media apparatus and the journalists more often than vice versa, and as suppliers of the raw material they have more impact on news content than do audiences.

In *Making News*, Gaye Tuchman analyzes more ordinary levels of the news system and describes news routines at a television network affiliate, at an eastern-seaboard metropolitan daily, among reporters covering the Women's Movement, and at the City Hall Press Room in New York. In contrast to Gans, who views the role of journalists primarily as "summarizing, refining, and altering what becomes available to them from sources" (p. 80), Tuchman accords newswriters a much more active and influential role in shaping the news. For Tuchman, making news is the construction of reality itself, and it is front-line journalists who make the initial constructions. Newswriters, she argues, are a group with more power than most to "create, impose and reproduce social meanings—to construct social reality" (p. 208).

Despite these differences of emphasis—as well as sharp differences in style and theoretical perspective—these three treatments of the news media have much in common. In all three, we find the working journalist beleaguered by cross-pressures; all three demonstrate that the end product of newswork bears only some resemblance to what actually occurs in social life; and in all three we are either told, or can conclude for ourselves, that the main social consequence of our news system is to reinforce the estab-

lished social order. All three studies deal with four separate news operations, a fact which may be only coincidental but which explains why all three books are the products of long-term inquiry. Field research is a time-consuming enterprise. Halberstam worked full time for five years researching and writing, and he presents a list of over 500 identified sources (leaving others unnamed) which includes virtually every prominent name in American journalism from the forties through 1978. Gans's data were collected by means of participant observation and interviews during two field periods spanning a decade, and he also presents a content analysis of the news as well as data on news audiences. Tuchman's fieldwork, which also involved participant observation and interviewing, consumed a 10-year period between the mid-sixties and mid-seventies. Thus all three authors have spent enormous amounts of time observing and talking to news people, and none can be faulted for a superficial or hastily considered analysis of the news process.

Overt and Covert Editorial Control

"The news may be too important to leave to the journalists alone," Gans concludes (p. 322), as he advocates review panels on which citizens and journalists share responsibility for news selection. That this premise may be commonly accepted among media executives is hinted at repeatedly in Halberstam's account of professional life at the top of the system. According to him, the prototype of editorial control by executive order was *Time* under Henry Luce. Who controlled the news was never an issue for Luce, who is said to have once remarked, "From the first page to the last . . . whatever comes out has to reflect my view, and that's the way it is" (p. 91). Luce vested absolute control with personally selected top editors, and staffers either toed the line or left—and they sometimes left. But such control was made possible by a division of labor almost unique in American journalism, in which all reports filed by field correspondents were completely rewritten by staffers at the home office. In this structure, there is little chance that the news will fail to reflect company interests. There is also little chance to avoid strife between reporters and editors or low staff morale, conditions Gans noted at *Time* long after Luce was dead. In addition to heavy-handed editing, control was also enforced by "prekilling" stories, a practice not unlike "dead-horse" typesetting, in which editors order up stories they, and their reporters, know will never be printed.

Time is not the only example of raw editorial power in modern journalism. It was not long after Edward R. Murrow's historic telecast on Senator McCarthy that the CBS corporate elite, which is to say William Paley and Frank Stanton, realized that too much authority had been placed in the hands of the reportorial staff. That telecast, Halberstam feels, represented a turning point in broadcast journalism, for it guaranteed that neither Murrow nor any other journalist would ever again have that much power and autonomy. From then on, "the network itself would control the

journalists, the shows, the hours, and, to a considerable degree, the subjects" (p. 209).

Nor was control ever much in doubt at the *Los Angeles Times*. From General Otis through two generations of Chandlers, the *Times* functioned solely as an instrument of family aggrandizement. "Its job was not so much printing information as withholding it; it would sanitize the large, significant political and economic developments within the city, boost the ones the Chandlers wanted boosted, while printing minor stories about crime" (p. 162). Halberstam does not comment on how this style of news control affected journalists, but he does report that newsroom trouble surfaced at the *Times* in the early sixties when, under Otis Chandler, the newspaper began to upgrade its standards in a bid for professional respectability. Halberstam describes how difficult it was for Chandler, who had bolstered his reportorial staff with a cadre of well-known eastern correspondents, to change the interior of the paper's editing bureaucracy—a difficulty resulting in an unusually high level of tension between the field staff and the home office. This is one of the few places where Halberstam hints that newsprocessing operations, like other bureaucracies, may develop a life of their own which can successfully resist dictates for change introduced by top management.

Neither Tuchman nor Gans reports many instances of overt editorial control of this type. Gans notes that though "the Paleys and Luces can intervene in the news whenever they choose to, they do so only rarely" (p. 84). And Tuchman, who began her study expecting to find frequent conflicts between reporters and management, found many fewer such incidents than she had anticipated. In fact, Tuchman portrays relationships between newswriters and editors as more symbiotic than adversarial. She notes a close correspondence between journalistic professionalism and the interests of news organizations of such a nature that "professional practices serve organizational needs" (p. 51).

For Tuchman, and for Gans too, organizational control over newswork is a much more subtle process than that described by Halberstam, a process accomplished by the adherence of both sets of actors to the canons of modern professional journalism. Professionalism for a journalist, Tuchman remarks, consists precisely in "knowing how to get a story that meets organizational needs and standards" (p. 66). And the knowledge in question consists of all the conventions which allow journalists to claim that they have been "objective." When journalists verify factual assertions, seek out supporting evidence, attribute all evaluative statements to sources, and rely on official sources, information acquires a "web of facticity" which satisfies both professional obligations and organizational needs. Tuchman does not view these tactics as constraints; instead they are rituals which newswriters learn to observe in order to fulfill their own goals. By adhering to the unusually strong verification requirement of checking all alleged facts against two independent sources, for example, Woodward and Bernstein were able to continue their investigation of the Watergate conspiracy and, in so doing, to

construct the unlikely and highly risky news theme of "president-as-crook."

Gans views the power of journalists as more circumscribed than this. Although he notes, as Tuchman does, that journalists must combine their own news judgment with "what they think will please their editors" (p. 102), he implies that they very seldom have agendas of their own since the national news media seldom attract journalists who hold strong views. More than Tuchman, Gans feels that journalists impose a kind of self-censorship on their work. Whereas Tuchman highlights the tactics that will get information through the gatekeepers, Gans emphasizes the negative—the taboos and constraints which must be observed in preparing news stories. For example, he describes broadcast journalists as being very defensive about audiences: audiences must be protected from unnecessary personal upset, from content that would cause them to panic, from content that would be offensive to taste. Although larger audiences mean larger corporate profits, broadcast journalists fear audience enlargement as a potential threat to their already precarious professional autonomy.

Sociology versus Journalism

Gans characterizes journalism as the "strongest remaining bastion of logical positivism in America" (p. 184), which is not an unfair representation of the main thrust of Halberstam's study. *The Powers That Be* is extremely good reading. It builds a lucid and engaging narrative about important people and important events and is the only book of the three that will keep readers up after bedtime. It is also crammed with anecdotes available only to insiders, the kind of anecdotes that academics will use to enliven their lectures on politics and television or on media organization. (For example, how many of you knew that during their first television debate, when Nixon began to perspire profusely, Kennedy's video manager in the control booth was unsuccessfully urging that the cameras be focused more frequently on Nixon? Or that while CBS correspondents were filing devastating reports on Lyndon Johnson's foreign policies, Frank Stanton was personally coaching Johnson on how to present himself better on television?) *The Powers That Be* satisfies all major journalistic criteria for story selection and qualifies simultaneously as "hard news" and "soft news."

Yet for all the information and inside dope, one is left with precious few insights about how modern news organizations have changed or about contemporary newswork. Halberstam identifies two main shifts: the greatly enlarged power of the presidency with the advent of television and the greatly increased strength (and bureaucratization) of news organizations as they have become corporate conglomerates. Yet these themes are not developed very systematically and are buried by a deluge of information about key actors. In effect, Halberstam individualizes social process. No organizing premise or conceptualization is evident; no explicit criteria are invoked to govern the selection of data. Even the chapters and subdivisions within chapters are left untitled.

The Powers That Be presents an extremely selective view of the news process. What we obtain is not only an overly dramatic image of newswork itself (we meet no copy editors, or reporters who cover fires or who write obituaries) but probably an unrepresentative portrait of the day-to-day routines of White House correspondents as well. For every Watergate or My Lai story, members of the Washington press corps surely must endure months of tedious press conferences and routine interviews. Yet we get very little flavor of this aspect of newswork from the narrative. In short, the selection criteria Halberstam has employed are those which highlight the idiosyncratic rather than the repetitive and the dramatic rather than the mundane. These criteria, after all, are what make a good news story. For Halberstam, the evolution of the modern news organization is captured by focusing on events and actions that stand out from the ordinary: he does not document changing patterns of regularized news activity.

Good journalism will necessarily portray social process differently than will good social science. And it is this feature of the news system that both Gans and Tuchman find disturbing. For Gans, both sociology and journalism are empirical disciplines, but sociology has a superior methodology, and he advocates greater use of its methods in covering the news.

The Bias of News

Using content analysis, Gans shows that domestic news concentrates on knowns rather than unknowns, with substantial coverage of presidents; presidential candidates; and federal, state, and local officials. If ordinary people get into the news, it is usually as protesters, law breakers, or victims. Gans distinguishes "social disorder" news from "moral disorder" news and reports that while stories of the latter type sometimes focus on the transgressions of the elite, social disorder news involves ordinary citizens—usually the poor, minorities, or youth. By equating social order with legitimized authority, the news reinforces the "domain assumptions" of American society: that democracy is altruistic, that capitalism is responsible, and that tradition is to be revered. By reporting primarily about persons instead of social processes, the news also reinforces individualism. These themes are latent and not deliberate, however, and Gans claims that national news is not ideological—or at least not blatantly so. It does contain a "paraideology." Yet Gans is quite clear about whose social order is being reinforced: "The news supports the social order of public, business and professional, upper-middle-class, middle-aged, and white male sectors of society" (p. 61).

Tuchman makes a similar assessment. News "confirms the legitimacy of the state" (p. 210) and may be said to express a "middle-class ideology" (p. 177). But more seriously, it obfuscates instead of amplifying social reality. Both Gans and Tuchman note that the news overlooks the intimate connection between the state and corporate capitalism and in so doing legitimates contemporary social arrangements. For Gans, journalists simply do not ask the "right" questions, which are "why corporations have so much

power and citizens so little; why unemployment, inflation and poverty remain; and why women and racial minorities continue to occupy an inferior position" (p. 277). For Tuchman, the problem is more deeply rooted.

Though claiming to have discredited the traditional social structural perspective on news (p. 183), Tuchman develops a persuasive, yet suspiciously structural, explanation for the bias of news. Her explanation focuses on the centralized structure of news gathering which has evolved in American journalism. The "news net," as she terms it, solves fundamental problems of social organization for actors engaged in surveillance of the environment on a continuing basis. It provides regularized locations for finding news. Today's news net, which is "intended for big fish" (p. 21), identifies legitimized institutions as collection points because it is there that important news sources can be expected to be found. Moreover, the assignment of media personnel to a specific location reinforces the legitimacy of that location as a source of important news. News organizations and news sources need one another, and the news net is eventually locked into place as a mutually beneficial structural arrangement.

Newswork is organized to manage time as well as space, and it is in response to time constraints that journalists play an active role in shaping the news. This shaping is accomplished through the categories journalists use to divide up the universe of news events, categories that have become part of the occupational culture. Journalists "typify" potential news stories as "hard" or "soft," "spot news," "breaking news," or "continuing news," all of which have temporal implications. When news stories can be anticipated, they can be "prescheduled," a practice that makes newswork manageable and efficient. It is in the application of these "typifications," Tuchman claims, that newswriters actively impose and construct social reality.

Although this analysis is both insightful and persuasive, Tuchman may underestimate the impact that sources have on the "typification" process. Most of the press agents employed by powerful sources are former newswriters, and they, too, understand the norms and constraints under which the media operate. As suppliers of information, moreover, they are in a position to manipulate time to their own advantage—as, of course, they do. Tuchman plays down the "news management" dimension of contemporary journalism and, in so doing, understates the role that sources play in shaping the news. To accord sole responsibility for the news to the definitions of events by journalists is to ignore one of the most important reasons that the news continues to celebrate the status quo. Both Gans and Halberstam discuss input from sources and show that it has important effects.

Can our news system be changed? Given the embeddedness of the news net and the investment in it by both sources and news organizations, one might well be doubtful. In a concluding chapter on news policy, Gans calls for major reforms and discusses alternative strategies for change. He feels that the news must become "multiperspectival," that is, it must provide information to serve the interests of various population groups and not just the political and economic elite. As it is, he argues, domestic news ignores

the poor, minorities, younger and older people, and rural populations. Multiperspectival news would provide a "bottom-up" perspective. After presenting a number of unrealistic alternatives, such as dividing up the news audience BBC-style into segments based on level of sophistication and then allocating responsibility for each segment to one of the existing networks, Gans settles on a more decentralized plan. His main recommendation calls for a federally sponsored National Endowment for News, which would fund projects designed to serve the news needs of neglected groups. We should probably not count on that happening for some time.

Review Essay: The Discovery of Objectivity¹

Discovering the News: A Social History of American Newspapers. By Michael Schudson. New York: Basic Books, 1978. Pp. xi+228. \$11.95.

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American sociologists have shown relatively little interest in the news press or the mass media generally. There are glittering and well-worn exceptions, to be sure, and there have been a handful of significant books in the past few years. But sociology has not accorded the social institutions of the mass media the same systematic and persistent attention lavished on small groups, formal organizations, or demographic change. This is as true for diachronic as for synchronic studies. Beyond Robert Park's thoughts on the natural history of the newspaper, Alfred McClung Lee's 1937 history of the press, or occasional studies such as Morris Janowitz's of the community press, there is little historical sociology of the mass media. What sociology exists has largely followed one or another path out of Paul Lazarsfeld and concentrated less on the mass media than on the social psychology of opinion and behavior change. While introductory sociology texts often include a chapter on the mass media, it is more often than not the most perfunctory, cursory, and least informed of the chapters.

That this should be true is anomalous. First, the mass media, by any measure, are central institutions in modern society, and only an informed historical sociology of the media will capture and explain this fact. Second, systematic American sociology and modern journalism were, to a certain extent, twin born. When John Dewey, George Herbert Mead, Charles Cooley, and Robert Park were joined in Ann Arbor in the 1880s by a curious itinerant journalist, Franklin Ford, modern sociology and the desire for a scientific, objective journalism began an implicit and reflexive development.

Michael Schudson's *Discovering the News* is among the few recent works in American sociology to continue the tradition first established in Ann Arbor and attempt an informed sociological history of the newspaper. It is a very good history indeed, though it does not have much competition. The standard histories of journalism, which have come largely from journalism schools, are consistently whiggish in tone. They account for the development of the press against an assumed background of the expansion of freedom and knowledge. The story of the press becomes identified with the

¹ Requests for reprints should be sent to James W. Carey, College of Communications, 119 Gregory Hall, University of Illinois, Urbana-Champaign, Urbana, Illinois 61801.

growth of human understanding and the enlargement of the rights and freedom of the First Amendment. This story has become a cliché where large social forces—literacy, industrialization, urbanization—are the unquestioned, objectified bearers of an essentially progressive and beneficent history. As a result the central sociological questions concerning the press—the emergence of the appetite for news itself, the shifting definitions of what constitutes news, the class and status conflicts which underlie the development of the institution, the changing composition of the audience, the interconnection between journalistic performance and political power, and the professionalization of the press—these questions never get asked seriously. Schudson is forced to break through the crust of accepted explanations in order to get some of these questions on the floor. However, in view of the accepted history of the press, the title of the book itself becomes misleading in at least two ways.

First, Schudson is not attempting to discover the news but, rather, one particular impulse in journalism—the impulse toward objectivity—and the type of news report that flows out of this impulse. He is not attempting to account for news as a genre but for one particular variant of the form. In the 18th century a new hunger for experience arose, a hunger which dispensed with the traditional, epic, and heroic in favor of common, useful, unique, individual, and new knowledge—in a word, news. This general appetite is the overriding fact and to discover the news is first to discover the roots and nature of this appetite. The second problem is to account for the changing shape of this appetite—the varying styles of reports from the hoax and the essay, the commercial report and the advertisement, to the human interest story and the objective report that have been the varying embodiments of it. Schudson's book draws from a larger investigation of the norm of objectivity in law and journalism. What remains unique to it is the history of the ideal of objective news: an attempt to account for the changing nature of news in relation to the ideals of objectivity.

The second misleading aspect of the title is that the book is not a social history of American newspapers. It is limited in time to the period between the 1830s and today. It is also pretty much limited to four "moments" in that period: the rise of the "penny press" in the 1830s, the professionalization of journalism in the 1890s, the twin growth of skepticism and objectivity in the years following World War I, and the bitter conflicts over the press in the 1960s and early 1970s. The history is also limited in space. Schudson concentrates pretty exclusively on the New York newspaper; and, however justified by the almost archetypal significance of New York journalism, this is less than the whole story. The New York press developed through complex interactions with newspapers and journalistic traditions in other regions, traditions which it tried to shape and with which it was in sharp and important conflict.

Thus, while casting sideward glances at other regions and other forms of news, Schudson concentrates on the norm of objectivity as seen through the New York newspaper. Prior to the 1830s American newspapers were not

objective but partisan, reflecting the ideological interests of political parties. The question Schudson poses is this: Why did the idea of news, once invented, turn into the notion of a nonpartisan, strictly factual news? He locates the origins of the idea and practice in the penny press which progressively dominated American journalism after 1835. Many scholars have tried to explain the origins of the penny press and its characteristic style by reference to the spread of literacy, or to the telegraph and its effect on the language, or, least satisfactorily, to Robert Park's notion of the natural history of the newspaper. Schudson instead locates the first modern newspaper in the egalitarianism of Jacksonian democracy. He assumes, and correctly, that the ideals of journalism are always consonant with the culture of the dominant class. He therefore locates the emergence of the penny press in the conflict between an emerging mercantile middle class and a declining gentry. The modern newspaper, then, was created by the sponsors of a democratic, market society. Democratic, in his usage, refers to a political culture of widespread participation, an economic culture in which the ordinary activities of buying and selling become pervasive features of consciousness and of a peculiar everyday culture, one dominated by interest in the odd, exotic, and trivial. The penny press established the newspaper as a consumer good. It reflected, therefore, not solely the world of commerce and politics but social life generally. Penny newspapers were the first to organize journalism around the beat and to actively search out news. This journalism served an audience, not a public, and it ratified any event, however trivial, for inclusion in the news of the world—particularly the odd and exotic details of the lives of the myriad individuals, groups, and classes that inhabited the city.

Objectivity begins, then, in the romantic realism which infused the pages of the penny papers: the details of trade and commerce, of the courts and streets, of the strange and the commonplace. But this interest in turn required the stretching of awareness beyond that of kinship and the small group; it awaited the creation of a larger urban container in which the lives of strangers took on significance. In the 1830s society took on an objectified existence; it became a realm apart from and other than the individual—a site of the strange and unknown. It was in this urban container of a democratic, market society and in the merchant and business class that emerged to lead it that the groundwork for the modern newspaper was laid.

But if the ideal of objectivity can be traced to this panorama of facts, this gastronomy of the eye, that emerged in the penny papers, the ideal required 100 years of evolution before it became an articulate belief. It awaited two crucial developments of the later 19th century. First, during most of the century, journalists had valued facts, if they valued them at all, only as vehicles for telling a story; in the 1890s, however, the ideals of journalism began to diverge and take up residence in quite different kinds of newspapers. Earlier journalism had striven for both realism *and* entertainment. Now a new group of journalists conceived realism and entertainment as opposites. Two models of journalism thus emerged, which Schudson

calls "story" and "information" models. The story model attempted to grasp life in the turn-of-the-century city in narrative form. It wished to make the richly variegated urban container—filled with a bewildering variety of worlds and subworlds, immigrant and native groups—intelligible in emotionally compelling ways. The *New York World* under Pulitzer exemplified the story model: mass journalism that rendered the everyday life of people new to the city, to politics, to America, to literacy, to mobility. The *New York Times*, on the other hand, grew to typify the information model: a form of writing which suppressed narrative for the disconnected fact; which suppressed the variety of the urban scene for a more systematic view of social life; and which suppressed, above all, the emotional tones of the social scene. These two models of journalism reflected the different outlooks, appetites, and moral values of the educated middle class which supported the *Times* and the middle and working class which apprehended the city through the story model of the *World*. The division is not neat, of course. The *Times* served as a forum of upward mobility for some and the *World* provided a voyeur's view of the low life for others. But these different models of the world—one controlled and abstract, the other emotional and disordered—reflected the location and experience of different classes in the social scene. Again, Schudson locates the development of journalism in class and status conflict.

The second critical fact about the 1890s was the emergence of a new group within journalism that experienced this conflict—a conflict of class and status realized as a conflict of style—with the greatest intensity. Nineteenth-century journalism was dominated by the printer and the editor until the reporter emerged as a distinctive figure late in the century. Reporters had somehow to render the urban scene intelligible, and they did so with relatively few literary guidelines. As a group they were also subject to all the pressures of professionalism that faced the upwardly mobile occupations. The conflict between story and information lay at the center of arguments about professionalism in journalism and, not surprisingly, the information model (supported as it was by the canons of science, canons which virtually defined professionalism) emerged as the dominant professional ideal. If journalists told stories, they were to be stories that came as close as possible to the standards of science: literary realism became scientific empiricism.

If the basic conditions of objectivity were laid in the 1830s and 1890s, the norm was articulated only after World War I and then (as so often in human affairs) only as the norm became impossible to realize. Perhaps the most distinctive aspect of Schudson's argument is his location of the practice of objectivity in the 1920s, precisely when the world of facts, of a hard and given reality, started to dissolve. If objectivity is the separation of fact from value and the insistence that the world be rendered exclusively in terms of the former, its doctrinal articulation in the twenties was a defensive posture in a world that did not admit of this separation. The rise of public relations and the example of wartime propaganda were, in Schudson's

view, the key events that gave rise to a distrust of facts. The world came to be seen as the subjective expression of human intentions; all perception became colored by ideological hopes and fears. Objectivity, with its emphasis on a strict method of professional practice, became a device for staying afloat in a world of skepticism, a world where all communication was colored by the interests it represented. Objectivity was the assertion of method in the teeth of disbelief.

And so the dirty secret at the heart of journalism is that the cardinal term of its practice was never quite believed in by its practitioners. By the 1960s the secret was out. Objectivity, the prime antidote to bias, came to be viewed as the most insidious bias of them all. The canons of professional practice were seen, not as guarantors of neutral fact, but as devices by which official views of reality were reinforced, by which wealthy and powerful groups imposed their will on the world. Objective reporting became a mask behind which journalists turned a blind eye to the basic structures of power and privilege. The increasing explicitness of news management and the emergence of a widespread adversary culture, along with the deep ambivalence in the heart of journalists themselves, sustained the attack on objectivity in the sixties and seventies. News management, particularly during the Vietnam War, rendered transparent even to journalists the political effects of professional practice. Adversary culture, a term Lionel Trilling used to describe the oppositional spirit of modern art, infiltrated the daily imagination of wider segments of the educated middle class and also the journalistic art itself. Thus news management and adversary culture conspired to radically weaken, if not destroy, the norm of objectivity, leaving modern journalism awash in uncertainty and many journalists confused about their art though determined to prevent the independence of the press from being co-opted by the established centers of power and persuasion.

Schudson has told an effective history of the norm of objectivity. His rendering of certain features in the history of American journalism will influence and reshape scholarship for years to come. Need I add that I admire the book and wish it a wide and sympathetic readership? It is, however, troubling in important ways and unconvincing at certain critical junctures.

The basic problem of the book is that it breaks in half, and the form of explanation that usefully illuminates the first half practically disappears as we approach the contemporary part of the story. Schudson's basic achievement is his application of a class- and status-conflict model to the development of the press during the 19th century. The model illuminates a wide range of details in the evolution of journalism; it also leads to compelling commentary on the evolution of stylistic features of the journalistic report. Yet even here there will be controversy. There is still a case to be made that the penny press reflected class conflict, not between the middle class and the gentry, but between merchants, traders, and manufacturers on one side and mechanics, artisans, and the developing proletariat on the other. It will be important to consider the penny press as a commodity which served

to choke off and deflect the development of forms of journalism grounded in the interests and style of the working class. Although the problem remains, Schudson's work will allow it to be intelligently debated.

More troubling is the fact that the model of class and status conflict developed so tellingly for the 19th century is not applied to the press and the norm of objectivity as they developed in the 20th. Oddly enough, this trouble first appears when Schudson ascribes the growth of the norm of objectivity to the emergence of public relations and propaganda in the 1920s. What is evident here is vacillation between a deep and a shallow sociology of knowledge. The entire 20th century is in fact one long epistemological crisis that goes much deeper than its surface manifestations in commerce and politics. Throughout this century, philosophy has challenged the grounds of belief, and journalists have been subject to all the epistemological pressures which have haunted scientists, even if in less elevated form. The focus on propaganda and public relations, instead of on the deeper problems of belief, obscures the extent to which these problems are bound up with class and power. Schudson illustrates the emergence of objectivity through a contrast between the writings of Walter Lippmann before and after World War I. Over that period Lippmann moved from what John Dewey called a belief in "science in society" to a belief in a "science of society." In the 1920s a new class of scientists and technicians emerged, a class that wanted to manage society through knowledge not available to ordinary men and women; journalists were enthusiastically among this class. Thus the conflicts and uncertainty over objectivity reflected not only problems of belief but also the problems associated with buttressing new claims of power and privilege and with working through new relations between the traditionally dominant groups and new professionals both aligned with them in class terms and divided from them in status terms.

If the problem of sustaining a class and status analysis into the modern era weakens the section on the 1920s and 1930s, it does more severe damage when we come to the 1960s. The use of the notion of adversary culture implicitly ignores the fact that protest against the press and against the norm of objectivity came from quarters larger than educated youth and rebellious reporters. It also came from Spiro Agnew and the "silent majority" who saw objectivity as a mask for the power and privilege of the press and of the adversary culture itself. The weakness is exemplified in a sentence: "Thus while a culture of criticism was finding more leaders and more followers in higher education in the 1960's, there was also an apparently long-term trend toward greater political sophistication and critical scrutiny of government on the part of a substantial portion of the population" (pp. 178-79). This is not a judgment I find compelling. There is a deep civil war of sensibility going on among segments of the middle class, just as there are deep divisions over an appropriate political and economic life. Schudson's sentence casts the balance of his loyalties in one direction in this conflict. That is acceptable, but it drains the analysis of its organizing concepts and the true dimensions of this conflict are lost. The critical culture



is found in places other than universities, and adversaries of the press are everywhere. The press itself is deeply divided on issues of sensibility, politics, and economics; and a somewhat more neutral tone is necessary in order to excavate the full dimensions of this conflict.

Despite these reservations, I believe Schudson has made a major contribution to contemporary scholarship concerning the press and has, I hope, reinvigorated a historical sociology of the mass media.

Book Reviews

The Dialectic of Ideology and Technology: The Origins, Grammar and Future of Ideology. By Alvin W. Gouldner. New York: Seabury Press, 1976. Pp. xvi+304. \$14.95.

The Future of Intellectuals and the Rise of the New Class: A Frame of Reference, Theses, Conjectures, Arguments, and an Historical Perspective on the Role of Intellectuals and Intelligentsia in the International Class Contest of the Modern Era. By Alvin W. Gouldner. New York: Seabury Press, 1979. Pp. 121. \$8.95.

The Two Marxisms: Contradictions and Anomalies in the Development of Theory. By Alvin W. Gouldner. New York: Seabury Press, 1980. Pp. viii+397. \$17.50.

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We have here a trilogy; with the added trouble that the third volume is itself the first of yet another trilogy to be devoted to post-Marxism. The present trilogy deals with what Alvin W. Gouldner calls the "dark side of the dialectic" or, more plainly, what went wrong with Marx. That something is wrong with Marxism will be obvious enough both inside and outside Marxism. It is so whether we take it as an academic pastime or as an attempt to solve the problems of freedom, civil rights, and bare subsistence in countries that have espoused Marxism as an ideology. What is wrong will vary according to whether one asks for an opinion from Solzhenitsyn, David Rockefeller, the Pope, the Gang of Four, or the shipyard workers of Gdansk. It makes little difference whether one reads *Time* or *Telos*. Yet none of this can be cause for smug sociological satisfaction. There is a lot wrong with American sociology, too—as we know, or may prefer to recall with a little help from C. Wright Mills, or from Alvin Gouldner's earlier *Coming Crisis in Western Sociology* (New York: Basic, 1970), a belated work in which he first displayed the reflexive theorist's anxiety that feeds all his later work. The Parsonian precursor is now officially dead, having served to oedipalize American sociology as effectively as the Marxian incubus seduced the socialist mind. The recent appearance of Bottomore and Nisbet's *A History of Sociological Analysis* (New York: Basic, 1979) should be enough to convince anyone that all is quiet on the western front; the mummies are in telling tales of the dead.

Gouldner knows all this. Yet he remains remarkably alive and open to

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the best and the worst of it. The trilogy under review is a testament to one American sociologist's persistent attempt to absorb current European Marxist theory without losing hold of what still seems to me his better and ultimately American sense on some of the issues. It is the work of an admittedly flawed intellectual, honestly probing what he considers the fundamental issues in the ideological limits of the social sciences as expropriators of the standards for rational discourse in Western society.

Here I mean to dwell on a central theme in the trilogy. (What else can I do? Although it demands to be read as a whole, I can merely treat one of its leading ideas in the space allotted to me.) Sociology develops (I shall be paraphrasing Gouldner) as part of the positive faith of Enlightenment society that its affairs, practically and morally speaking, might be made the business of a science. Marx turned this claim into an ideology by pointing out that the social sciences were failed sciences in the sense that they depend for their existence on the very social institutions whose "laws" they claim to discover. Here, then, we have one of Gouldner's "two Marxisms," namely, scientific Marxism, which exposed bourgeois ideology and arrogated to itself the character of a science of society grounded in the law of socialist development. But—"whether the newly emerging socialist society coincides with the specifiable requisites of a rational consciousness or discourse, whether the new social structure imposes certain (even if different) *limits* on consciousness, and whether and how far these might be modified to protect and strengthen reason, is not made problematic" (*The Dialectic of Ideology and Technology* [referred to hereafter as *Dialectic*], p. 15).

Such issues, however, are the very stuff of the second Marxism, namely, critical Marxism. Readers will have to see for themselves how they are developed in *The Two Marxisms*. I shall only note that I would have liked to see Gouldner make more contemporary use of discourse and rhetorical analysis rather than try to resuscitate Thomas Kuhn's paradigm, which hardly comes to terms with the linguistic production of scientific inquiry, the issue suggested in the first two volumes of the trilogy. (See John O'Neill, "The Literary Production of Natural and Social Science Inquiry," *Canadian Journal of Sociology*, vol. 6 [Spring 1981].) Nor is there sufficient exploration of what I call the "two sciences" in Marx. Is Marxism a science like the natural sciences? Can the natural sciences be critical sciences like Marxist science if the latter is indeed critical or not natural? Gouldner shows the historical consequences of ignoring these ambiguities, but he does not consider them in the light of post-Kuhnian sociology of science.

Critical Marxism derives from a nagging worry which, in my view, Marx first expressed in the Third Thesis on Feuerbach, namely, Who shall educate the (socialist) educators? In dealing with the reflexivity in this question Gouldner finally wrestles free of the baleful influence of Alan Blum's *Theorizing* (London: Heinemann, 1974) by turning to Mannheim, Basil Bernstein, and Jürgen Habermas. (Nevertheless, chap. 5, "From the Chicago School to the Frankfurt School," is an exercise in intellectual rapid transit that I find hard to take!) Once again, with desperate brevity, the

issue comes to this: we cannot be sure that the social sciences, and Marxism in particular, are not a further form of expropriation in the context of modern administrative states—especially single-party states. According to Gouldner, “In this respect, Marxism like other ideologies is a rational mode of discourse that embodies a specific communication pathology—‘objectivism.’ *Objectivism* is discourse lacking in reflexivity; it one-sidedly focuses on the ‘object’ but occludes the speaking ‘subject’ to whom it is an object; objectivism thus ignores the way in which the spoken object is contingent in part on the language in which it is spoken, and varies in character with the language—or theory—used” (*Dialectic*, p. 45).

The reflexive question for critical Marxism is how scientific Marxism can be at once a science of history and for history, that is, at once nature and norm. To answer this, Habermas gave to Marxism a linguistic turn (in his *On Critical Theory*, ed. John O’Neill [New York: Seabury, 1976]), bringing to the fore the issue of symbolic production as the more general context of material production. In the speech community, then, we find a universal that rivals the proletariat as the native and norm of historical development. Gouldner deserves credit for setting the otherwise overly “ideal speech situation” in the context of Habermas’s historical study of the development of the bourgeois realm of the public and the rise of the media of “rational” political opinion (*Strukturwandel der Öffentlichkeit: Untersuchungen zu einer Kategorie der bürgerlichen Gesellschaft* [Neuwied/Rhine: Luchterhand Verlag, 1962]. Habermas informs me that an English translation is under way. Gouldner appears to rely on a short German encyclopedia article which summarizes the argument, an article translated into English in *New German Critique* 3 [Fall 1974]: 49–55). Keeping in mind, then, the historical background of the ideal speech situation in bourgeois institutions, Gouldner observes that “Four ‘simple’ elements seem crucial for the practical production of Habermas’ ideal speech situation—(1) no violence, (2) permeable boundaries between public and private speech, (3) allowance of traditional symbols and rules of discourse to be made problematic, and (4) insistence on equal opportunities to speak” (*Dialectic*, p. 142).

Gouldner makes some cogent criticisms with regard to the difficulties of instituting political action in terms of the critical theory of communicative competence. If this is so even in the context of mass-educated society, with relatively uncensored media and plural party democracy, it is all the more difficult in relatively illiterate colonial societies where brute force is the likely response to any form of awakening consciousness, as Frantz Fanon and Paulo Freire have shown (John O’Neill, “Le Langage et la décolonisation: Fanon et Freire,” *Sociologie et sociétés* 2 [November 1974]: 53–65). Nevertheless, Gouldner bites the bullet. Socialist criticism can only be part of bourgeois criticism. This is so because socialist criticism that does not take a thoroughly conspiratorial view of bourgeois values is impossible wherever rudimentary parliamentary process, human rights, and freedom of the press and other media are absent. Gouldner, like the rest of us, reads and writes enough books to know what that means!

So we need to look ourselves in the face, and what we see in Gouldner's mirror is the New Class of intellectuals, technicians, experts, educators, and media workers. These are people with a professional interest in the power and privilege of ownership in a technical language (*sociolect*) that speaks on behalf of a public, clientele, or interest group that may be serviced by it, provided such service does not overly limit the elitist professional values of the new servants. Gouldner's "*Thesis Six: The New Class as Speech Community*," begins by stating: "The culture of critical discourse (CCD) is an historically evolved set of rules, a grammar of discourse, which (1) is concerned to *justify* its assertions, but (2) whose *mode* of justification does not proceed by invoking authorities, and (3) prefers to elicit the *voluntary* consent of those addressed solely on the basis of arguments adduced. CCD is centered on a specific speech act: justification. This grammar is the deep structure of the common ideology shared by the New Class. *The shared ideology of the intellectuals and intelligentsia is thus an ideology about discourse*" (*The Future of Intellectuals*, p. 28).

The New Class is no better and no worse than the society it serves or dominates. Each is unthinkable without the other. Despite the jargon, the nonsense, and the lies—it is publish or perish. Gouldner's "*Thesis Fourteen: The Flawed Universal Class*" puts it like this: "Even as it subverts old inequities, the New Class silently inaugurates a new hierarchy of the knowing, the knowledgeable, the reflexive and insightful. Those who talk well, it is held, excel those who talk poorly or not at all. It is now no longer enough simply to be good. Now, one has to explain it. The New Class is the universal class in embryo, but badly flawed" (*The Future of Intellectuals*, p. 85).

Gouldner soldiers on. If he has a good mind and his heart is in the right place, what else can an intellectual do? Besides, we should otherwise be ruled by the little men of sociology, or the gray men of the party, without a voice of their own! What else needs to be said of Gouldner's life work cannot be confined to a review. We lack vital community of sociological thought. The books sell or they do not. One can only hope that those who buy them will read them with the intelligent care they deserve.

Essays on Interpretation in Social Science. By Georg Simmel. Translated and edited with an introduction by Guy Oakes. Totowa, N.J.: Rowman & Littlefield, 1980. Pp. 207. \$19.50.

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Forget the title. Only half the book comprises "essays" actually written by Georg Simmel, essays drawn mostly from an uncompleted supplement to his earlier work, *The Problems of the Philosophy of History*. The other half, written by editor Guy Oakes, contains a long analysis of Simmel's

essays and their place in his overall thought. As for "interpretation," only about one-third of Simmel's and the editor's writings actually concern our understanding of people and artifacts, past and present. Nor does this book deal with "social science" in the broad sense: sociology, economics, and political science are scarcely mentioned, despite Simmel's voluminous contributions to them. The only social science discussed at length is history—although any editor who uses "historical sciences," "social sciences," and sociocultural "sciences" interchangeably might not see this synecdoche as misleading. A more accurate though more awkward title would be *Simmel's Philosophy of History, with Special Reference to Historical Interpretation and Its Place in His World View*.

I do not mean to disparage this book because it concerns *only* history. Simmel's conception of history is important because his work has often been falsely accused of being ahistorical. In fact, he was well aware that not only the contents of his forms but even their natures themselves have changed over time. At various historical periods, for instance, the "art" form comprised not only different works of art but also different conceptions of what constitutes art. Simmel, however, disagreed with the extreme historicism prevalent during his time, the claim that all we need to know about a phenomenon is its genesis. He contended that such historical explanations do not exhaust our understanding because phenomena develop formal properties independent of their genesis, properties which must also be understood. Thus Simmel's effort to distinguish formal from historical explanations parallels Durkheim's effort to distinguish functional from causal ones.

Oakes's explication of Simmel's views is clear and accurate—no easy feat. He is especially good at pointing out ambiguities in Simmel's thinking, such as the various meanings of "form." His translations too are very readable and make sense—which is more than can be said for some earlier translations.

Yet for all its scholarly virtues, Oakes's discussion of Simmel's thought is slow, ponderous, and somewhat dull. I believe he violates the spirit of Simmel's writing by trying to capture the letter so precisely and rigorously. The last thing Simmel would want is the reification of his conceptions, for that would strangle the vitality of his views. Simmel's most important contribution to modern social science was not procedures (as was Durkheim's) or substance (as was Weber's) but inspiration.

For example, consider the following insights which Oakes merely mentions in passing, insights that are still living today:

1. The essence of modern culture, according to Simmel, lies in its focus not on ends but on means, such as money and methods. Cultural works are now acquired not for their intrinsic value but for their exchange value. Intellectual disciplines have now elevated their techniques of investigation to a privileged, quasi-sacred status, even higher than their substantive results. This tragic reversal of ends and means pervades all aspects of modern culture, to the detriment of human values and understanding.

2. Simmel anticipates many of the recent concerns of ethnomethodology.

He discusses the way in which we construct our conception of a phenomenon artificially out of fragments accessible to us empirically. In everyday life, we form the gestalt of an entire personality from the few features and behaviors we actually encounter. In intellectual disciplines such as history, we synthesize discontinuous data into continuous wholes, as well as fracture the continuity of the temporal process. For example, we artificially lump together individual works of music into "The Classical Period" and distinguish that artificially from "The Romantic Period." Moreover, understanding in the historical sciences is based on understanding in everyday life. As Simmel put it, "Ultimately our understanding of the Apostle Paul and Louis XIV is essentially the same as our understanding of a personal acquaintance" (p. 65).

3. "The first condition of historical understanding, Simmel claims, is the following: the historian must be able to recreate the mental processes of the historical person" (p. 59). (Here the editor omits a useful comparison with Weber's views on *verstehen*.) How best to recreate the minds of others is an issue at the heart of some current controversies. Should only blacks write black history? Should only women write women's history? Simmel's surprising answer would be a qualified yes. His discussion of how much "psychological isomorphism" there should be between historian and historical personage is sophisticated, though ambiguous. Particularly relevant to recent polemics is Simmel's conclusion: "Women can be expected to make original contributions to the sociocultural sciences, contributions which could not have been anticipated by men" (p. 78).

4. In Simmel's short essay "On the History of Philosophy," included here and one of his most important, he asserts that philosophy is not a series of verifiable propositions, but an attitude toward life. As such, a philosophy cannot be refuted; its truth or falsity does not matter. Different philosophies simply appeal to different types of people by articulating their own—half-conscious—attitudes toward life. Might we not say the same about sociology? Should a sociology be judged not by whether it is true or false but by whether it clarifies the social world for a certain type of person? No doubt those who believe that sociology should generate true propositions about the world would object strongly to this view. But their objection loses force when we consider that this "verifiable" conception of sociology itself merely articulates their own peculiar attitude toward the social world, an attitude they are trying to impose on those who do not share it.

5. Finally, since Simmel regards the social and cultural world as splintered and subjective, he sees any attempt to understand it rigorously and universally as bound to falsify it. Hence systematic social or cultural sciences are false per se. Instead of being a deficiency, then, Simmel's fragmentary approach offers the only way to comprehend the kaleidoscope of life without simplification or falsification. Yet he himself sometimes violated his own view and tried to unify his own thinking—attempts generally regarded as failures. It seems better for thinkers like Simmel (and like Goff-

man today), whose talents lie in their concrete observations about individual social and cultural phenomena, to avoid self-synthesis and leave to others the task of unifying their specific perceptions into general theories.

Consequently, I find Oakes's clear and comprehensive attempt to unify Simmel's scattered insights even more valuable than Simmel's own fuzzy efforts. Simmel's theories of form, culture, and interpretation, Oakes believes, all center on the point at which human creations become independent of their creators. Unfortunately, Oakes misses at this point the priceless opportunity to pull together much of social theory by comparing Simmel's "Golem" or "Frankenstein" model of human creation with similar models of other social theorists, such as Marx's view that the economy becomes increasingly alienated from its producers, Freud's view that the society becomes increasingly repressive to its sires, and Goffman's view that the self-presentation becomes increasingly autonomous of its presenter.

Still, Oakes's interpretation of Simmel is admirable, if incomplete. Just as the world, in Simmel's view, is so fragmented that many different interpretations of it are necessary to capture its multiple essences, so Simmel's own observations of this world are so fragmented that many different interpretations of them are necessary to capture the variety of principles that motivated his work.

Despite its quality, I cannot recommend this book to most sociologists, for it tells more about Simmel's views on esoteric topics than they probably want to know. Neither can I recommend it to undergraduates or even most graduate students who want an introduction to Simmel; this difficult book is definitely "advanced Simmel." But Simmelphiles who want to track down the sources of his living insights mentioned above, and professors of social theory who want to stay one step ahead of their students, will certainly find this book worth plowing through.

The Politics of Elite Culture: Explorations in the Dramaturgy of Power in a Modern African Society, By Abner Cohen. Berkeley and Los Angeles: University of California Press, 1981. Pp. xxii+257. \$20.00 (cloth); \$6.95 (paper).

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The "Creoles" of Sierra Leone are descendants (sociological and largely biological) of late 18th- and early 19th-century settlers of African origin brought by the British to the fledgling West African colony around Freetown. They came from Britain, from the Caribbean, and off slaving ships sailing from West Africa to the New World but captured by the British squadron charged with suppressing the slave trade. These Creoles took on an ethnic identity distinct from that of the surrounding African population and, being Christian and educated in Western ways, they became the

mediators between the intrusive colonial power and the indigenous Africans. In time, the group produced the local elite of civil servants, traders, Free-town landlords, lawyers, educators, and other professionals, as well as humbler folk such as artisans—all of whom saw themselves (and were seen) as a group apart. Although there is considerable historical literature on these Creoles, Abner Cohen's is the first anthropological study of them. *The Politics of Elite Culture* is not an ethnography so much as a case study that examines a set of related theoretical questions about elites and their self-maintenance. This is a crisp study, in which the data are tightly harnessed to specific theoretical concerns and to an overall mode of analysis in which the perpetuation of power is seen to be in a dialectical relationship with symbols and their dramaturgical reenactment in social rituals.

With the coming of independence to Sierra Leone, the Creoles found themselves in the dilemma of many "in between" colonial groups: how does such a minority adapt when political power passes from a colonial structure into the hands of the indigenous majority? This dilemma brings into exceptionally sharp and dramatic focus the problems of elite self-maintenance. Cohen analyzes Creole society in these terms, taking for his theoretical inspiration the work of Marx, Simmel, and C. Wright Mills and merging it with the superb tradition of analytical ethnography developed by British social anthropology, specifically by the so-called Manchester school of Max Gluckman. In Cohen's formulation, all elites in liberal societies (and I would amend this to encompass all formally egalitarian societies, including Marxist ones) face a contradiction that they must "mystify" since it is unresolvable: on the one hand, the elite must maintain its exclusive in-group values and interests and, on the other, it must present its position as serving "universalistic" society-wide values and interests. Accordingly, the Creoles have stressed their attachment to such values as antitribalism, civility, nonpolitical legal institutions, an independent civil service, professionalism, and promotion by merit and educational achievement. At the same time, there has been, since independence, a tightening of Creole networks through such in-group institutions as churches, exclusive clubs and social affairs, secretive masonic lodges, and funerals and ancestral cults. Cohen provides rich sociological data and vivid description in presenting the workings of the various integrative Creole institutions, their symbolic trappings, and the dramatic public reiteration and intertwining of universalistic and in-group values.

The theoretical thrust Cohen chooses to give his data carries him into the sociology of elites. The Creole situation suggests other analytical paths as well, paths that Cohen has explored in his previous work. For instance, the Creoles are an ethnic group amid other ethnic groups, in a society in which ethnicity is a fundamental cultural principle of social classification. No matter how Creoles might wish to be seen, the overwhelming African majority around them would see them in most social and political contexts as they see themselves—as belonging to an ethnic category. When eliteness is perceived in ethnic terms by the actors, the elite's strategies must differ from those of an elite free of ethnic entanglements. Of the 40,000 Creoles

in Sierra Leone, only a few thousand are in the elite of a country of over 2 million, but the "tribalism" of the surrounding society diffuses the problems of the Creole elite to all the Creoles. Cohen has dealt with the dynamics of ethnicity in his well-known work on the Hausa traders of Nigeria, and *The Politics of Elite Culture* should be read in conjunction with that work, which suggests issues that Cohen does not pursue here. In combination, the two studies provide a finely wrought framework for a continuing analysis of the ethnically tinged dynamics of much of Third World politics.

Other case studies along the lines of Cohen's work would be invaluable. Now that Marxism, rather than fascism or liberalism, provides the political rhetoric for a large portion of the world, there are exciting possibilities in the study of ethnic and quasi-ethnic elites. With the Creoles specifically, an interesting comparison is offered by present-day Angola. Here, a Portuguese-speaking mestizo and Creole ethnic group has acquired and maintains its power under a Marxist ideological banner—an ethnic variation on the New Class theme. All in all, it is surely time for rigorous neo-Marxist analyses to be addressed to societies arising out of applied Marxism carried to different cultural settings.

White Supremacy: A Comparative Study in American and South African History. By George M. Fredrickson. New York: Oxford University Press, 1981. Pp. xxv+356. \$19.95.

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George Fredrickson's *White Supremacy* makes a superb contribution to the study of comparative race relations and, through a rigorous comparative method, superbly illuminates the peculiarities of racial conflict and domination in the United States and South Africa. The book is elegantly crafted and shows extraordinary sensitivity to what is idiosyncratic and shared in these settings. It is difficult to imagine that scholars, whatever their perspectives on these problems and societies, will not be intrigued by (and forced to deal with) Fredrickson's arguments.

In the rush of superlatives and rave notices, however, one should not lose sight of what is wrong with this book. Some of what is wrong is inherent in the method itself. Our respect for the stamina, discipline, and breadth of historians and social scientists who move easily across continents must be tempered by some recognition of the limits of comparative historiography. Some of what is wrong with this book rests with the author's specific approach to the subject. In particular, there are understandable decisions to emphasize certain areas, groups, or periods over others, but with little notice to the reader about what is missing or about the importance of these choices for understanding white supremacy in South Africa and the United States. There is a reluctance to follow through and elaborate

on a number of provocative insights. Finally, although there is a broad sensitivity to the role of material factors shaping race relations and racial beliefs, there is also a troubling reluctance to articulate conceptually the role of such factors or to raise the issue to the level of thematic importance granted other concerns.

Fredrickson begins by setting out a reasonable terrain for his project in comparative history: an emphasis more on differences than similarities; a recognition that a focus on historical process is indispensable if one is to transcend the specificity of each case; a call to go beyond the parochialism that too often characterizes single case histories; a recognition that one must take account of both race and class factors and their interaction, though without granting either *a priori* primacy; and a need to limit one's comparative focus, in this case, by emphasizing conflicts within the dominant European communities.

The remainder of the work is organized around broad historical periods during which roughly similar processes emerged in the two settings. For each thematic period, Fredrickson provides a fascinating account of unfolding white supremacy, drawing expertly on secondary source materials and his own long-standing comprehension of racial imagery and ideology. The exercise is effective and engaging.

In the section on original European settlement and subjugation of indigenous populations, Fredrickson details a process which is commonly brutal but which takes radically different courses in the two settings—the process toward dispossession and genocide in the case of the American population and toward incorporation, clientage, and “semi-feudalism” in the case of South Africa's Khoikhoi. In both accounts, Fredrickson underlines the way settlers and colonial authorities used images of the indigenous population in the process of subjugation and how that imagery changed as the process continued. Indians, for example, were seen as savage during the process of settlement, but were depicted increasingly as exotic while they were being slaughtered or pushed west—in either case, being removed from the immediate world of European settlers.

Fredrickson's account of racial slavery is acutely sensitive to the importance of slavery to subsequent patterns of race domination and thinking; indeed, he argues that its presumptions prove more enduring than any other single factor in the development of white supremacy. Reflecting a premise set out in the preface, Fredrickson shows how such a powerful “idealistic” force shaping subsequent events is itself shaped by real and imagined class struggles within the European community. European indentured labor was curtailed in favor of chattel slavery after the 17th century, as southern landowners faced a growing, economically marginal European population, a population with increasing numbers free of indenture and with independent access both to the means of subsistence and to arms. While southern landowners had sufficient autonomy to fashion a labor system suited to their needs, Cape slaveowners operated under closer scrutiny by the Dutch East India Company and later by the British colonial office. The former helped define baptism as a basis for manumission, furthering the growth of a free

population of color; the latter eventually emancipated the Cape slave population and barred the spread of slavery into the interior of southern Africa.

In the chapter on race mixture and the color line, Fredrickson uses his comparative design and prior research to maximum advantage, forcing, I suspect, a serious reexamination of the issue, at least in South Africa. He points out how the two societies came to treat the offspring of racially mixed unions differently: the South permitting only two castes with the offspring relegated to the subordinate position; South African society allowing for the possibility that such offspring would occupy a middle position, or in some cases be admitted to the dominant European caste. Fredrickson, by judicious use of secondary and primary sources, offsets a parochialism in South African historiography that underestimates the significance of interracial unions. The difference between the two settings, he suggests, may be explained by patterns of settlement and colonial rule (e.g., in the South, sex ratios were more balanced than among Cape settlers; in South Africa, the Dutch East India Company imposed rules for both baptism and manumission, and the settler population was smaller, more scattered, and lacking representative institutions). The explanation also rests with an understanding of class considerations within the dominant caste. A free mulatto caste in the South would not have served any identifiable need of upper-class whites. The mulattoes, for example, were not needed for military service, since sufficient numbers of whites could bear arms. Indeed, the two-caste system served to bolster the social prestige of a large lower-class white population, tending therefore to reinforce the institution of racial slavery.

Fredrickson's next section is somewhat unwieldy, covering the period from 1776 to 1910 and events as diverse as the American Revolution, Civil War and Reconstruction in the American South, and the Boer Republics, the Anglo-Boer War, and the establishment of union in South Africa. Fredrickson describes the struggles that racked and democratized white politics while creating further disabilities for non-European populations. He thus lends strength to his central proposition that the contours of white supremacy emerge out of the "attitudes, beliefs and policies of the dominant whites . . ." (p. xx). In response to abolitionist, missionary, and liberal capitalist pressures, whites in the South and in South Africa articulated a racial defense of slavery and, more broadly, of a "racially circumscribed democracy." The idea of white supremacy became a centerpiece in attempts to legitimate coercive labor forms in both regions.

Fredrickson believes that the industrial process incorporates preindustrial racial beliefs and practices, although, at least initially, it followed radically different courses in the two settings. In the United States before World War I, blacks were largely excluded from manufacturing jobs, reinforcing the conception that they were incapable of industrial discipline; in South Africa, blacks were incorporated but within a rigidly hierarchical employment structure that segmented the labor force racially. With regard to South Africa, Fredrickson writes, the "entire system can be viewed as an adaptation to industry of the traditional pattern of coercing black labor

that had its ultimate roots in slavery and Khoikhoi indentured servitude" (p. 220). In later periods, the pattern of industrial race relations proved competitive and conflictual in both settings: in South Africa, white workers successfully blocked the wage-depressing effects of a "disenfranchised, semi-servile, and ultra-cheap class of workers" by imposing statutory job color bars; in the United States, white workers, with little support from the state, could not legally cordon off their areas of protected employment. In one case, a "slaveholding mentality retained its legitimacy" within an industrial milieu and within the modern state; in the other, coerced labor lacked a compelling legitimacy and the support of a well-developed labor repressive state apparatus (p. 238).

In his final section on segregation, Fredrickson despairs of systematic comparison between the Afro-American and African situations, as their lack of commonality seems to belie an examination of their differences. Still, he is able to show that the South's regional, only semiautonomous status and Jim Crow's limited legitimacy and scope (it had no pass system, for example) left the racial edifice vulnerable to the rise of black protests in the fifties and sixties. The system's fragility helps account for the endemic extralegal violence that has characterized the South's development since Reconstruction. It also helps account for the ultimate failure of white supremacy in the South, the failure to make preexisting beliefs about servile labor and racial capacities a regularized part of industrial life.

It is difficult, once one has acknowledged the unique power and scope of such insights, to belabor the work's shortcomings. Still, we would do Fredrickson and comparative history little good by taking leave of his book without examining a number of important problems.

Only a fool would venture into a comparative history project of this scope without a road map, and Fredrickson is no fool. He is guided by thematic periods and historic processes, particularly conflicts within the white communities of South Africa and the United States. Consequently, he avoids, as he must necessarily, great areas of the historic landscape. Unfortunately, he does not take note of what is missing.

For the most part, Africans are missing—no mean omission in the case of South Africa, a country where Africans constitute nearly 70% of the population and the essential object of white supremacy. The first serious mention of European-African contact and emerging racial patterns does not appear until the last third of the book. While "Africans" are not listed in the index (except for two brief notices about "Zulus" and "Xhosas"), there are extended references for "Afrikaners," "Boers," "British," "Cape Coloreds," "Chinese," and "Khoikhoi." The emphasis is understandable enough, as the Cape and mixed-race groups provide much more fertile ground for the comparative project. Warnings to this effect, however, would have allowed readers to assess better the value of Fredrickson's contribution.

But the omission is more serious than that. When the discussion moves to the Transvaal and Natal, where European-African race relations constitute the core of white supremacy, Fredrickson includes African society only vaguely, almost as an extension of non-European Cape society. He

does not dwell on the difference between the scattered and disorganized Cape Colored community—fully dependent on the European economy and integrated onto white-owned farms—and the various African societies—largely intact, often well organized, and sometimes able to market agricultural surpluses. Having failed to take note of the organization of African society, Fredrickson cannot begin to understand the forms of racial segregation and labor exploitation ascendant during the past century of South Africa's history. The "reserves," first established in Natal and the Transkei and later extended to the Free State and Transvaal, now constitute the historical justification for spatial segregation, bantustans, and "grand apartheid." The long-standing dependence of African societies on organized subsistence production made necessary "hut taxes" and other state measures to dislodge African labor; the later disintegration of the African rural economy called for pass laws, labor bureaus, influx control, and other state measures to regulate the flow of African labor into the cities. It is these policies that make South Africa's brand of white supremacy seem so well developed in comparison with that of the South. Nor are the peculiar justifications for race domination in South Africa—notions of "separate freedoms" and "separate development"—likely to be understood apart from this historical experience.

Although other important areas are neglected in this work, I will not dwell on them. The account of industrialization in South Africa ends somewhere in the 1920s, before the weight of industry shifted from mining to manufacturing, well before white agriculture had completed the transition to capitalist production, and before the rise of state corporations. I understand the necessity of periodization in comparative work, but one must at the same time qualify theoretical speculation that depends on the longer view. On the American side, Fredrickson does not look closely at patterns of industrialization outside the cotton-textile industry, thus missing the principal forms of labor-force segregation and hierarchy developed since the thirties. The development of racially segregated lines of seniority and promotion, now exposed by the Equal Employment Opportunity Commission and the courts, established job color bars in the steel, rubber, paper, automobile, and shipbuilding industries, color bars nearly as extensive and rigid as those imposed by the South African state, employers, and unions.

There are a number of themes that Fredrickson develops through the book, enabling the reader to connect the thematic periods. Most important among these are the discussions of political conflicts within the respective European communities and the carrying forward and repeated incorporation of earlier racial experiences, particularly racial slavery. The Eurocentric focus will trouble some readers, though I do not think the problem should detain them. Fredrickson makes a powerful case for the importance of white intracaste conflicts and their importance for white supremacy. In addition, he repeatedly tempers this assertion with empirical discussions of intercaste conflicts that impinge on and shape white politics. Without this or a similar limitation, his comparative project would surely have slipped into separate case histories or disorder.

The centrality of racial slavery is an equally provocative argument, though a less convincing one, in part because Fredrickson does not fully follow through with the comparative exercise. He notes that "the long experience of enslaving nonwhites had a broadly similar impact on the genesis of white racial attitudes in the two societies. More than any other single factor, it established a presumption that whites were naturally masters and members of a privileged group while nonwhites were meant to be their servants and social inferiors" (pp. 92-93). The slave experience shaped the "pre-existing beliefs" that in turn structured race relations through this century; even in urban and industrial settings.

Fredrickson, while affirming broad similarities, provides evidence for profound differences; evidence of which he makes little use. Slavery in South Africa was largely confined to the western Cape, and even there servile but legally free Khoisan labor constituted a significant alternative or complement to imported slaves. The great majority of non-Europeans, the Africans, were never enslaved and never lived in a slave society. Moreover, slaveowners in the western Cape never controlled the state, the system of law, or dominant ideas.

By contrast, slaveowners in the South were ascendant; in control of the political institutions and society, they were sustained by a range of ideas that justified the supremacy of whites and the enslavement of blacks. The slaveowners did not have to contend with non-European servile laborers functioning outside the institutions of slavery. Fredrickson observes that the "individual master-servant relationship" here, more than in South Africa, had the "autonomy and power to shape the rest of society in its image . . ." (p. 91).

But while noting the difference, Fredrickson seems to play it down in his later analysis, apparently wanting to emphasize these shared preindustrial relations in the explanation of white supremacy. But one can easily imagine that such differences might reasonably figure in the explanation of "intermediate castes" and the absence of a proslavery movement in South Africa. By emphasizing a powerful common slave experience, one which is common only under the vaguest conceptual and empirical rubric, Fredrickson probably misreads capitalism's "adaptation" of traditional patterns. There were certainly adaptations, although they encompassed labor patterns central to the African nonslave experience, including the continued access of most Africans to areas of subsistence agricultural production. By relying so heavily on this preindustrial slave experience, he neglects what is genuinely distinctive in capitalist forms of race domination in South Africa, including pass laws, compounds, labor bureaus, and the process of controlled and partial proletarianization, all phenomena that have characterized South Africa's development in this century. The flowering of white supremacist ideologies came with industrialization and the transformation of agriculture, not with the slave experience of the 18th-century southwestern Cape Colony.

One of the singular strengths of Fredrickson's work is the intricate and subtle interweaving of ideological forms and material relations. Here, Fred-

rickson is masterful. The weaving runs through his treatment of settlement and subjugation (the "ideology of colonization"), racial slavery (the "racial defense of slavery"), liberty and union ("herrenvolk egalitarianism"), and industrial development (adaptation of premises).

From the outset, Fredrickson studiously avoids granting theoretical primacy to either "race" or "class" factors. Yet his refusal to explore the issue further seems coy, since his comparative analysis suggests something more than interaction. While emphasizing the content of beliefs, consciousness, and ideology, he describes a process in which material relations—specifically, class relations within the European communities—shape and exploit the realm of ideas. For example, despoilers of Indian and Khoikhoi lands and societies invoked images of savages, subhuman creatures; slaveholders in the Cape and the American South helped shift the rationale for slavery from religion to ancestry to encourage the belief that "the normal condition of dark-skinned people was abject servitude" (p. 85). The racial defense of servile labor and hierarchy was an essential part of the opposition to abolitionists and missionaries and essential to the legitimation of the labor system among poorer whites. Finally, traditional racial beliefs and expectations were employed by white owners and laborers in their struggles to construct the labor market and workplace.

The verbs and phrases that pepper the text—"invoked," "rationalized," "was crucial to," "legitimized," "justified," "was a way of obscuring," etc.—are suggestive. They are also curious, because Fredrickson in his introduction limits himself to the interaction of race and class. In fact, the work opens up potentially rich areas of inquiry, areas in which ideas gain salience and coherence under particular class circumstances but continue to shape events and to be invoked by emergent class actors after those original circumstances have passed. The most powerful case in point made by Fredrickson involves the presumptions in the racial defense of slavery. But he fails to elaborate his argument by examining subsequent labor systems, including patterns of wage employment and labor control in South Africa and the South, and their generation of new ideas and use of old ones.

It is the force of these lines of argument—some developed explicitly by Fredrickson and others provoked by him—that makes *White Supremacy* one of the most important books on race relations published in recent years.

Race and State in Capitalist Development: Comparative Perspectives. By Stanley B. Greenberg. New Haven, Conn.: Yale University Press, 1980. Pp. xiii+489. \$11.50 (paper).

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Although the title of Stanley Greenberg's book, *Race and State in Capitalist Development*, suggests an attempt to affiliate it with a rather rigid Marxist problematic, it is in fact an extremely open-minded theoretical and empir-

ical study and one of great importance to comparative and historical sociology. Its theoretical problem is that of showing why racial and ethnic differentiation and exploitation based on such differentiation persist in highly developed capitalist societies. Greenberg seeks to do this primarily by a comparison between Alabama and South Africa, but he also argues for the relevance of the cases of Israel and Northern Ireland, to which he devotes two chapters.

Naturally in a well-researched book of this kind the final conclusions are sometimes somewhat ambiguous, so it is worth quoting the note on the dustcover: "Capitalist development in racial societies initially brings an intensification of racial disabilities and the growth of a state racial apparatus. Commercial farmers use the state to undermine the subsistence economy and retain labour in the rural area. Businessmen turn to the state to help organize and control the labour force. And industrial workers seek job security. Each of these classes demands state vigilance over the race lines and labour market; each contributes to the elaboration of the racial state. In the later stages of capitalist development, the pressures become more mixed, but only widespread political disorder and external threats to the autonomy of the state have brought serious movement away from racial differentiation."

This is in fact the main conclusion; but the book is a many-layered study which addresses itself to a number of theoretical and empirical questions. It is important in that it addresses colonial peripheries of the world system, places to which writers like Wallerstein attribute repressive labor systems without saying why. It is an interesting analysis of the use of unfree labor by capitalism, even though both the Marxist and Weberian traditions in their different ways seem to suggest that capitalism and unfree labor are ultimately incompatible. It offers a theory of racial situations, one which serves both to criticize and to use the insights of various theories: the theory that regards race and ethnicity as primordial, the plural society theory, the theory of modernization, and both Marxist and non-Marxist theories of underdevelopment. Finally, on the specific question of South African racism it transcends the rather juvenile debate which resulted from Marxist polemics against the alleged liberal interpretation of South African history. It does so by showing that one does not have to hold either that South African racism is an archaic survival which will be swept away by the logic of industrial capitalism or that it is the creation of that capitalism.

Greenberg would, I think, see himself as adventurously introducing Marxist concepts into this exercise in historical sociology. This may be true, but it is important to note that the work as a whole would be entirely acceptable to scholars who call themselves Weberian. Those who might find it unacceptable are the followers of Poulantzas, for whom serious Marxism has to be a kind of dynamic systems theory, and who, with their master, dismiss as wrongly situated in a "historicist problematic" any study which starts from the orientation to action of class actors.

What Greenberg does in fact is to examine South Africa and Alabama, seeing their social structures as determined by the orientations to action

and the interests of three class actors: farmers, businessmen, and the white working class. The historical data available under each of these headings are well explored for South Africa and rather more briefly for Alabama. There are also chapters on businessmen in Northern Ireland and Jewish workers in Israel.

In the South African case the evidence adduced seems to show that farmers were in fact the dominant force behind the creation of the South African system. Starting out as inefficient large landowners with squatter labor, they moved toward capitalist agriculture, not simply by employing free wage labor, but by adopting what Greenberg sees as the "German" way of confining and exploiting the peasantry more systematically. Among the businessmen, the mine owners worked willingly within the framework created by the farmer-dominated Parliament, while secondary industrialists adapted to it, even if they disliked it. White artisan unions, as well as the general unions which survived, also worked for job protection, despite their official rhetoric (as black workers came to play a larger and larger part in industry) about the importance of trade union rights for all. There is little evidence offered here that the system as such undergoes any natural development toward deracialization and labor freedom. A kind of capitalism has evolved which, while highly modernized and efficient, takes racial exploitation and oppression for granted. This is likely to be changed only by outside forces.

The basic type of social structure which evolved in Alabama in the post-bellum period and survived at least to the fifties is seen as having much in common with that in South Africa. Perhaps the main difference is that it was less elaborated ideologically. It has, of course, been far more substantially modified in relation to outside pressure than has the South African system so far, though even now some aspects of the old order are surprisingly resilient in the South, where blacks are still subordinated, albeit by economic rather than political forces.

The two chapters Greenberg devotes to businessmen in Northern Ireland and Jewish workers in Israel are quite tantalizing. One feels bound to ask why there are no chapters on Northern Irish farmers, Jewish farmers, Northern Irish workers, and Jewish businessmen. Also, while conceding the similarities between the Irish and Jewish cases, one might ask whether there were not important differences. Nonetheless Greenberg deserves congratulations for proposing this agenda of study. It is high time that we had a center for the systematic study of troublesome societies, ones in which racial or religious settler domination is now challenged and sometimes violently resisted.

It is, in fact, as an agenda for future study that this book is most important, however immediately valuable the study of South Africa and Alabama. Would that there were more Greenbergs to follow that agenda through.

With that said, it might legitimately be asked whether we can accept Greenberg's framework as theoretically clear enough to support such a comparative research program. Certainly Greenberg is aware of the prob-

lems involved, and in a sense he is wise to choose farmers, businessmen, and workers as the neutral terms for his class actors. There is all too great a tendency, in South African studies at least, to impose Marxist terms on any class analysis, even though such terms may be derived from the Eighteenth Brumaire and Poulantzas rather than from a more generalized Marxism. Yet it may still be asked whether we have thought enough about who the significant class actors are in capitalist colonial societies.

A more sophisticated view might suggest that the major class actors are the following: those who deliberately set up and exploit systems of unfree labor in agriculture and mining; the secondary colonialists (such as Indians in South Africa) who take up the lesser economic opportunities; the settlers, including farmers, capitalists, and workers, who move in once the primary system of exploitation has been established, and who establish a freer economic system than that which prevails in the mines and plantations; the state officials, politicians, and ideologists; and the various groups of "native workers."

In many colonial societies the various settler groups are not strong enough to take control after independence, but in South Africa they have been strong enough. The society has been governed in effect by an alliance of the mine owners, secondary capitalists, and white workers and farmers. Although there is a class struggle between them, they all depend on the maintenance of the racial order, and any liberalizing tendencies that might be latent among, say, the settler capitalists, are subordinated to the institutions of labor unfreedom which the mines have set up. The compound is the main labor institution in South Africa. It operates in conjunction with the reserves, which have the task of reproducing labor. Its main features of unfreedom are generalized in the cities, where the "free" labor employed by secondary industry is badly housed but very efficiently policed.

There is little here, probably, with which Greenberg would disagree. Perhaps the best tribute to his book, a book based on a simple and clear research design, is that there is now a much better case for spelling out and discussing the theoretical issues it raises.

The last point to be made is that what has been discussed in this review so far are the interests and attitudes of class actors who are on the side of the exploiters. Complementary to this, and providing the basis for a more dynamic class analysis, would be an analysis of those class actors' attitudes which have yet to impose themselves on history. They include the great mass of black migrant workers and peasants, the increasingly permanently urbanized black proletariat, the small class of black businessmen and professionals, and the coloreds and Indians in all their differentiated statuses and class positions. One hopes that the South Africa Research Program at Yale will be encouraging a book complementary to Greenberg's and covering this ground. The theory of the revolution will be about the way in which the existing system, apparently so intractable at present, is forced to adapt to the demands of these class actors.

For a reviewer born and brought up in South Africa, this book comes as a breath of fresh air: 25 years ago sociologists in South Africa were still

trying to make sense of their society in some sort of Parsonian terms. Books like Greenberg's indicate that it is now recognized that one can make no sense of many of the most crucial social systems in the world unless one first understands South Africa. Perhaps too it is time that we understood that capitalism and colonialism have always been based upon violence and unfree labor. That is a lesson which both unilinear Marxism and unilinear Weberianism have prevented us from grasping.

The Road: Indian Tribes and Political Liberty. By Russel Lawrence Barsh and James Youngblood Henderson. Berkeley and Los Angeles: University of California Press, 1980. Pp. xv+301. \$14.95.

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Significantly, and alone among racial and ethnic groups in the United States, American Indians form a legally defined minority. They do so not by virtue of blood or culture—at least not directly—but by virtue of tribal membership, itself variously defined, sometimes by blood, but invariably with certain legal consequences. It is not the Indian-federal relationship which sets Indians apart in this regard but the *tribal*-federal relationship. The legal and political status of tribe and the ideological and social reality of tribalism remain today at the heart of Indian-white relations and of the Native American experience. They lie also at the heart of this book.

In *The Road: Indian Tribes and Political Liberty*, Russel Barsh and James Henderson have undertaken a monumental task. Through a detailed, historical analysis of treaties, legislation, and judicial decisions, they show how, from the end of the 18th century to the present time, the tribal-federal relationship has been defined and redefined by the Congress and the courts. Only in the period of treaty making, which Congress ended unilaterally in 1871, did Indian input regularly play any significant role in that definitional process. Indeed, the major casualty of that process has been the political liberty which Indian tribes originally brought to the negotiating table, a liberty eroded as Congress and courts have attacked, misrepresented, and steadily undermined the sovereignty of the tribes. One of the striking aspects of this process, meticulously outlined in this book, is its lack of consistently applied legal principles. The authors demonstrate compellingly the remarkable degree of confusion and inconsistency in both legislative and judicial actions on Indian affairs, and particularly in the crucial decisions of the Supreme Court, a confusion and inconsistency increasingly evident in recent years. Furthermore—and this is a critical aspect of their work—they show how the assault on tribal sovereignty, abetted by legal sophistry and historical misconception, directly contravenes those political principles on which courts and Congress themselves depend and on which American freedom was postulated.

"To us," they write, "political liberty means an effective voice in national government, *and* the right of the people to establish local governments to exercise any or all of the powers they have reserved to themselves" (p. ix; their italics). This is a notion of political liberty whose American roots, they argue, lie in the Revolution and the Constitution. Yet it is precisely this liberty which has been twisted and denied through two centuries of Indian-white relations. Legislature and judiciary have been crucial players in that denial; guns, cavalry, and the Bureau of Indian Affairs would be poor agents of sustained political domination without the prior sanction and superior efficiency of the law.

Barsh and Henderson are lawyers themselves, and *The Road* is the most important examination of Indian law since Felix Cohen's 1942 *Handbook*. It makes a sustained argument which is highly critical both of the law and of the received wisdom regarding it, an argument which includes occasionally revolutionary interpretations and some surprising but persuasive positions. (Not least among the latter is its critical attitude toward Cohen himself, the seminal figure in studies of Indian law to date.) But *The Road* is also a significant work of political sociology, the first systematic effort to trace the process by which Indian groups have been brought under the political and legal control of the United States. As such it is replete with insights, some central to the argument, others incidental to it but no less useful to the sociologist or historian. The authors' discussion of the Indian Reorganization Act of 1934 is superb, arguing convincingly that, far from restoring a measure of political autonomy to the tribes, the act reinforced and in effect legitimated federal control over them. In a refreshing analysis of the Bureau of Indian Affairs, they argue that its costs to Indian tribes far outweigh its benefits, an argument that contradicts what many Indians—however reluctantly—say. Barsh and Henderson's brief but perceptive comments on recent Indian political activism should be noted by activists and scholars alike.

The Road is likely to draw fire from other legal scholars and perhaps historians, who may find certain of its interpretations hard to swallow. My own objections, pitched more at the sociological aspects of the book, have largely to do with two concepts which are important to the work but relatively unspecified within it. Tribalism, as ideology and as social organization, is a complex and ubiquitous element in the lives of most Native Americans and forms the context within which the authors make their argument. But while they begin with it, regrettably they never return to it, and we are uncertain from their few remarks just what it is. Elusive, too, is the meaning of sovereignty, at least for Indians themselves, to whom it often has a significance more profound than any legal definition comprehends. Henderson, I know, has spoken eloquently on the topic elsewhere; it needs more attention here.

The book closes with a radical proposal for change. The authors envision a constitutional amendment creating a system of "treaty federalism" which would give tribes a status resembling that of states, guaranteeing to them extensive powers of self-government and a form of collective representation

in Congress. It is a proposal creative enough to be intriguing, radical enough to seem utterly unrealistic. It also begs a nagging question about contemporary Indian affairs. What are these tribal governments whose powers are to be guaranteed? The problems of political participation and representation which inform this book have yet to be fully resolved, not only at the federal but also at the tribal level. As formally constituted political units, many tribes are relatively recent creations. A number of tribal governments have only mixed support—or a good deal less—from their own constituents. The antagonism is not simply a matter of reservation politics. At issue, often enough, is the very nature of tribal government and tribal citizenship, the freedom not only of tribes but of various—and variously defined—Indian communities to make their own ways in the world. This is a problem Barsh and Henderson do not address; admittedly, in their present work they did not need to. But it is a problem both empirically and conceptually related to those they tackle here; given the striking success of their present effort, we would surely benefit from their thoughts on the other issue as well.

Race Relations in Colonial Trinidad 1870–1900. By Bridget Brereton. New York: Cambridge University Press, 1979. Pp. x+251. \$34.50.

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University of Chicago

Among historians and social scientists working at the University of the West Indies there has been a close and, on the whole, beneficial relationship. *Race Relations in Colonial Trinidad 1870–1900*, originally a Ph.D. dissertation, is a product of that relationship; it exhibits all the careful scholarship of the growing body of West Indian history, while at the same time its themes and overall structure are clearly derived from current (and not-so-current) sociological discussion about pluralism, social stratification, creolization, and race. If the data are more interesting than the theory, it is because Bridget Brereton has remained faithful to the historian's obligation and let the theory go when it proved more hindrance than help. The result is a valuable addition to our knowledge of complex, multiracial societies.

Sociological research in the Caribbean has concentrated on the attempt to understand how "societies" have, or have not, been created out of the polyglot assemblage of peoples who were gathered there to produce commodities for developing European capitalism. This search for an understanding of the forces making for integration was lent urgency after World War II as a new wave of nationalism swept across the colonial world. Now that colonies are indeed new states, the search is no less urgent, for the "integration" is no more assured. This problematic was crucial for Brereton, and it may even have determined her choice of period for, as she

explains on page 1, the peaceful years from 1870 to 1900 were years which saw the emergence of a nonwhite "middle class" which "began to articulate a 'national' ideology, and which held the key to the political and social future of Trinidad." Similarly her study ends on page 212 with the observation that "the story is a continuing one: the evolution of a national society."

We are given two pictures of that evolution, one self-consciously theoretical and the other a less guarded narrative. In the "theoretical" view, a slave regime was succeeded by creole society in which the white elite constituted an exclusive caste, imposing its values and culture upon a black and colored (racially mixed) middle class. This middle class, in turn, made every effort to distinguish itself from the "creole masses" who, resisting white hegemony, retained a distinct, though syncretic, culture. The large East Indian population, imported as plantation workers, was at this time unassimilated. The chapter headings reflect this theoretical scheme as they move from "The Environment" to "The White Elite" through "Education and Mobility" to "The Coloured and Black Middle Class," on through "The Urban Labouring Population" and "The Black Rural Masses" to "The Souls of Black Folk" and "The Indians" to culminate in "The Divided Society." In the end, however, Brereton is unable to decide whether Trinidad was a society structured by relations of class or one radically divided into racial segments, each with its own culture and values. Her dilemma is not unusual, and in the end she settles for blurring these two models into one when she writes, "Class, colour, caste and race combined to create an immensely complex pattern of human relationships in late nineteenth century Trinidad. . . . We cannot hope to arrive at a clear-cut or definite picture of race relations in the Trinidad of a century ago . . . but we have been able to use different types of source materials to suggest what Trinidadians of this period thought about race, and class, and colour, and to indicate how these attitudes affected their relationship with other Trinidadians" (p. 212).

The other picture of Trinidad's evolution is much more interesting; it is drawn with a historian's eye for detail and provides some of the description necessary to a more interpretative analysis. Trinidad differs from the rest of the British Caribbean in significant ways. For 300 years after its discovery it remained almost uninhabited until Spain finally encouraged the settlement of French planters in 1783. Immigration increased after 1789 as French planters with their slaves, and an unusually large number of free people of color, fled the revolution in the French islands. Although it became British in 1802, attracting some British planters, merchants, and administrators, the French influence and Roman Catholicism remained strong. It is one of the virtues of Brereton's book that she makes us vividly aware of a great many things which escape the net of her theory. There is space here to mention only a very few.

The persistence of a French creole elite tradition, combined with the fact that most of the black population spoke French patois rather than English all through the 19th century, resulted in a sharp modification of English cultural hegemony in spite of the spread of English primary

education. Continued immigration from Venezuela augmented the Spanish-speaking population, and Indian immigrants spoke Hindi, Urdu, or regional Indian languages. In 1872 Bishop Rawle observed that "the language talked in the streets is the scouring of Babel, a negro commixture of French, Spanish and African, to which for Coolies is added mashed English" (p. 164). Some of the most interesting materials are contained in the strangely titled chapter "The Souls of Black Folk"—strangely titled because much of it is about gang organization, carnival, and calypso. Other British Caribbean colonies had syncretic cult groups similar to the Rada and Shango sects of Trinidad, but if there was anything comparable to the organized bands which emerged from the slumyards of Port of Spain, it has gone unremarked. Gangs, or bands, with such names as Sweet Evening Bells, Island Builders, Don't Care Damns, Black Balls, and the Peau de Canelle are not to be explained away in terms of syncretism or safety-valve theories. Brereton explains how this urban "sub-culture" was "dominated by the 'jamets' or 'diamètres,' the singers, drummers, dancers, stickmen, prostitutes, pimps, and badjohns in general. . . . The jamets boasted their skill in fighting, their bravery, their wit and ability at 'picon,' their talent in song and dance, their indifference to the law, their sexual prowess, even their contempt for the church. In short they reversed the canons of respectability, the norms of the superstructure" (p. 166).

These data resonate in a much wider comparative context. They are not really dealt with in the theoretical discussion, but they are, surely, crucial to a more sensitive understanding of the nature and evolution of Caribbean societies, and New World societies in general.

Continuity and Change: A Study of Two Ethnic Communities in Israel. By Rita James Simon. New York: Cambridge University Press, 1978. Pp. xi+180. \$13.95 (cloth); \$4.95 (paper).

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Israeli society is divided into many contrasting ethnic and minority groups. The pressures for cultural assimilation have been powerful, and groups that stood apart have been labeled as "dividing the nation." This has been the pattern for the Jewish immigrants and their children that compose the majority of the population. Minority groups, however, have had a different relationship to the state and society.

Continuity and Change, Rita Simon's comparative study, focuses on two "non-mainstream" Israeli groups: ultra-Orthodox Jews and Israeli Arabs. Both are minorities (Israeli Arabs make up about 13% of the population, and the ultra-Orthodox less than 2%) and each differs in critical ways from the Jewish secular or "traditional" majority. Israeli Arabs do not serve in the army, for example, and they regularly face the awful

dilemma of being loyal to a state that seems in perpetual war with their closest kin. Ultra-Orthodox Jews are perched on the other edge: non-Zionist or even anti-Zionist in the Jewish state, some do not even recognize the legitimacy of that state, and all mark themselves off from other Jews. It is, therefore, a fine stroke of sociological imagination to contrast these two "non-mainstreamers." How do they succeed in maintaining their separate-ness? Is there harmony or dissensus across the generations? Are their distinctive patterns likely to weaken over time? These are some of the important questions that Simon poses in this intriguing study.

The book is divided into two general sections: the first half summarizes background data regarding the two groups, and the second presents the interview material that forms the core of the study. In the first section, Simon succeeds in presenting the different contexts of the two minorities in a straightforward, convincing manner. For example, we learn that the ultra-Orthodox Jews living in Jerusalem's Mea Shearim district refer to their neighborhood as the "Jewish section" of the city, and that local schools take pride in teaching students "nothing that is new" (pp. 58, 61). Similarly, she notes the inherent contradictions faced by Israeli Arabs; how, she asks, "Can the Israeli educational system make 'Israelis' out of Arab children and still have them retain their Arab-Muslim (or Christian) identity?" (p. 67). The main point that Simon stresses is the paradoxical relationship to the Jewish secular majority: Israeli Arabs are endeavoring to be assimilated into a Jewish society that suspects or rejects them, while the ultra-Orthodox Jews adopt an elite stance and purposely distance themselves from the Jewish majority that, in principle, is prepared to accept them. Paradoxes indeed!

The main contribution of the study, however, rests in the interview data in the second half of the book. Simon's research team interviewed a sample of families in the two populations; these included fathers and sons and mothers and daughters. The structured interviews ranged over a variety of topics, focusing mainly on attitudes toward religious practices, the media, and images of the future, as well as political orientations and work experience. Interviewing Arab families did not pose special problems. However, the ultra-Orthodox Mea Shearim community is notoriously difficult to study, and the interviewers had to overcome serious obstacles in order to obtain reliable information.

What was learned from the interviews? Simon's analysis focuses on similarities and differences between sexes and across generations. The tables show, for example, that religious females have closer contacts with secular society than do religious males, and also greater exposure to the mass media. Arab women, on the other hand, have fewer contacts with Jews and hence are deemed to be more traditional than their husbands. These survey results are more or less expected. The intergenerational contrasts are more interesting. Most striking is the high degree of consensus among religious parents and their children: Simon's data show that the ultra-Orthodox minority has remarkable success in socializing youngsters into their closed community. The young perceive themselves as having an elite status among

secular Jews; they feel they are an elect group and they seem firmly tied to the community's traditions. While there is also a high degree of continuity among Israeli Arabs, younger males appear to have more nationalist political sentiment than their parents, and they are also critical of the older generation's restricted occupational pattern. Their aspirations stretch further than do those of their elders.

These are important facts, and they provide a solid understanding of the aspirations and attitudes of these two minority groups. The study is careful and intelligent throughout: scholars interested in these two particular groups, or in the general issue of "non-mainstream minorities," will read the book with profit. One finishes the book, however, with a faint sense of disappointment. What is absent is a certain broader range, a fleshing out beyond the survey data. For example, there is hardly any sense of variation, and none at all of deviance, in the presentation. Such uniformity within families and generations seems unlikely, and one wishes for a fuller range of expression. There is also a marked absence of generalization. Early in *Continuity and Change* Simon writes that the two minorities were selected for study "because of the important and distinctive roles they have played and are likely to play in defining the quality and characteristics of Israeli society as a whole" (p. 29). This might have been a central and highly relevant point—yet the author never comes back to it. Selected discussion of broader topics such as this would have added dimensions to this otherwise well-crafted study.

The Unexpected Rebellion: Ethnic Activism in Contemporary France. By William R. Beer. New York: Columbia University Press, 1980. Pp. xxxii+150. \$18.50.

Katherine O'Sullivan See

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In this tantalizing but ultimately disappointing book, William Beer raises important theoretical questions about the recrudescence of ethnic activism in advanced industrial societies. He provides, as well, a lucid review of the historical and modern manifestations of such activism in ethnolinguistic regions of France. But the analytic power of *The Unexpected Rebellion* is undermined by the author's conceptually muddled use of theory, by methodological problems, and by occasionally contradictory argument.

The historical discussion of ethnic activism is trenchant. Beer deftly destroys the myth of a culturally homogeneous France by pointing out the geographic artificiality of state boundaries and the considerable overlap of language groups across political divides. His comprehensive summary of the incorporation of ethnolinguistic groups and regions into the French state (through conquest, annexation, and policies of centralization and linguistic standardization) is rich in detail. His succinct discussion of variegated

forms of ethnic activism (from the linguistic revivalism of the Occitan Felibrige to the Nazi collaborationism of Breton nationalists to the bombings by Corsican autonomists) reminds the reader of the complex and mercurial nature of ethnic politicization. The author attempts no systematic comparison of these patterns, except to find a common thread in resistance to state centralization and linguistic chauvinism. Historical comparative analysis is not the intent of this book; instead, the author seeks to explore the recent sources of ethnopollitical and cultural activism.

Beer appropriately eschews singular explanations, arguing that ethnic activism is rooted in global changes, shifts in French government policy, and the variable modernization of individual regions. Specifically, he contends that the rise of supranational agencies (NATO, EEC) and the penetration of multinational corporations have weakened the significance of state boundaries. At the same time, the decline of French colonial power (in Indochina and Algeria) and of central control at home (after the 1968 May Days), together with the decentralizing policies and cultural pluralist rhetoric of the government, have challenged the paradigm of an indivisible nation-state. If true, none of this explains why activism is articulated in ethnic terms. To answer that question, Beer turns to the theoretical literature on ethnicity.

His critique of classical and contemporary modernization theories will be familiar to students in this field. What I find perplexing is Beer's effort to utilize an internal colonial model, even though he demonstrates expertly the difficulty of applying it to France. He acknowledges that some of the less developed French regions are *not* ethnically distinct, whereas some *ethnic* regions are highly developed; but he counters that the model is still relevant, for it explains the preservation of ethnic inequality. Yet his use of the internal colonial framework is sloppy; unlike Hechter or Nairn, Beer provides no comparative data on ethnoregional inequality, no evidence of specific state policies designed to sustain unequal development or to support an existing cultural division of labor. Although Beer faults other authors for fuzzy application of the term, in his own scheme, "internal colonialism" becomes merely a surrogate for uneven economic development.

Dressed in the rhetorical garb of internal colonialism, the substance of this book adheres to pluralist modernization approaches. Beer argues that rapid social change has produced a rise in popular expectations and that violent ethnic protest is more prevalent in areas that experience lower rates of economic growth. He tests this hypothesis by measuring the correlation between regional rates of economic development and various forms of ethnopollitical militancy. His findings are unstartling: extraelectoral dissent is higher in relatively underdeveloped areas; electoral political ethnicity is more characteristic of developed regions. However, Beer errs in collapsing all nonelectoral activities into a single category. Postwar Corsican activism has been dominated by regional and separatist organizations, whereas Occitanian activists have focused on cultural revivalism. To consider these patterns equivalent gives us little insight into the sources of differential

regional politics in areas whose rates of economic development are not radically different.

The major problem I find in this book concerns Beer's treatment of ethnic activist leaders. Here, his acceptance of modernization theory is undisguised. He claims that the ethnic leaders are motivated by social anomie, "assuaging feelings of dislocation, resulting from rapid social change and the experience of rapid social mobility" (p. 91). But, in surveying ethnic activists, Beer overestimates the extent of social mobility (his measure of anomie). Of his sample, 55.4% are located in the same general occupational status levels as their parents—only 5% less than the populace as a whole. Moreover, because he provides no comparative data on nonethnic political activists (or on nonactivist ethnics), his claims about the *particular* anomie of the sampled leaders are unwarranted. Finally, his argument that these activists were motivated by disruption in (or devaluation of) the traditional order is contradicted by his own findings (the majority come from francophone homes and were attracted to activism by friends and teachers, not by negative experiences of chauvinistic metropolitan education).

Despite these flaws, Beer's work is a welcome addition to the comparative literature on modern ethnicity. *The Unexpected Rebellion* contains a fine historical overview and useful data about contemporary French politics; it is a literate and provocative introduction to an important research area.

African Businessmen and Development in Zambia. By Andrew A. Beveridge and Anthony R. Oberschall. Princeton, N.J.: Princeton University Press, 1979. Pp. xv+382. \$22.50.

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Two loosely connected themes hold together *African Businessmen and Development in Zambia*. One theme is the political context and consequences of President Kaunda's campaign for Zambianization of enterprise. Initiated in 1968, this campaign was intended to restrain Asian and European business enterprise at every level while encouraging Africans to enter business. In intent these reforms mirrored Idi Amin's brutal assault on Uganda's Asians, but Kaunda's means were gentler. How successful was Zambianization? This is an important problem for students of middleman minorities and, indeed, for anyone seriously interested in organic solidarity. Four years after Kaunda's reforms began, Zambianization's effects were principally visible in rural areas where approximately one-half of European and Asian enterprises had been taken over by Africans, one-quarter had closed, and one-quarter continued in "expatriate" hands (p. 251). In Lusaka, Zambianization had a negligible effect in first-class areas of town, and only a slight effect on second-class areas (p. 250). Only 7% of Lusaka businesses were owned by Africans in 1972; 93% still belonged to expatriate owners.

Why did the political reforms not have more effect upon the ethnic composition of Zambia's business population? The most interesting section of the book describes the manner in which Asian and European business owners frustrated the working of the law by legal subterfuges. One device was that of reporting business ownership in the name of Zambian-born children with citizenship. This device reminds one of the way Japanese Americans evaded the Alien Land Law in California. A second device was that of hiring Zambians to front for the business. This reminds one of affirmative action fraud during the Carter presidency: to get government contracts, white businessmen hired nonwhites to front as owners. A third device was application to the government for special exemption. Certain Zambian officials owed favors to expatriate business owners, some of whom had made generous contributions to their election campaigns. Therefore, many expatriate businessmen easily secured exemptions from Zambianization laws.

Do these machinations completely explain the frustration of Zambianization? Perhaps the entrepreneurial unpreparedness of Africans also inhibited the reform? Andrew Beveridge and Anthony Oberschall do not raise the latter question in exactly these terms, but they appear to be arguing against such an explanation. For the second and much-stressed theme of their book is the utter responsiveness of entrepreneurial supply to enhanced opportunity. Lauding Schumpeter, the authors proclaim that cultural factors cannot explain the "rush" of Africans into business following the Zambianization campaign. Obviously the number of African businesses did increase and the presumptive cause was the abruptly enhanced opportunity resulting from political inhibition of "expatriate" competitors. On the other hand, the "rush" was closer to a crawl in urban areas, and even in rural districts the rush of Africans into business produced a net decline of one-quarter in the business population.

However, the authors adduce a second, much more heavily stressed body of evidence bearing on this issue. Their multivariate analysis of African business owners' monthly incomes revealed negligible relationships with religion, ethnicity, or any cultural factor. "Contrary to theories that suggest the importance of cultural factors, every major Zambian ethnic group contributed to trading and did so roughly in proportion to its size" (p. 101). The major correlates of monthly income were sponsorship, previous experience, and, above all, innovative business practices. Reaching for an overall conclusion, Beveridge and Oberschall declare: "Sound management and competitive position are the keys to business success" (p. 298). These keys are within the reach of anyone regardless of "ethnicity and parental social class." Entrepreneurship is "an elusive, unsystematically distributed talent" (p. 225). Joseph Schumpeter pioneered the view that entrepreneurship depends on innovation and that innovative personalities are equally distributed in every human population regardless of class or ethnic characteristics. The authors treat their findings as a vindication of Schumpeter's economic view and even proceed to question (pp. 288-93) some of the very extensive literature which has reported the contrary. "Findings about

ethnic differences in business success tend to be weak or nonexistent and the few observed differences have alternative interpretations" (p. 293).

These conclusions are unsound not only because they are internally inconsistent with evidence presented, but also because they are overgeneralized and compatible with alternative interpretations. First, the method of analysis is loaded in favor of an individualistic conclusion. For example, the authors' definitive measure of business success is individual income, a variable that treats individuals rather than groups. Suppose the aggregate income of groups were the dependent variable. Would the results be the same? Second, business survival is a more basic measure of business success, and business survival yielded a completely opposite and un-Schumpeterian result. Looking at survival with longitudinal evidence, the authors "found that innovation actually contributed to failure and that survivors were disproportionately less innovative" (p. 225). Business survival is closely linked to size of the African business population, a variable they ignore. They ask: Which African individuals are most rewarded? If one asks instead why the African business population is not larger, one is also asking why the group's money return from business is not bigger. Too much innovation is the only answer one could derive from their evidence.

But what about the negligible relationship between cultural variables and individual money returns? This is dubious for two reasons. First, the measure of ethnicity is spurious. Their variable is regional origin, and regional origin is a potpourri of ethnic backgrounds. Thus, their "ethnic groups" of Eastern, Northern, and Central origin each include eight distinct linguistic and tribal units which have been aggregated for analysis (p. 133). This is analogous to treating the ethnic groups in the United States as the Northeast, the Middle West, the South, etc., rather than as Italians, Greeks, Serbs, Mexicans, etc. Small wonder they found no relationship with ethnicity! Second, their data show more intergroup differences than their later conclusions reflect. "Quite clearly, however, there was a striking overrepresentation of non-Zambians and to a lesser extent of Northerners, and a concomitant underrepresentation of Easterners" (p. 133). Does this description fit a Schumpeterian country with no intergroup differences in entrepreneurship?

Finally, the ethnic evidence they adduce is dubious because it is incompatible with other reports. For example, they found no ethnic neighborhoods in Lusaka: "African residential areas in Lusaka do not have distinctive ethnic characteristics in the sense that members of one ethno-linguistic group form a controlling majority in a neighborhood" (p. 98). If there are no ethnic neighborhoods in Lusaka, perhaps there are no ethnic groups there either, in which case the absence of any association between ethnicity and entrepreneurship would be fully explained by the absence of ethnicity. On the other hand, it seems unlikely that no ethnic neighborhoods exist in Lusaka. Such an absence would be so incompatible with the known proclivity of Africans to form ethnic neighborhoods that it leads one to wonder whether Beveridge and Oberschall's ethnic data do not contain serious flaws somewhere. If the authors next concluded, on the basis of this questionable

evidence, that reports of ethnic neighborhoods in other African cities had been overstated, readers would accuse them of jumping to an erroneous conclusion from a dubious basis in evidence. Yet this is precisely the conclusion to which they jumped on the basis of the statistical independence they found between cultural variables and individual money income in Zambia.

The New International Division of Labour: Structural Unemployment in Industrialized Countries and Industrialization in Developing Countries. By Folker Fröbel, Jürgen Heinrichs, and Otto Kreye. Translated by Pete Burgess. New York: Cambridge University Press, 1980. Pp. xvii+406+19. \$54.50.

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The realization of a truly global economic system embracing the advanced industrial countries (AICs), the less developed countries (LDCs) and, increasingly, the centrally planned socialist bloc means that the logic of capitalist development is working itself out at the international level. Increased competition among capitalist producers in the form of transnational corporations (TNCs) over this new global market, combined with the nature of production processes for technologically sophisticated manufactured goods, drives business to pursue a new and truly international division of labor. The result is a global reorganization and relocation of industrial production away from the AICs (resulting in increased structural unemployment) and into LDCs (resulting in low-wage, labor-intensive production). Thus the traditional division of labor, whereby LDCs exchange unprocessed materials for the manufactures of AICs, has been transformed, and LDCs are becoming major sites of industrial production. Although these claims will seem old hat to some, Folker Fröbel, Jürgen Heinrichs, and Otto Kreye have organized them around a simple but often compelling theoretical formulation and have provided impressive documentation of the extent and impact of the new international division of labor (NIDL).

The authors of *The New International Division of Labour* intend their theoretical analysis to rest on Marx's discussion of capital, but its essential parts are derived from the analysis of the division of labor by the early political economists, especially Adam Smith and Charles Babbage. From Smith they draw the argument that the impact of improved transportation and communication has been to create a (global) market sufficiently large to support an expanded division of labor, with the associated increases in productivity and hence in aggregate output. From Babbage they draw the observation that, as complex tasks are divided into many steps, the individual steps are themselves rudimentary and can be performed by unskilled labor. Thus, at the same time that output is increased through improved

productivity, costs are reduced by the substitution of low-wage unskilled labor from the LDCs for the more expensive labor in the AICs. Competition among transnational corporations to minimize production costs and increase profits leads them to relocate production in LDCs where surplus pools of cheap labor are available.

This theoretical argument serves as an organizing framework for the orgy of data which follows. Extensive detail is provided on three case studies covering (1) changes within the West German textile and garment industry as production shifts abroad; (2) general patterns in the shift of German industrial production and employment abroad; and (3) the impact on LDCs, with an emphasis on free production zones. These are truly raw data, and they come in large clumps. Typical examples include 12 pages of selected quotations from business sources on the attitudes of TNCs to foreign manufacturing operations (chap. 6.4), a 27-page compilation of German corporate activities abroad (chap. 11.3), and a seven-page sampler of the incentives offered in free production zones (chap. 14.1). There are also 57 tables for those who prefer their data in more quantitative form. All this can make for tedious reading, but judicious skimming provides a rich and comprehensive documentation of the transformation of German production. Lamentably, no index is included to make access to specific details easy.

Although this compilation of data is useful, *The New International Division of Labour* is disappointing in terms of what the authors do—or, rather, fail to do—with all of their facts. First, there is in no sense a rigorous test of the theory. Instead its correctness is assumed and used as a guide to interpreting the voluminous data. Alternative explanations are not considered, and what appears to be contradictory evidence is either ignored or else subsumed under the theory by assertion. Some readers may find it hard to swallow the implication that German investment in the United States is motivated by the availability of cheap labor in precisely the same way as is investment in, say, Taiwan (p. 251). Others might suspect the selective nature of the evidence for the claim that labor productivity in LDCs universally approaches European levels within months (pp. 355–57). Everyone should marvel at the flexibility of this theory in explaining even capital-intensive production in the Third World (p. 328). Such awkward stretching of the theory is unfortunate because it detracts from its usefulness in analyzing current developments in the international economy.

The second and related problem is that the authors do not use their empirical evidence to refine or expand the scope of the theory. Despite the richness of their data, nowhere do they build on their initial formulation in an integrated fashion so that we might better understand why different countries or industries are differently incorporated into the international economic system through the NIDL. For example, the authors gloss over the fact that among LDCs only a handful—the so-called newly industrializing countries—account for the overwhelming proportion of LDC manufacturing output and exports. It would seem reasonable to expect an explanation as to why the *international* division of labor has actually involved

only a very few countries and why even these countries show strong differences in the nature of their incorporation into the world system (e.g., the export profiles of South Korea and Taiwan suggest a very different portfolio of production than do those of Brazil or Mexico). Similarly, the authors assume that the advanced countries are purely passive actors in this process (e.g., pp. 180-81). They ignore the possible impact of certain policies, such as recent ones increasing trade protection, on the development of the NIDL. Thus, even if they have correctly described the long-run destiny of international capitalism, the authors have done remarkably little in exploring the various paths by which different countries may arrive at it. But despite this shortcoming, the extensive detail about the impact of international capitalism on both AICs and LDCs, and the discussion of the logic underlying the NIDL, will make this volume a useful reference for those interested in processes of national economic development in the context of a global economy.

Protest and Participation: The New Working Class in Italy. By John R. Low-Beer. New York: Cambridge University Press, 1978. Pp. xviii+285. \$16.95 (cloth); \$4.95 (paper).

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During the 1960s, social scientists in the advanced capitalist countries began to notice the rapidly rising importance of technicians and other white-collar but still dependent workers. Serge Mallet and Andre Gorz, among the most influential thinkers on the subject, interpreted unprecedented participation in strikes and political mobilizations as an indication that the new class of white-collar workers was destined to revitalize militancy rather than end class struggle. Mallet and Gorz held that technicians especially were placed in a contradictory position, caught between the rational ideals of science and oppressive, irrational, and nonparticipatory workplace organization. Efforts to resolve this contradiction would lead, they thought, to class-conscious mobilization. While the new working class would not seek instantaneous transformation of all of society, it would pursue a program of "nonreformist reforms" which would be no less revolutionary in its ultimate consequences. Daniel Bell and Alain Touraine, on the other hand, argued that the organization of work was no longer the primary determinant of political allegiance or opinion, let alone of self-identity. John Goldthorpe and David Lockwood stressed the affluence of the new working class as the primary source of its orientations toward social life and political action. Gorz and Mallet deprecated the role of pay and stuck to an emphasis on the work itself and the workplace. Into this set of disputes John Low-Beer has introduced a solid and pointed empirical study.

Protest and Participation is set in Italy, which Low-Beer holds to be sim-

ilar enough to the other advanced capitalist countries to permit generalization. He finds evidence of the same rising importance and militancy of white-collar workers which prompted Gorz and Mallet to rethink theories of working-class political action. Low-Beer's aim is somewhat narrower, though; he focuses the bulk of his attention on the hypothesis that a contradiction between the structure of tasks and that of organizations is central to the activity of the new working class. To test this hypothesis, Low-Beer drew a sample of technical workers and work settings from two large electrical concerns in the Milan area. This sample had the advantage over a number of other studies in the field that it permitted attention to specific workplaces, rather than generalizations about whole firms or all white-collar technical workers. Even more important, one of Low-Beer's companies had introduced a highly participatory management scheme in some of its operations, while the other was more conventionally hierarchical.

Low-Beer's research indicates that incongruence between characteristics of tasks and of organizational settings is indeed important in predicting discontent. However, in contrast to Gorz and Mallet, Low-Beer finds this incongruence to be important whether the organization is participatory and the job not involving, or the job involving and the organization bureaucratic. In the latter case the incongruence leads to the sort of creative, work-oriented militancy described by the new working-class theorists. The former sort of incongruence—participatory organization and noninvolving work—leads to more straightforward, less creative demands for freedom from close supervision, demands unaccompanied by any claim to do the job better. In fact, the introduction of participatory organizational schemes is likely to exacerbate tensions between employers and those workers in relatively uninvolved, closely controlled jobs.

Perhaps more important, Low-Beer argues that Gorz and Mallet overestimated the importance of work and workplace issues and underestimated the importance of "preconditions" for militancy. In particular, Low-Beer finds changes in the structures of opportunity for technical workers crucial as university degrees become essential for promotion or industrial or national economic deterioration threatens job security. Characteristics of workers' lives outside their jobs are also significant, with marital status and family background differentiating among workers in similar jobs. For example, married workers in Low-Beer's sample are much more likely to fit the Goldthorpe/Lockwood model of privatization and narrowing of involvement in class-based aspirations and actions. Political socialization exerts a further significant influence, with workers showing a high degree of continuity across generations in their political attitudes and orientations toward collective action. Some 60% of the technicians in Low-Beer's sample were of working-class or peasant origins. He suggests that this is a major determinant both of their propensity to see basic disjunctures between workers and management and of their willingness to engage in collective rather than just individual struggles.

Low-Beer uses a variety of methods to address questions of attitudes toward jobs, satisfaction with work conditions, ambitions in careers, and

other important issues. Nonetheless, *Protest and Participation* is very definitely a monograph; indeed, it reads more like a very long journal article than like a book. Despite opening and closing chapters on the new working-class debates, the book does not attempt to advance theory. Certain propositions are tested, and a number of theories and ideas are brought into play to support the tests or explain their results. Instead of developing an overall argument, however, the middle five chapters which form the meat of the book consist of a series of more or less discrete observations and comments. Low-Beer's book is about protest and participation only as factors in a fairly narrow debate within industrial sociology; he differs from other writers who wish to set the new working class in a much broader analytic context. Indeed, although Low-Beer uses as points of departure certain Marxist arguments of the 1960s, he makes no effort to follow developments in Marxist thought into the 1970s. Low-Beer's disinclination to go beyond his immediate research project can be forgiven, though, since the major aim of *Protest and Participation* seems to be simply to refine the empirical basis of arguments about the new working class and the affluent worker and to extend the discussion to Italy. If one asks of the book no more than this one will not be disappointed, for it accomplishes its task in a careful, capable way.

Women, Work, and Family. Edited by Frank L. Mott. Lexington, Mass.: D. C. Heath & Co., 1978. Pp. xii+153. \$15.95.

Working Women: A Study of Women in Paid Jobs. Edited by Ann Seidman. Boulder, Colo.: Westview Press, 1978. Pp. xix+217. \$15.00.

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These books have one thing in common—a focus on women's experiences in the labor force—but they differ considerably in style, methodological approach, content, and quality. *Women, Work, and Family*, edited by Frank Mott, is a compilation of six articles about the interrelationships of socioeconomic background, education, family events, and work among a sample of women entering adulthood. The chapters were prepared by a group of economists at the Center for Human Resources Research (Ohio State University) and are basically geared toward an academic audience. However, policy-minded readers may find a smattering of useful information in chapter 8, which is only four pages long and is titled, "Highlights of the Volume and Some Policy Implications."

Though much less sophisticated technically than Mott's book, *Working Women*, edited by Ann Seidman, is a cumbersome piece to plow through. The first part reads like a glorified census report, chock full of tedious facts from published volumes. Few readers are likely to move beyond the first

section: probably only those who expect Seidman to deliver on the promise to address systematically the "causes of the many obstacles thwarting women's advance in the paid labor market" (p. 173). Unfortunately, with such an ambitious, albeit respectable, purpose, Seidman can only disappoint the reader, and that she does.

Seidman's work is of the applied research genre. It is intended to help us understand how to design and implement effective action programs to improve women's labor market outcomes. *Working Women* is the product of a regional pilot project organized to overcome the obstacles to women's advancement in the world of paid work. Part 1, which examines the New England economy and public policies as they affect women workers, is a dreary and monotonous statistical overview of the project setting, including a discussion of the social institutional context. The remaining parts are slightly more palatable, but not very novel. Particularly disappointing is the discussion of barriers confronting wage-earning women in New England, because this section restates widely known facts which sound almost like rhetoric. For instance, the authors write that welfare mothers constitute a group with few rights and little ability to push for better status in the paid labor market. There is no serious analysis of this fact, nor any new insights in the discussion. Later, readers are reminded that institutionalized societal practices function at almost every level to sanction and promote the myth that women should fulfill themselves through home and family. Ho hum. Add racism to sexism and we have double jeopardy. Again, not too exciting, except to generalists who need sources to back up the obvious. Part 3 provides more specific details about the problems confronted by service workers, clerical workers, blue-collar workers, and managerial/professional types; but like the previous two parts, it is largely an exercise in beating dead horses.

The need for restructuring the world of work is discussed in terms of the need for (1) full employment, (2) full and creative participation of women at all levels, and (3) unity among all women workers. Few would quibble with these ideals, in principle, but everyone will wonder whether and how they can be achieved. With the bad taste of the ERA struggle in our mouths, it is unlikely that strategies for goals 2 and 3 will even enter political platforms. This realization did enter the authors' minds, and the book concludes on a rather pessimistic note: "It will be difficult to alter the institutionalized practices that tend to coerce women into low-paid jobs as long as unemployment and hierarchical employment structures dominate the world of paid work" (p. 174).

The papers bound in the volume edited by Mott are unified by their focus on a very specific population group (American women aged 14-19 in 1968), a common data set (National Longitudinal Surveys of Young Women, 1968-73), and a clear economic orientation which guides the conceptualization and empirical execution of the analyses. Mott's introductory chapter attempts to place the six articles in *Women, Work, and Family* into a broader frame of reference by describing changes in household and family status between 1968 and 1973 and comparing changes in the labor

force participation of black and white women during this period. He also justifies the importance of the study population, arguing that it represents a group undergoing critical life-cycle transitions, including leaving high school, entering the labor market, forming permanent unions, and child-bearing, all during a period marked by major changes in opportunities and alternatives for women. Accordingly, the authors of succeeding chapters analyze the implications of several such transitions for the labor market-related outcomes of the cohort for young women. Specifically, the six chapters following the introduction are concerned with (1) factors shaping desires for college attendance; (2) the labor force dynamics associated with labor market withdrawal and reentry due to pregnancy; (3) occupational sex typing and occupational preferences; (4) work attachment, human capital investment, and earnings; (5) migration and labor market outcomes; and (6) the causes and consequences of marital breakdown. I will not tell you my favorite.

The general shortcomings of these analyses derive from the sometimes overly narrow economic focus, the use of a short (five years) time interval to measure change, and the occasional crude measurement of key variables (e.g., experience, in the chapter on work attachment). Sociologists will fully appreciate how the authors are constrained by their attempt to force their data into limited economic paradigms. For example, Steven Sandell finds that the decisions of young women to enter college are consistent with the economic investment model but acknowledges the importance of socioeconomic background, including parental educational attainment, as a determinant of educational decisions of young women. (This is hardly a surprise to sociologists.) However, Sandell's chapter contains insufficient substantive discussion of the racial differentials and of the changes in the pattern of relationships between 1968 and 1970. These changes are dismissed as resulting from higher inflation, unemployment, and interest rates during the latter period—that is, from cyclical fluctuation. My point is that greater familiarity with the sociological literature on socioeconomic background and achievement would have helped flesh out the substantive meaning of the results; but, curiously, the many pertinent studies of Duncan, Sewell, Hauser, and Featherman are not acknowledged.

Although there are several technical flaws in the various chapters, most are not serious enough to discuss in this review. For the most part, the analyses are methodologically sound and competently executed, although not all chapters are equally strong. Had I edited this book, I would have excluded chapter 7, which deals with the causes and consequences of marital breakdown. This is not to say that the topic is trivial or that it should remain the domain of demographers and sociologists. This topic is critically important in these days of increasing concern over the rising incidence of female-headed households. However, because the authors are less able to make a compelling case for the salience of economic factors, the analysis is rendered bland and indecisive. As an alternative to omitting this chapter, I would recommend that the authors focus on the *economic* consequences of marital disruption. That is, given that marital disruption occurs, the

focus might be on how it affects the labor market behavior of women. As it stands, this chapter adds little new or interesting information. Most of us already know that marital disruption is accompanied by sharp declines in family income, increased receipt of welfare payments, increased desire for job-related training, and, among some groups, higher rates of labor force participation. Here too, it is frustrating to see economists continue to ignore important sociological contributions, but at times, they must fall over their own feet before they realize they are ignoring them.

Changing Places: Men and Women in Transitional Occupations. By Carol Tropp Schreiber. Cambridge, Mass.: MIT Press, 1979. Pp. xiv+244. \$15.00.

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With the pressure of affirmative action legislation, small but significant numbers of men and women are moving into jobs traditionally regarded as the territory of the opposite sex. Although there have been studies of the changing sex composition of occupational groups over time, and of particular groups of men and women who have entered sex-atypical occupations without the benefit of affirmative action plans, there has been almost no work on the personal and interpersonal results of organizations' affirmative action plans. Carol Tropp Schreiber, in *Changing Places: Men and Women in Transitional Occupations*, has made a start toward filling this gap. In cooperation with a large northeastern firm that had had an affirmative action program in place for two years, she interviewed 25 nonmanagement employees from each of four categories: women in four predominantly female jobs, women in four predominantly male jobs, men in (the same) four predominantly female jobs, and men in (the same) four predominantly male jobs. In addition, she gathered information from the supervisors of the 100 respondents and had access to archival data on the firm.

The motivating hypothesis of the research is that those in sex-atypical jobs are in positions of stress because of their differences from other members of the work group (differences accompanied by social isolation and lack of identification) and also because of their general lack of familiarity with both the job and the social structure of the work group. However, Schreiber did not find much evidence to support this hypothesis. There were few differences between those in sex-typical and sex-atypical jobs in their satisfaction with the intrinsic and extrinsic rewards of the job, in tension and anxiety associated with the job, and in self-evaluation of fit with and performance on the job. What differences did exist showed that those in sex-atypical positions were more satisfied with their work than those who were sex-typical. In fact, being a member of the work group's "minority" sex seemed to counteract the depressing effect on job satisfaction of being in the two least-satisfying jobs. Schreiber hypothesizes that this result, surprising in view of her initial hypotheses, could indicate that

those in sex-atypical positions have found a particularly good match between themselves and their jobs (having gone actively to jobs beyond those stereotyped as appropriate to their sex). It could also indicate that they feel more pressure to become more involved in the jobs because of their token status and that their greater commitment leads to greater satisfaction. Finally, it could indicate that the two negative stresses, being atypical for a job and being in a job that is in any case stressful, combine to produce a positive effect.

Examining the entry of men and women into sex-atypical positions from both their own perspectives and those of their sex-typical coworkers, Schreiber found general acceptance of the sex-atypical worker by both the supervisor and other workers in the group. But she also found initial apprehension on both sides, teasing (especially of women in typically male positions), and feelings of pressure on the part of the atypical worker to do the job especially well.

Finally, Schreiber turns to the implications for individual careers of holding a sex-atypical job. Men moving into jobs usually held by women—all four of which were clerical—viewed them as opportunities for promotion to management positions, whereas women in typically male positions (one of which was being phased out of the firm) saw less of a future with the company and were less likely than those in any other group to discuss future work plans. What makes this difference especially interesting is that empirically the clerical jobs were *not* stepping stones upward for men in them. Schreiber speculates that attitudes toward one's future in a sex-atypical position are a result of a feeling about where one is versus where one "should" be. A woman in a male job may feel she is higher than she should be and thus focus more on proving competence in that job than on moving still further upward. However, even if a clerical position represents an upward move for a particular man, his position remains lower than that of the average white-collar male, and he is more likely to aspire to additional upward movement. The author's emphasis on the part that an atypical job plays in its holder's career is a valuable one.

Schreiber's book, while exploratory, offers a number of valuable insights and includes examples of what happens to individuals who participate in an affirmative action plan, either by taking a position atypical for their sex or by working with co-workers who deviate from the norm for their sex. The book could have made an even more valuable contribution, however, if it had developed a coherent conceptual framework for the study of the matching of people and jobs. Such a framework could begin by developing reasons why people enter jobs in which they are different. Indeed, one problem with *Changing Places* is that one is forced to ask why anyone would enter a sex-atypical position if such positions are hypothesized to be so stressful and are entered with apprehension; and one does not get much of an answer until the analysis of respondents' beliefs about the impact of these positions on their careers. The conceptual framework could then go on to develop hypotheses about the reactions of both the new entrant and the other work-group members when the new member is different in some

obvious way, and to make suggestions about the progress of the integration between the new and predominant workers. The actions of the firm in implementing affirmative action should be part of the conceptual framework as well. What sorts of pressures does management exert on people to choose atypical jobs and on those who work with those who are atypical? To what extent are firms reorganizing job ladders for those positions that are targets of affirmative action programs? It is almost as though the author felt that she had to recreate for the reader the process through which she went in developing her hypotheses and interpreting her findings, instead of drawing on what she had learned to offer the reader a coherent framework from the start. What is especially frustrating is that the pieces for such a framework are almost all here.

Similarly, Schreiber's preoccupation with a change in her initial research design is irritating. Originally, she had planned to compare men and women in two positions, one predominantly female, one predominantly male. She then found that the male position was being phased out for technological reasons. Using only the original two positions would have confounded stress from being sex-atypical with stress from the threat of job loss. Therefore, she expanded her design to include a larger number of positions and in the analysis included type of job as a controlling variable. Her recounting of this problem is valuable in that it reminds one to pay attention to the context in which a social experiment such as affirmative action takes place. It does not seem to warrant fairly extensive philosophical discussion about whether one should admit one's "mistakes." Again, it seems as though the author wanted to carry the reader through the whole process of the research instead of organizing her presentation in a more theoretical way.

Despite these shortcomings, this book should provide a useful starting point for those interested in the personal-interpersonal and organizational consequences of breaking the barriers to equal opportunity in the occupational structure.

Female Labor Supply: Theory and Estimation. Edited by James P. Smith. Princeton, N.J.: Princeton University Press, 1980. Pp. x+384. \$20.00.

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Since the early 1960s, labor economists have emphasized increasingly the family context in which economic decisions are made. The work of Gary Becker, Jacob Mincer, and others demonstrated that viewing individuals as acting to maximize their *families'* welfare rather than their own yielded richer insights into the constraints and incentives facing them. Closer scrutiny of the family brought with it the recognition that the quantity of labor supplied to the market by husbands and wives is determined jointly with the family's consumption, saving, and even its size. Much applied work

focused on the labor supply of wives, partly because of the dramatic rise in their labor force participation during this century. Furthermore, since the labor supply of married women tends to be more sensitive to the economic circumstances of the family than does the labor supply of men, it was believed that much of the labor force response to proposed welfare reform and income maintenance schemes would come about through adjustment in the labor supply of wives. (With a labor force participation rate of about 50% as opposed to the much higher rates for men and single women, married women have ample room to respond in either direction.)

The papers in *Female Labor Supply*, edited by James P. Smith, concentrate on three methodological issues that have arisen in attempts to estimate with greater care the parameters of labor supply functions. The first issue is the so-called selection-bias problem. The problem arises when the selection rule that generates a sample used to estimate a behavioral relation is not itself independent of that behavioral relation. The papers by T. Paul Schultz ("Estimating Labor Supply Functions for Married Women"), John Cogan ("Married Women's Labor Supply: A Comparison of Alternative Estimation Procedures"), and James Heckman ("Sample Selection Bias as a Specification Error") all address the selection bias in estimates of labor supply functions for married women. The specific problem here is that one is proposing to infer the behavior of the entire population of married women from a sample of married women who have found their market wage to exceed the marginal value of their nonmarket time when they do not work.

The papers by Schultz and Cogan were written earlier than others in the volume. Schultz suggests nonlinear procedures (Logit and Tobit) as alternatives to ordinary least squares in estimating labor supply functions. These procedures correct for the truncation of hours worked at zero and provide a means of estimating the response to market wage increases both by women currently out of the labor force and by those currently working. Cogan shows that if the relationship between observable characteristics and the value of time in the household is nonstochastic or if the relationship between observable characteristics and market wages is nonstochastic, then labor supply functions free of selectivity bias can be obtained easily. His results suggest that most of the differences in the labor supply of women with identical characteristics is due to the unmeasured differences in tastes and nonmarket productivity instead of those in market earning ability.

The most influential paper in the volume is undoubtedly Heckman's. He first demonstrates that the problem of selection bias is equivalent to omitting a relevant variable from a least-squares regression. He goes on to construct a computationally simple procedure which combines linear regression and probit analysis to correct the bias. The procedure has already found wide application in econometric studies of unionism and manpower training programs, to name only two areas. Like Cogan, Heckman fails to turn up evidence of significant selectivity bias in estimating market wage equations. He does, however, find that the coefficients of labor supply functions for married women increase markedly in absolute value when esti-

mates are corrected for selectivity bias and when the endogeneity of labor force experience is recognized. In particular, he finds that the labor supply of married women increases far more with increases in their wage and decreases far more with increases in the husband's wage than has been acknowledged traditionally.

A second issue, taken up by James P. Smith in "Assets and Labor Supply," concerns the proper means of estimating the effect of wealth on labor supply. The traditional method, long recognized as inappropriate, consists in inserting assets or nonwage income into a labor supply regression. Building on the work of Gilbert Ghez and Gary Becker, Smith constructs a life-cycle model of consumption, saving, and labor supply. He emphasizes that at any given time assets reflect not only inherited or endowed wealth, but also (via saving) past consumption and labor supply. Under certain fairly restrictive assumptions, Smith is able to estimate the coefficients of husband's and wife's wage in life-cycle labor supply functions. Given the near impossibility of constructing an acceptable instrumental variable for assets (one uncorrelated with disturbances in the life-cycle consumption and labor supply functions), Smith's major point is a negative one. Reliable estimates of the wealth elasticity of labor supply require a time series of observations on the same individuals' assets and labor supply. The currently available Parnes data may satisfy this requirement.

The final issue concerns the possibility of a discontinuity in labor supply functions imparted by fixed costs of working. Certain costs are typically incurred by a worker simply by the decision to work at all on a given day, costs such as those of commuting to and from work and arranging for child care. To the extent that these costs are independent of hours worked, there is an incentive for the worker to bunch hours of work into fewer days than he or she would in the absence of these costs. No one will willingly work fewer hours in any day than is necessary to recoup fixed costs, hence the discontinuity of labor supply at a positive minimum number of hours per day. In "Hours and Weeks in the Theory of Labor Supply," Giora Hanoch takes the workweek as the period over which costs are fixed (no changes are required in order to substitute work day for workweek). He argues that the tendency to bunch working hours into weeks is opposed by the worker's desire for two kinds of leisure: leisure during workweeks and leisure during nonworkweeks. Hanoch simulates labor supply functions with a Cobb-Douglas utility function and shows that even fairly low fixed costs can generate sizable discontinuities in the supply of hours worked per year.

Both Hanoch ("A Multivariate Model of Labor Supply: Methodology and Estimation") and Cogan ("Labor Supply with Costs of Labor Market Entry") try to estimate the discontinuity in labor supply functions of married women. Hanoch estimates the range of the discontinuity in annual hours as 0-820 hours of work; Cogan estimates it as 0-1,288 hours. Both claim that Heckman's high (+4.8) estimate of the wage elasticity of labor supply of married women results from forcing a continuous labor supply curve all the way through zero hours of work.

These attempts to estimate the effects of fixed costs on labor supply are

the least-satisfying empirical efforts in the volume for a number of reasons. First, neither Hanoch nor Cogan possesses any direct measures of fixed time or money costs of work. Both therefore turn to cumbersome nonlinear statistical methods which must in turn be simplified to ease the computational burden. As a result the asymptotic properties of their estimators are unclear. Second, neither paper can demonstrate that the estimated discontinuity is really due to workers' preferences, not to employer's preferences. If firms prefer employees to work longer hours and are willing to pay them higher wages, both Hanoch and Cogan are estimating a mixture of demand and supply parameters.

Finally, the sort of discontinuity obtained is disturbingly sensitive to the measure of labor supply used. In chapter 5, Heckman shows that a discontinuity of the kind estimated by Hanoch and Cogan results when annual hours are measured by multiplying weeks worked by average hours per week. A quite different sort of discontinuity is obtained if annual earnings are divided by the reported wage. Reliable measures of hours worked together with some direct measures of fixed costs of working offer the best hope for resolving the problem.

Readers who are unfamiliar with this literature should be aware that these papers are not a compendium of facts or results on labor supply. The methodological insights in these essays, however, will repay those willing to invest the time.

Deviant Street Networks: Prostitution in New York City. By Bernard Cohen. Lexington, Mass.: D. C. Heath & Co., 1980. Pp. xvii+200. \$19.95.

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Bernard Cohen's *Deviant Street Networks: Prostitution in New York City* is an interesting and creative effort to study contemporary urban street life and prostitution. The study is based primarily on direct observation of 13 locations where deviant activities, particularly sexual solicitation, are openly pursued. Unlike other observational studies, this one was done from a distance: Cohen did not become directly involved in the lives of his subjects. Indeed, most of the research was done from his car as he drove through the neighborhoods or parked to make more extensive observations. He also observed by standing at strategic points on the street and appearing to be waiting for someone or simply loitering. Using Cohen's typology of street people, he was a "hanger-on." This is "a person who frequented locations of visible street deviance but was not a police officer, john, worker, shopper, or ordinary passerby. A hanger-on simply selected a spot at a visible deviant location and stood or sat for hours at a time" (p. 66). These observations were supplemented by limited and informal interviews with prostitutes and others found in the street scenes.

Cohen's study is also unique because he is not content to treat his observations in traditional qualitative ways. Instead he transforms them into quantitative data, a process which, he argues, increases the precision of his study because his analysis is based on both "hard" and "soft" data. Much of the analysis, then, is concerned with patterns of street life as they are reflected in Cohen's numerical tabulations. Much attention is given to the age and racial distributions of prostitutes and other street actors, their times of street activity, and the locations where they are found. Similarly, information is provided on the characteristics and activities of police officers who patrol the neighborhoods. In addition to this sociodemographic information, Cohen discusses the street scene as a stratified order and its relationship to the actions of police and other members of the neighborhood. Specifically, he discusses the implications of police crackdowns (they do not reduce the level of street activity so much as they redistribute it) and the ways in which members of the deviant street network and others in the community influence one another. It is argued that deviant street networks cannot survive in neighborhoods where residents are organized to oppose them, but that the development of such networks frequently sets in motion a process whereby persons who will tolerate them are attracted to the neighborhood and others are repelled. Thus, the networks both shape and are shaped by their environments.

Clearly, the primary strength of this book lies in the discussion of the relationship between deviant street networks and their environments. Although many studies of prostitution discuss the implications of police policies and actions on prostitutes as individuals, the literature contains little discussion of the implications of these policies for the spatial and temporal distribution of prostitution within a city or town. There is an even larger gap in the literature on the relationship between deviant street networks and the neighborhoods within which they are found. Cohen's book addresses both issues in a direct and fruitful way and implicitly demonstrates the need for further research along these lines. Another strength is the appendix, based on the work of Ronnie Shulman, which provides a thoughtful and comprehensive overview of the existing sociological literature on prostitution.

More problematic is Cohen's research style, which centers on observation from a distance and quantification of qualitative data. Although all research is perspectival and Cohen is careful to specify the observational perspective he uses, one must ask whether the vantage point of the hanger-on is the most useful one for gaining information about street life and deviance. Although observation from a car or some other distant place provides better access to the full panorama of street activities, it also limits access to the content of the interactions observed. This content is critical for analyzing the meanings of street actors' activities and relationships and for making sense of the distribution of these people in time and space. A second problem, related to this first one, involves the usefulness of transforming direct observations into quantitative data which presumably make the study more

rigorous and its conclusions more reliable. Aside from the obvious fact that Cohen's "hard data" are no better than the "soft" observations on which they are based, there is the problem of whether these numbers provide us with a better understanding of street networks than would a different, more conventionally qualitative, treatment. In particular, should age, race, space, and time be treated as conventional variables (they are treated as having an existence outside the interpretive frameworks of the persons studied) or should they be treated as socially constructed phenomena based on the everyday activities and interactions of prostitutes and others who make up the street scene?

In sum, the most important problems associated with this book are not unique to it; indeed, they involve a general dilemma in contemporary sociology. Is sociology best done from a positivistic vision of science or from a different view—such as that found in phenomenological sociology? Cohen's research and analysis must be viewed as examples of positivistic sociology, and as such they are worthy contributions to that approach.

Museums of Madness: The Social Organization of Insanity in Nineteenth-Century England. By Andrew T. Scull. New York: St. Martin's Press, 1979. Pp. 275. \$17.95.

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Within the past decade several accounts have described the historical foundations of the treatment of mental illness and other kinds of deviance. These historical analyses have been particularly useful to the deviance field in sociology because they have demonstrated convincingly that the definitions and treatment of deviance cannot be understood apart from the social and political context. Andrew Scull's *Museums of Madness* fits into this category of historical studies; he attempts to place the beginning of the modern treatment of mental illness within the context of the vast social and economic changes occurring in England in the early 19th century.

In *Museums of Madness*, Scull argues that there were dramatic changes in the treatment and definitions of insanity in England in the first part of the 19th century. He attempts to document and understand the changing conceptions of insanity, the increasing proportion of the insane in the population, and the development of separate categorizations and institutions for the mentally ill.

He outlines the basic ways in which deviance and its control in modern times differ from those in pre-19th-century times: there is greater involvement by the state, the deviants are segregated in institutions, and the kinds of deviants are carefully differentiated. The last point is amplified by Scull, and its documentation is one of the book's important contributions. The growth of institutions and several professions specializing in the treatment

of the mentally ill is a major but relatively undescribed development of the past two centuries.

Scull's main thesis is that these changes were related to the social and political changes occurring in England in the early 19th century. He rejects the idea that increasing urbanization led to the changes. Instead, Scull suggests, "the main driving force behind the rise of a segregative response to madness (and to other forms of deviance) can much more plausibly be asserted to lie in the effects of the advent of a mature capitalist market economy and the associated ever more thoroughgoing commercialization of existence" (p. 30). However, he does not present convincingly the connections between these broad political and social changes and the treatment of deviance. One does not come away from his book with a real understanding of the links between changes in the conception of the mentally ill and changes taking place in the broader society.

He does describe the changes in the treatment and categorization of the mentally ill. The leaders, philosophies, and political events surrounding their increasing segmentation are dealt with in detail. Both the moral values and the economic self-interests of the leaders in various factions of the debate over the treatment of the mentally ill are described with convincing evidence.

One of the more interesting discussions in the book concerns the development of moral treatment. According to Scull, moral treatment, which grew out of the movement to reform treatment of the insane, viewed its goal as instilling within the individual both ability and interest in exercising internal control. Previous conceptions of mental illness had viewed the insane individual as a beast or as possessed by the devil; thus external and physical restraints were seen as necessary for both change and control. With the rise of the reform movement and moral treatment, physical restraint or punishment was considered a last resort to keep patients from hurting themselves or others. This distinction between reformers and nonreformers lay not in the morality of one group and the immorality of another, but in their definitions of insanity.

Several kinds of evidence might have made Scull's argument more convincing and analytically interesting. He attributes the increasing proportion of the population committed to insane asylums to the self-interests of the "mad doctors" and the need of the community to exercise more formal social control over nonproductive members. His conclusions might have been better supported if he had described how individuals were committed to the institutions, who in society was most likely to be committed, and who was most likely to do the committing. Neither the actors nor the process of committing a patient is detailed in *Museums of Madness*. The roles of "professional" opinions, families, and troublesome political views and activities are not discussed.

Scull very carefully and convincingly makes the point that during the period studied the mentally ill were increasingly distinguished from other deviants such as criminals and paupers. However, he does not compare

changes in the treatment of the mentally ill with changes in the treatment of any other category of deviants.

This book belongs with the literature that relates the conception and treatment of deviants to economic and social changes. However, Scull refers very little to other discussions of either mental illness or other kinds of deviance. Given sociology's growing concern with the societal reaction to deviance, it would have been helpful for Scull to summarize such work and to place his own book within this framework. One intriguing question is: With the increase in the institutionalization of the mentally ill, what happened to the institutionalization of paupers or criminals?

Thus, Scull's account is limited by a failure to link adequately the changing conception and treatment of mental illness to the economic and social context—despite a thesis that argued for their intimate connection. Moreover, he does not consider the growth of institutions for the insane in the context of what was happening to institutions for other deviants. His theoretical framework within the field of deviance is not as well defined or well developed as the state of the field deserves.

All this aside, Scull's careful consideration of the changing views of madness, the reform movement, the history of moral treatment and its evaluation, and the growth of psychiatry make this book worthwhile for those interested in the relationship of the societal conception and treatment of deviance to broader cultural and social foundations.

Decarceration: Community Treatment and the Deviant—a Radical View.
By Andrew T. Scull. Englewood Cliffs, N.J.: Prentice Hall, Inc., 1977.
Pp. viii+184. \$3.95 (paper).

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Andrew Scull's *Decarceration: Community Treatment and the Deviant—a Radical View* is one of the few works which both symbolize and promote a shift in the theoretical approach to social and sociological problems. Like Howard Becker's *Outsiders* and Thomas Scheff's *Being Mentally Ill* in an earlier era, *Decarceration* radically shifts the emphasis of deviance theory.

Decarceration represents both an extension of and a theoretical advance on the labeling theory of deviance. As an extension, the book draws attention to the legislative and political processes of creating labels; as a theoretical advance, it shifts attention from social-psychological processes to the macro-level structures which determine the defining and handling of deviance. In contrast to the historical and micro-level foci of labeling theory (which at the time represented an important theoretical advance but which at this time have become, as Scull notes, a "cult"), the political-economy perspective focuses on historical and structural—economic, political, and legal—determinants of social control and social policy.

The purpose of the book is to explain, from the theoretical perspective of the political economy, the policy of decarceration (or deinstitutionalization), a term which Scull says is "shorthand for a state-sponsored policy of closing down asylums, prisons, and reformatories" (p. 3). The substantive chapters on mental patients and on criminals and delinquents focus on the history of and conventional explanations for the decarceration of these populations. The remainder of the chapters are theoretical explorations of the dimensions, causes, and consequences of this shift in social policy.

In Scull's (and Spitzer's) terminology, under capitalism "social junk" takes the form of people unwilling or unable to be part of capitalism's pool of surplus labor. This useless underclass has, from the 19th century to the 1950s, been confined in increasing numbers to "asylums" for the mad, the bad, the poor, the elderly, and the otherwise worthless.

Scull explains the decarceration of asylum inmates as a consequence of the increasing economic problems of advanced capitalism, particularly the fiscal crisis of the states. He makes quite clear the inutility of currently popular noneconomic causal theories of decarceration. For example, the "technological determinism" which alleges that the development of psychoactive drugs "caused" the emptying of the state mental hospitals is proved faulty, since the beginning of the population decline preceded their invention.

One of the consequences of the policy of decarceration, as Scull points out, is that it has been replaced by a myth: the myth of community care—the return of the deviant to the fold. The reality behind the myth is one of a shift from state-run to private entrepreneurial—federal welfare forms of incarceration, such as the board-and-care and nursing home. These institutions have the dubious advantages of being cheaper to run per patient than the state institutions and of making a profit for entrepreneurial individuals or corporations.

A few elements in Scull's argument could have been more thoroughly supported empirically; his overall thesis would have been strengthened by including the handling of the elderly as social junk and by considering (if he could have uncovered them) relevant statistics on private as well as public mental hospitals and the psychiatric hospitalization of juveniles. I can think of little that could weaken his thesis—despite the criticism (unwarranted, in my opinion) that the book is "vulgar Marxism."

As a once-dedicated—and still partially committed—micro sociologist and labeling theorist, I feel a little strange writing this review. But *Decarceration* came to hand at a time when, writing about a mental health court, I had begun to realize and wrestle with the inadequacy of the micro perspective. I had moved "outward" from the organizational to the interorganizational determinants of decision making and discovered that I had to come to terms with yet another level of determinants: those at the historical and the structural levels of analysis. Scull's book is, to my mind, the most lucid, convincing, and empirically demonstrated argument to date for a political-economy perspective on deviance. It should be read by every deviance theorist worthy of the name.

Deviant Behavior in Defense of Self. By Howard B. Kaplan. New York: Academic Press, 1980. Pp. xi+255. \$32.50.

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At first glance, the theme of *Deviant Behavior in Defense of Self* appears seductively simple: In search of an increase in self-esteem, those who believe themselves to be repudiated by society turn to deviant forms of behavior. This thesis rests, according to Howard B. Kaplan, "on the premise that, universally, the self-esteem motive is a characteristic feature of human personality development. The *self-esteem motive* is defined as the personal need to maximize the experience of positive self-attitudes and to minimize the experience of negative self-attitudes" (p. 8, italics in the original).

As subjects for a three-year study, Kaplan identified 9,335 seventh graders in Houston, Texas. Asked to respond to questionnaires, 82% provided usable information for the first year, 50% responded for the first two years, and 34% participated for the three years of data collection (1971, 1972, 1973). The high rate of attrition and its relation to variables measured during the first year raise problems not adequately discussed by Kaplan. He reports that deviance, but not low self-esteem, predicted non-response for the second and third years. Yet if low self-esteem leads to deviance, as Kaplan suggests, and deviance leads to nonresponse, self-esteem should be related to subsequent attrition.

Kaplan used seven of the 10 items from the Rosenberg scale of self-esteem to measure "self-derogation." The two items having highest loading on the self-derogation factor were: "At times I think I'm no good at all," and "I certainly feel useless at times."

Kaplan considered 28 measures of deviance, repeatedly referring to them as "essentially uncorrelated among themselves [*sic*]." Subjects were asked whether "they performed the deviant behavior in question during a specified period prior to the test" (p. 28). Six items inquired about thefts (some asking about the value of what had been stolen and some about the place from which it was taken); three asked about drug use (alcohol, narcotics, marijuana); six asked about aggressive behavior (including participation in gang fights and starting a fistfight); five items touched on school difficulties (e.g., skipping school and receiving a failing grade). Both common sense and prior research lead one to expect correlations within some of these subsets. Absence of expected correlations raises doubt about the veracity of the respondents. This doubt is increased by the analysis of agreement between students' reports and reports of behavior known to school personnel: "For 10 of the 24 items, at least 50% of the 'known' instances were also self-reported" (p. 33). The doubt is reinforced when we learn that of those who reported having had police contacts, and of those who reported being suspended or expelled from school, and of those who reported being sent to a therapist, "at least 60%" (p. 36) denied the event the following year.

Definitions of intervening variables are scattered through the text and are

difficult to find when subsequently referenced. These include "self-protective responses," "self-enhancing potential of the normative environment," "awareness of deviant response patterns," "devaluation of norms," "stability of self-attitudes," and "subjective distress."

Some of the scales used to measure key concepts switch reference terms between "I" and "you." For example, the measure of self-protective strategies included both "If you want people to like you, you have to tell them what they want to hear, even if it isn't the truth" and "When I do something wrong, I usually admit it and take my punishment" (p. 71). The measure of defenselessness/vulnerability summed affirmative responses to 12 items, including: "Do you often get angry, annoyed, or upset?" and "I get nervous when things aren't just right" (p. 103). Although the choice between styles may be irrelevant, their mixture is confusing; respondents cannot be expected to know that they are the referents for both.

Although Kaplan concluded that his study supported his theory, the evidence presented in the book fails to show convincingly a universal desire to maximize self-esteem. Nor did Kaplan show that either such a universal desire for self-esteem or such a maximizing strategy is a necessary basis for the theory that deviance is a successful adaptation to low self-esteem.

Had Kaplan made the more modest assumption that under some (perhaps identifiable) conditions some types of deviant behavior are motivated by a desire to increase self-esteem, his research would have had clearer direction. Unfortunately, both methodological sloppiness and obscure writing detract from his interesting project.

Despite these many defects, Kaplan put to good use the sequencing possibilities of his longitudinal data. Thus he shows that among students who had not previously reported deviant behavior self-derogation predicts increases in reported deviance. Poor defenses of self-esteem predict increases in self-derogation. And among males, the effect of deviance on self-attitudes depended on strength of defenses: deviance increased self-derogation if defenses were strong but decreased it if defenses were weak.

More should be asked of both author and editor. *Deviant Behavior in Defense of Self* could have been an important book; perhaps another work culled from these data will fulfill that possibility.

The Penalty of Death. By Thorsten Sellin. Beverly Hills, Calif.: Sage Publications, Inc., 1980. Pp. 190. \$18.00 (cloth); \$8.95 (paper).

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Thorsten Sellin's new book on the death penalty is his third on the subject, having been preceded by his important monograph for the American Law Institute (*The Death Penalty*, 1959) and his widely used anthology (*Capital Punishment* [Harper & Row, 1967]). *The Penalty of Death* lacks the

novelty of his first work and the plural authorship of the second, but it contains material not to be found in either: a worldwide survey of capital punishment from the earliest times (chap. 1) and an evaluation of executions as retributive punishment (chap. 2). The book also provides an updated review of the central empirical issues discussed in his two previous volumes: unfairness in the use of capital sentencing (chap. 3), the likelihood that a murderer will be executed (chap. 4), general deterrence (chaps. 5 and 6), recidivism among unexecuted capital offenders (chap. 7), and the effect on criminal homicide rates of the abolition and restoration of the death penalty (chaps. 8–10). Many familiar and important issues are not discussed, however, or are mentioned only in passing. These include errors of justice and the execution of the innocent, the problem of the constitutionality of the death penalty, the economic costs of a criminal justice system that includes the death penalty, the demographic and other characteristics of persons under death sentence, the decline in the practice of executive clemency, and the political sociology of the abolition movement. Thus, the book is not a comprehensive treatment of the death penalty in America. Nonetheless, Sellin has provided the reader with much information based on a wide variety of (often obscure and virtually unobtainable) state government documents, as well as a good guide to much of the most recent research by others, some of it still unpublished.

No new empirical research by Sellin himself is reported. He is best known for his earlier research on the deterrent aspects of the death penalty, and on that subject his views in this new volume still emerge with forceful clarity. First, he remains convinced that, on the evidence, the death penalty as a deterrent is not marginally more effective than long-term imprisonment. Sellin's preferred argument here remains the same as the one he used in his testimony 30 years ago before the Royal Commission on Capital Punishment: the comparison of crude homicide rates in adjacent states, some of which have abolished and the others retained the death penalty (see pp. 170–72). Second, he is unpersuaded by the methods and results of the well-known research performed during the mid-1970s by Isaac Ehrlich, to the effect that each execution in the nation during 1933–67 prevented between one and eight criminal homicides. Although Sellin mounts no extensive counteranalysis of his own, he does cite and quote from the writings of others who have examined closely Ehrlich's conclusions and found them wanting (pp. 175–79). Third, despite skepticism about both the quality of current deterrence research and its relevance to policy decisions, a skepticism expressed in 1978 by the Panel on Research on Deterrence and Incapacitative Effects of the Committee on Research on Law Enforcement and Criminal Justice of the National Research Council's Assembly of Behavioral and Social Sciences, Sellin firmly believes that research with the data at our disposal is not irrelevant. He believes that it can "furnish policy makers with facts that point to the general nature of the issue and the probable answers to the questions it raises" (p. 87).

The book is marred by many typographical errors; let us hope that it inspires no imitators as an example of the bookmaker's art. Also, the lack

of a bibliography will impede scholarly use. Even so, *The Penalty of Death* is a fine testament to its author's enduring concern to make the results of sociological inquiry both accessible to the general public and relevant to informed decisions on public policy. It is, moreover, a fine testimony to the unflagging energies of an octogenarian whose scholarly contributions now stretch over half a century.

Jury Trials. By John Baldwin and Michael McConville. Oxford: Clarendon Press, 1979. Pp. 150. \$14.95.

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The body of *Jury Trials* is an opinion poll on the jury. A sample of 370 jury verdicts rendered in 1975 and 1976 in the Crown Court of Birmingham is commented upon by the "other key participants in the trial"—the judge, the police witness, and solicitors for prosecution and defense (permission could not be secured to interview barristers). Interviews with police officers who participated in approximately 350 jury trials in the Crown Court Centers in London in 1975 are used to supplement the Birmingham data.

John Baldwin and Michael McConville use these data primarily to address the debate on the competence of the jury. Thus they focus on two problems: "questionable acquittals," and "doubtful convictions," with secondary attention to the question whether "professional criminals" manipulate the jury to their advantage. Regarding Birmingham, they find that 36% of jury acquittals and 5% of convictions are questionable. (An acquittal is regarded as questionable if the judge and one other participant spontaneously raise doubts about it; a conviction is deemed questionable if any two participants spontaneously raise doubts.) For London, a smaller percentage of jury verdicts were deemed questionable, in spite of the use of a lower threshold of doubt. In both jurisdictions, cases involving professional criminals do not appear to be more likely to lead to questionable verdicts.

Although the authors emphasize the seemingly high rate of questionable acquittals, recalculation of the data indicates agreement between judge and jury in over 80% of the cases. Thus, although the measures are not precisely identical, it seems that the basic findings here are comparable with those of Harry Kalven, Jr., and Hans Zeisel in *The American Jury* (Chicago: University of Chicago Press, 1966). The major difference here is that there are no obvious, and in particular no "legitimate," explanations for the judge-jury (or other participant-jury) divergence. Principles of jury equity and the like do not, in the authors' judgment, explain the acquittals: "The truth is that we could not detect any common characteristics amongst these cases and, indeed, found many more cases amongst the *convictions* in

which some measure of jury equity would have been more easily justified" (p. 62; *italics added*). With respect to the doubtful convictions, the authors conclude that juries do not apply the high standard of proof required in criminal trials and fail to comprehend the evidentiary issues involved in specific cases. In a separate analysis, the authors find that the rate of questionable outcomes is not related to jury composition in terms of gender, age, or social class.

While *Jury Trials* makes a significant contribution to our fund of knowledge on the English jury in criminal cases, its authors acknowledge it to be limited by its design, which is patterned after that of *The American Jury*. Aware of the criticism of Kalven and Zeisel's methodology, Baldwin and McConville argue cogently that alternative means of studying juries either are not demonstrably superior or are unavailable. (Jury research in England is even more restricted than in the United States; e.g., jurors cannot be questioned even after a trial is completed.) Baldwin and McConville do succeed in overcoming one of the objections to the Kalven and Zeisel study by soliciting information from more than one participant, but in other respects they have many of the same types of problems. For example, three-fourths of the questionnaires used were completed by 12 judges, only one-fourth of the judges in the sample. Finally, the striking variation between Birmingham and London in the types of cases tried, in the rate of acquittal, and in the rate of questionable verdicts suggests that Birmingham may not be a representative jurisdiction and that a structural analysis of variations among jurisdictions is necessary.

Many readers will find the data analysis in the book unduly simplified. All of the tables are uni- or bivariate, measures of association are not used, and statistical significance is mentioned only occasionally. Many of the data are buried in the text, and many of the most important arguments are made with no systematic presentation of data. On the other hand, the refreshing use of examples from specific cases enlivens the presentation.

In many ways the most important contribution of Baldwin and McConville's work is a point they mention in the conclusion and develop more fully in a subsequent article, "Trial by Jury: Some Empirical Evidence on Contested Cases in England" (*Law and Society Review*, vol. 13 [1979]). Research and policy regarding the jury must ultimately consider the jury not as an isolated institution but as one in a set of institutions. Jury competence cannot be evaluated in the abstract; the jury is imperfect, but so are other decision makers. The most logical alternative to the jury is the judge, but judges have hardly been free from error. For example, in the present study, in all the cases of doubtful conviction the trial judge apparently let the case go to the jury and let the verdict stand, despite the judicial power either to order an acquittal or to overturn a jury's verdict if it is manifestly unjust. Thus, as the authors state, "It may be the case that the [jury] reaches a right and just determination as often as can reasonably be expected of any tribunal and that we must live with a proportion of failures" (p. 134). On the other hand, it may be that the proportion of failures can be reduced; the point is that present conceptualization of the

issue does not allow a meaningful determination. Until we first define the question of jury competence in a comparative framework and then determine the circumstances under which one or another institution renders a "better" decision (a definitional problem in itself, as Baldwin and McConville recognize), we shall fail to understand the true role of the jury in the Anglo-American system of judgment.

Surveys by Telephone: A National Comparison with Personal Interviews. By Robert M. Groves and Robert L. Kahn. New York: Academic Press, 1979. Pp. xii+358. \$21.50.

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Over the past few years, commercial research firms have been shifting their emphasis from a reliance on personal, in-the-home interviewing to interviews conducted over the telephone from central locations, using WATS telephone systems. The profitability of these firms depends on gathering data adequate to their clients' needs at the lowest possible cost, and the commercial sector has always been pressured by the discipline of competitive bidding to conduct interviewing as cost-effectively as possible. But information concerning the details of commercial survey research techniques generally is proprietary—these firms are not inclined to disclose financial information, nor do they often expose key specifics of their methods to public scrutiny. And although they are concerned with data quality, they rarely evaluate it with academic rigor.

Therefore, *Surveys by Telephone: A National Comparison with Personal Interviews* performs an especially valuable service for the survey research community. In particular, it offers some perspectives and insights to academic and not-for-profit researchers whose clients, for example, the federal government, have traditionally been willing to pay higher costs of in-the-home interviewing to ensure the quality of the data. With the meticulous attention to detail and the high degree of rigor that we have come to associate with the University of Michigan's Survey Research Center (SRC), Robert Groves and Robert Kahn present a comprehensive comparison between telephone and in-the-home interviewing, both in terms of costs and, more important, in terms of the quality of the data obtained.

The book compares three surveys conducted simultaneously in 1976, surveys which used approximately identical questionnaires. (The differences were primarily adjustments to permit show-card questions such as "income" and seven-point "satisfaction/dissatisfaction" scales to be administered over the telephone.) The surveys involved were: (1) A personal interview survey conducted in the Survey Research Center's national household sample of 74 counties and metropolitan areas (areal probability sample, $N = 1,548$). (2) A telephone survey in the same sample areas, with telephone numbers selected by random-digit dialing in each of the 74

counties and metropolitan areas (clustered sample, local interviewers, $N = 865$). (3) A telephone survey in which the numbers were selected by random-digit dialing throughout the entire United States rather than being clustered within sample counties (single-stage sample, central location interviewing, $N = 869$).

Surveys by Telephone discusses exhaustively the advantages and limitations of these three approaches. The differences the authors highlight include (1) central location interviewing affords the opportunity for automatic data entry and computer-assisted telephone interviewing (CATI); (2) telephone interviewing provides access to security buildings and dangerous neighborhoods; (3) random-digit dialing provides access to the estimated 20% of residential telephones in the United States that are unlisted; (4) central-location telephone interviewing facilitates interviewer training, supervision, and monitoring; (5) surveys for which visual stimuli are necessary are generally doomed to the higher costs of in-the-home interviewing.

One presumed drawback of telephone interviewing—limited coverage of the population—may not always be as severe as many suspect. Random-digit dialing will reach approximately 90.4% of all U.S. households, while the SRC areal probability sample is estimated to provide access (at least in theory) to about 95% of all dwellings in the coterminous United States. Overall this appears to be a negligible difference, but the authors point out that the 10% left out of the telephone sample is well defined in terms of income and race, leading to the threat of bias for certain topics. Of course, bias may also reside in the practical inability of in-the-home interviewing to cover adequately security buildings and dangerous neighborhoods.

For the in-the-home survey, 74% of all sample households yielded completed interviews. Depending on how one accounts for numbers unanswered after at least 17 attempts, the telephone approach yielded either 70% completed interviews (if such numbers are assumed to be nonworking) or 59% (if they are assumed to be working). The authors feel that the former assumption is safe enough and that the response rate of the telephone surveys was not far behind that of the in-the-home survey. Other surveys, of course, may experience different response rates than those reported here.

The authors' cost comparisons confirm the obvious: telephone interviewing is far less costly. For these surveys, data collection by telephone (including all sampling, field administration and supervision, training, travel, telephone, printing and mailing) was about half as expensive as in-the-home interviewing. The detailed cost breakouts presented are very helpful in understanding the differences between the two approaches.

The comparisons of distributions of responses to the individual items in the questionnaire are for the most part reassuring. Results from the two approaches tended to be comparable, within sampling error, over a wide range of topics and item formats. The authors present a thoughtful discussion of the types of items on which the two approaches differ.

Surveys by Telephone will be of interest to several audiences. Professional survey administrators may find it helpful in making survey design

decisions. Survey research clients may find useful guidance in understanding the costs, benefits, limitations, and tradeoffs involved in choosing between the two approaches to data collection. Students of survey methodology may find this book an outstandingly clear, comprehensive, and rigorous discussion not only of the foregoing topics but also of the nuts and bolts of conducting field research. Perhaps the most timely contribution of the book will be to survey researchers (or clients) facing cutbacks in research support. The book offers considerable reassurance that telephone interviewing, when used in appropriate situations, offers a highly cost-effective means of gathering data of entirely acceptable quality.

Total Survey Error. By Ronald Andersen, Judith Kasper, Martin Frankel, and associates. San Francisco: Jossey-Bass, Inc., 1979. Pp. xxxvii+296. \$15.95.

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Total Survey Error, by Ronald Andersen, Judith Kasper, Martin Frankel, and associates, is an analysis of nonsampling error in estimates of health-care utilization and expenditures derived from survey data. The book is organized on the general model of survey error devised by Leslie Kish (*Survey Sampling* [New York: Wiley, 1965], pp. 509-29). Three sources of nonsampling bias are considered: field bias (respondent reporting error), processing bias (imputation error), and nonresponse bias.

This book reflects the considerable experience of its authors with health expenditure and utilization surveys, although the data presented are derived primarily from the most recent in a series of four surveys carried out by the Center for Health Administrative Studies and the National Opinion Research Center at the University of Chicago (CHAS-NORC). Therefore, although the book is about equally divided between text and tables, the reader would be well advised to obtain a copy of the survey report by Ronald Andersen, Joanna Lion, and Odin W. Anderson (*Two Decades of Health Service: Social Survey Trends in Use and Expenditure* [Cambridge, Mass.: Ballinger, 1976]) for reference while reading *Total Survey Error*.

The book is organized into four sections. In the first (chap. 1), the authors present their main thesis, the discussion of the Kish model and operational definitions of the sources of bias. Section 2 (chaps. 2-6) evaluates the effects of the three error sources on the estimates of health-services utilization, expenditures, health-insurance coverage, and diagnoses for which a doctor was consulted. In addition, chapter 6 discusses the effects of "problem respondents," defined as those who refused permission for verification or were uncooperative in other areas of the survey, such as income reporting. In part 3, chapters 7-10, several alternative approaches to assessing bias are described. Part 4, chapter 11, contains a general over-

view of the findings and implications of the book. Three areas are discussed: survey methods, policy, and health-services research.

This is a very informative and honest book. It is in the tradition of such studies as *The American Soldier* (Samuel A. Stouffer et al. [Princeton, N.J.: Princeton University Press, 1950], vols. 1-4) and *The Authoritarian Personality* (T. W. Adorno et al. [New York: Harper, 1950]) in its effort to analyze methods and consider ways in which methodological problems might be better addressed. Andersen and his staff have here described and examined their own logic in making decisions to adjust for various types of error. They have then gone beyond this description and prepared a critique of their decisions, including alternative and in some instances more effective approaches.

A trivial criticism might be made of the title. Although the concept of "total survey error" is introduced and Kish's model is presented, the book is in fact about three kinds of survey biases: nonresponse, erroneous reporting, and erroneous imputation. Moreover, their definition of the scope of these three sources of error is further limited by the methods used.

However, this is a relatively minor concern because in fact the major sources of bias in the estimates with which they are concerned derive from the factors which Andersen and his colleagues do discuss. Thus, the book begins with a description of how the adjustments were in fact made in the analysis of the data. In each case the strengths and weaknesses of their approach are considered, and of course the relevant data are presented.

The third and fourth sections of the book provide the real meat of the discussion. For example, in chapter 7 the authors discuss three methods by which imputation can be made about the characteristics of nonrespondents. They admit that their choice to use data from external sources required "heroic assumptions" about the comparability of the sources of these data with their own sample. In fact, in reading the discussion of this choice in chapter 2, one might have concluded hastily that the external sources used for imputation may contribute more bias than they correct, owing to the lack of generalizability of these data. This point is addressed, however, in chapter 7, where the authors review the relative merits of each approach and in the end advocate using internal data sources for such imputation because of the difficulty of finding credible external sources. In the process, though, the further point is made that this approach may also require the questionable assumption that the nonrespondents do not differ from the respondents.

Although not everyone will agree with the conclusions this book draws, it is an important methodological analysis of the problems in conducting this type of survey. It is a highly technical discussion but worth the effort because it provides both theoretical and practical advice about how to deal with some problems of survey error. It makes for interesting reading from both substantive and methodological viewpoints; and it is quite well written. Before undertaking this book, however, the reader will be well advised to review the original survey report (Andersen, Lion, and Anderson 1976) cited above.

Cultural Materialism: The Struggle for a Science of Culture. By Marvin Harris. New York: Vintage Books, 1980. Pp. xii+370. \$4.95 (paper).

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Those who have read Marvin Harris's other works will find little new in *Cultural Materialism: The Struggle for a Science of Culture*, which argues that most of his anthropological and sociological colleagues are confused, incompetent, and bedimmed because they pay insufficient attention to his brand of cultural materialism. The list of those attracting his scorn is impressive: Émile Durkheim, Talcott Parsons, Alfred Schutz, Clifford Geertz, Alvin Gouldner, Noam Chomsky, David Schneider, Ward Goodenough, Robert Lowie, Franz Boaz, Marshall Sahlins, V. Lenin, Thomas Kuhn, Edmund Leach, and Mary Douglas, to note only a few. Even his idols Marx and Engels are castigated.

For Harris, cultural materialism "leads to better scientific theories" (p. ix). From his account it appears that Harris does not comprehend the social and philosophical bases of either science or social studies, which he believes should emulate what he thinks are the methods of the "hard" sciences. Therefore it is not surprising that philosophical and historical writings receive little serious consideration here.

Imputing moral callousness or political irresponsibility to many with whom he disagrees, Harris proclaims his own liberalism. Yet the derivation of his own values is unclear, especially since he seems to view "true" science as "value free." He nowhere explicitly recognizes that social studies cannot and should not be devoid of moral judgments; nor can these judgments be entirely separated from methodology and theory, and the connection must be carefully examined.

Harris's belabored distinctions between the emic (ideas and values) and the etic (observed behavior) appear naive, for he fails to provide a proper epistemological apparatus by which one's own emic thought is related to interpretation of the etic of others. He never mentions Weber's concern to distinguish between *verstehen* and *wissen*. Weber's account, with his dismay over the possibility of forging a study of society out of such complex interrelations, is a more useful starting point for appreciating the magnitude of the problem. The epistemological and ontological issues raised by the relation between ideas, values, and behavior pose the real problem of social explanation. Writers as diverse as Max Weber, R. G. Collingwood, M. Merleau-Ponty, Ludwig Wittgenstein, Peter Winch, Malcolm Crick, Roy Bhaskar, and E. E. Evans-Pritchard (none of them considered by Harris) have agonized over this problem.

Harris argues that most researchers fail to appreciate that the infrastructure (determined by material culture, modes of production, population, and resources) lies at the heart of social understanding. It is doubtful that many would reject this broad assertion, but would they follow Harris in subsequently selling short ideational, cultural-historical factors? Harris

seems to have resurrected Robert Merton's latent functionalism in evolutionary, positivistic form. While Harris gives lip service to anthropological studies of the beliefs and values of people, these studies are credited with little value; real explanation resides in his own views. Such intellectual hegemony is associated with the natural sciences but not with the value-laden nature of social thought, a field where comparison is far trickier. In Harris's case, I sense an intellectual and moral condescension toward non-Western perspectives, even though he presents himself as a defender of the downtrodden.

Harris maintains: "To deny the validity of etic description is in effect to deny the possibility of a social science capable of explaining sociocultural similarities and differences" (p. 45). From this, he goes on to assert that because people in a society may disagree about what is proper behavior, because the powerful may enforce conformity to their ideas and values, and because different persons within groups perceive their situations and interests differently, we should discard the idea of a society as a group with shared beliefs and values. This view has merit, but it hardly follows that we should pursue the etic and abandon investigating beliefs and thoughts held by various groups and individuals: "I doubt that any model for predicting behavior based on the assumption that behavior is determined by shared cognitive orientations can advance our understanding of how families manage to function as social groups" (p. 277). Etic description seems useless unless one first understands what the actors themselves think, yet nowhere does Harris touch on the immensely difficult and methodologically central question of translating cultural values and beliefs.

The second half of *Cultural Materialism* characterizes the supposed competitors of Harris's position, dismissing them one by one as though this would inevitably lead to his position as the only alternative. He avoids reporting or analyzing any social institution with the thoroughness or rigor required of scholarly work, repeatedly treating data uncritically or omitting crucial material unfavorable to his argument. For example, he stresses his and Michael Harner's interpretation of Aztec cannibalism, even though it appears unbalanced and distorted. He does not mention that the Spanish edict of 1503 (allowing enslavement of cannibals) encouraged reports that Amerindians ate human flesh; that recent studies describe other Meso-Americans as reasonably healthy despite a diet low in animal protein; or that human sacrifice primarily coincided with harvests when foodstuffs were plentiful. B. R. Ortiz de Montellano's damaging paper is cited but not directly confronted.

Not one important modern ethnographic monograph is anywhere cited; instead Harris refers to abstract theory, scattered ethnographic papers, and critical reviews. There is no consideration of important works on complex modern societies. Yet an exegesis of methodologies for data collection and analysis should address itself to such studies.

Harris often attacks a supposed school by selecting a peripheral or poor paper by an important thinker instead of coming to terms with the works that made that writer significant. Or, for example, to attack the phenome-

nological approach advocated by Schutz and taken up by Burger, Luckmann, and others, he presents Carlos Castaneda as representative, whereas Castaneda has about as much relevance to Schutz's method and scholarship as do the works of Tolkien. Harris rightly attacks inept and uncritical employment of the Human Resources Area File and other ethnographic atlases, only to cite approvingly work by himself and a follower which relies uncritically on comparable materials.

Technology, resources, and modes of production do exert important influences on society. But they do not in themselves explain many of the societal and cultural differences that currently absorb researchers attempting to understand everything from social organization and religion to bureaucracy, organization of complex institutions, the development of law, or the pervasive strength of rules about etiquette and alimentation. Harris's work relies on a muddled, shallow, antisociological view of understanding that cannot provide a useful method even for the valid points he makes.

Finally, the ungenerous, hectoring tone of this book suggests a mind disinclined to learn from or give credit to others. Its uncharitable manner may put off many readers, since its spirit seems antipathetic to collegial, scholarly endeavor.

The Structural Revolution. By Jean-Marie Benoist. New York: St. Martin's Press, 1978. Pp. viii+247. \$17.95.

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Jean-Marie Benoist's book is a rewarding study of modern French structuralism and related intellectual movements—movements that the English reader frequently finds discussed in a very cursory, controversial, and often contradictory literature. This can hardly be surprising if we realize that modern structuralism has emerged out of a typically French intellectual debate in reaction to the empiricist orientation in its historical, phenomenological, and positivistic varieties. It is no surprise, then, that the sparse reactions to structuralism which have appeared in the English-language sociological literature are of three inadequate kinds: positive but only marginally illuminating, critical for the wrong reasons, or downright misleading in propounding misinterpretations of the movement.

The Structural Revolution proves to be a lucid, though at times complex and highly metatheoretical, presentation of key issues raised by the structural movement. Benoist interrelates ably various ideas which constitute the intellectual background of structuralism and shows various differences as well as linkages among exponents of different forms of structuralism. More important, he knows how to capitalize on various ideas that emerged as reactions to or developments of structuralism, so that he points out where the enduring validity of structuralism can be found. This volume is

a distinct contribution to the history of ideas and, though not written by a professional sociologist, it is important reading for sociologists; the latter can no longer defer reckoning with a movement which has already permeated many of the humanities and social sciences. Such a task is even more imperative if we consider Lévi-Strauss's claim that his approach encompasses what remains valid in Durkheimian sociology.

In the opening chapter Benoist presents structuralism not as a doctrine but as a new methodological frontier, or rather frontiers, given the existence of various forms of structuralism. For Benoist these are the main sources of structuralism: the Saussurian notion of language as a set of differential contrasts; the independent discovery of the notion of "structure" by the Bourbakist mathematicians; and the philosophical insights of Leibniz, Gaston Bachelard, Martial Gueroult, Jules Vuillemin, and Victor Goldschmidt. These thinkers have convergently stressed the need to understand a phenomenon or a "text" as a closed and self-referring system to be explained not on the basis of the concrete and empirical determinations of the elements but on the basis of a formal set of elements and relationships among elements. Benoist discusses also the contribution of literary critics who have argued that a text should be analyzed as an autonomous entity in and by itself (e.g., Proust) and those who have argued that its meaning derives from its relationship with other texts (as in Kristeva's notion of "intertextuality"); in line with these principles, Roland Barthes has shown that a given text carries a multiplicity of meanings in virtue of its own structure. Since the text is characterized by such an "openness" and "freedom" and has a purely relational meaning, the subjectivity of the author is no longer important to determine meaning. Moreover, the subject is no longer understood as a conscious subjectivity (as in the works of Descartes and Sartre) or as a transcendental entity but as a set of syntactical structures (Lacan). Concurrently, Michel Foucault has shown that the transition from one epistemology to another is best explained not by reference to man but in terms of transformations of rule systems. With the abolition of the transcendental and conscious subjectivity comes the end of the unilineal and monodeterministic conception of history (e.g., that proposed by "economist" or "vulgar" materialists). Then too comes the end of the supremacy of diachronic explanation over synchronic explanation. Benoist considers Chomsky's works as a kind of integration and extension of Jakobson's work and finds in Lévi-Strauss an anticipation or a parallel development of Chomsky's transformational approach. Surely, Jacques Derrida may have a point when he criticizes both phenomenology and structuralism for being prisoners of Western metaphysics—they, in fact, assign a privileged place to Logos (and speech and phonological patterns). However, Derrida's stress on "deferment" and "writing" are seen by Benoist not as a destruction of structuralism but as an opening up of new structural frontiers. It is in Chomsky's and Lévi-Strauss's works that Benoist finds the development of new structural objectives; in the new "open mathematics" he finds the tools for bringing the promises of new structural frontiers to full fruition.

Chapter 2, a difficult chapter to read, is readily summarized. Althusser's contribution consists in rejecting mechanistic and humanist Marxism and in asserting the specific and autonomous influence of the superstructure on the infrastructure, the overdetermination of contradictions, and the determinacy in the last instance of the economic factor. Benoist's critique is a devastating one: Althusser is a positivist who opts for the primacy of formal simplicity and rational logic, both of which are antithetical to dialectical thought. With Althusser, dialectic is pluralized, negativity is eliminated, and syntactic contradiction is replaced by semantic ambiguity (overdetermination); moreover, dialectic is understood as the logic of the heterogeneous, and therefore it becomes an idealistic dialectic.

In chapter 3 the question of the arbitrary (Saussure) or nonarbitrary (Pierce) relationship of signs to things they represent is discussed together with the larger question of the relation between nature and culture. Both Lévi-Strauss and Chomsky deal with this question at its very fundamental level, that of language and code. Lévi-Strauss avoids the pitfall of individual subjectivism by assimilating culture to language and by focusing on the formal structure of language, with the related notions of paradigmatic network and universal combinatory matrix. Yet, by seeing culture as inherent in nature, Lévi-Strauss seems to remain a prisoner of Western metaphysics and its substantialism (Derrida's objection). Chomsky, on the other hand, vindicates the specificity of language, which he explains as a system of autonomous transformations generated from the innate structure of mind. Here Chomsky joins Kant in an antiempiricist posture which accounts for experience in terms of *a priori* formal conditions. However, it is not quite clear how, with the notion of structures innate to individuals, one can avoid metaphysical essentialism, psychological reductionism, and Cartesianism. According to Benoist the best antidote to these pitfalls can be found in modern biology with its formulations in terms of linguistic codes and in the Lacanian conception of the unconscious as language and syntax.

Chapter 4 is an excursus through modern biology and its explanations of living organisms in terms of information theory, "invariables," and scriptural language (Monod). However, Benoist argues that in modern biology one can still find the traps of traditional epistemology and the vestiges of traditional humanism, existentialism, and anthropomorphic pragmatism—and so a falling away from Bachelard's and Kant's achievements.

In chapter 5, the longest and pivotal chapter, Benoist presents his own synthesis of semiotic structuralism and elements of Lacanian Freudianism. He starts by stating that at the end of the 19th century we witnessed an axiomatic breakthrough—the advent of formal logic and the superrationalism of Bachelard. Seen from these perspectives, dialectics becomes inadequate because it is based on contradictions and the notion of contradiction is not appropriate to explain development. Moreover, the human mind does not work through an endless splitting up of things but rather by setting up systems of relations whose form remains constant. Consequently, what matters is a general formalization based on transformations, variations, and translations. Benoist discusses Serres's development of Bachelard's

formal approach and accepts Piaget's notion of structure as a system of self-regulating transformations. At this point he raises the question whether the Freudian unconscious disrupts the Leibnizian network and the syntactic order. Benoist rebuts Deleuze and Guatarri's critique of Lacan and Lévi-Strauss for its substantialist, causalist, Cartesian, and positivistic biases. Lacan shows the productivity of the semantic process in the sense that the latter corrupts the syntactic order: such a productivity, however, remains within the structure, since the structure controls the entry of the subject into the symbolic. Benoist argues that the notion of the corruption of the syntactic order by the unconscious permits one to avoid the danger of a formalistic structuralism and to opt for a dynamic structuralism and an open Marxism as well. It is true that Godelier and Althusser have liberated Marxism from a linear and causalistic view; but Benoist reiterates that with Althusser one still faces the danger of axiomatizing contradiction and transforming it into an idealistic combinatorics. According to Benoist the Freudian notion of "overdetermination" permits us to avoid such a danger, and at this point Benoist delves amply into Lacan's and Kristeva's notions of the productivity of the subject located in a decentered network (and so a subject not understood as individual subjectivity).

Chapter 6 recounts the merit of Lévi-Strauss's relational approach, in which thought is conceived as thought about relationships and not as a consciousness of an object. Lévi-Strauss holds a relational innatism closer to Piaget's position than to the subjectivist innatism of Chomsky: Benoist finds Lévi-Strauss's schematism and formal approach akin to Kant's *a priori* schemata, and the same is true of Lévi-Strauss's notion of a self-regulated transformational network. Lévi-Strauss does not abolish history but axiomatizes it and subordinates it to structure. One might argue that Lévi-Strauss seems to be a prisoner of the metaphysics of the "hereness" and the "present." However, for him myths continuously split off from themselves and continuously generate dissymmetries and discontinuities; through this process the system remains always decentered.

Where, then, is the locus of the structure? This is the question discussed in the last chapter of the book. Structure is not an essence nor does it have a cause; instead, structure is a strategy, an operational rule used to decode all aspects of human reality, including variations and changes. Benoist reiterates that the transcendental schematism of Kant lies at the origins of structural intelligence; schematism is also the absent source, prior to empirical intuition, from which tropes unfold in a web of invariances. Whereas transcendental schematism is the site of the decentered origin of structurality, heterogeneity—and not contradiction—is the constitutive mechanism of structurality (as Freud and Lacan have shown).

The reader of this volume is exposed to an intense series of insights and suggested connections among apparently heterogeneous ideas, a series not easy to assimilate. One cannot deny the brilliance of Benoist's interpretation of structuralism as an antiempiricist, antisubstantialist, anticausalist approach. Benoist is brilliant, too, in redeeming structuralism from the usual ingenuous objections of psychological reductionism, formalism, idealistic

combinatorics, and staticity. He also makes a skillful but selective use of Lacanianism to liberate Marxism from the unilineality of contradiction and to avoid the Althusserian idealistic combinatorics. The Freudian (Lacanian) input insures also an open, dynamic, and productive thrust of structurality which is presented as an operation of a mathematical and axiomatic nature. For this reason one must see in structuralism the culmen as well as the avenue for a synthesis of all forms of scientific endeavor.

The conventional sociologist will find in this volume enough material to allow him to reexamine stereotyped interpretations of the humanistic and antihumanistic forms of Marxism. The nonindividualistic notion of subjectivity, the dialectic and constitutive relationship between the structural and subjective principles of explanation, as well as the Lacanian interpretation of Freud, constitute a springboard for a profound rethinking of such fields as the sociology of socialization, the development of moral order, and the relationship between social structure and personality. More fundamentally, the structural perspective forces on the sociologist the arduous task of reelaborating traditional sociological paradigms and strengthening them with solid epistemological foundations and important analytical dimensions.

Natural Order: Historical Studies of Scientific Culture. Edited by Barry Barnes and Steven Shapin. Beverly Hills, Calif.: Sage Publications, Inc., 1979. Pp. 255. \$17.50 (cloth); \$8.95 (paper).

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For at least the past decade, scholars from various disciplines have paid increasing attention to the problematical nature of the selective accumulation of knowledge. This has given rise to a convergence of interest in the sociology, philosophy, and history of science. As a result of its interdisciplinary nature and its emergence in various social contexts, this field of inquiry has developed a diversity of theoretical orientations. One of the more recent approaches is exemplified in the work of a group of scholars primarily affiliated with Edinburgh University, scholars who maintain that scientific knowledge must be treated as an aspect of culture precisely like any other and that it should be analyzed in the same way and with the same social science methods as art, music, religion, or literature. *Natural Order: Historical Studies of Scientific Culture* is seen by its editors, Barry Barnes and Steven Shapin, as a "visible symbol" of this orientation. This new approach has its roots in the work of such social anthropologists as James Frazer, Mary Douglas, and Raymond Firth.

Several problems of current interest to sociologists of science are examined in the 10 case studies included in this book. Specific attention is paid to such problems as the development and evaluation of scientific knowledge, factors influencing receptivity toward and resistance to knowledge claims,

determinants and processes of controversy, and criteria utilized in choosing among competing theories. These problems are examined in various research sites, such as 18th-century Scottish physiology, the phrenological model of the brain in 19th-century Scotland, psychical research in late Victorian England, the current race/IQ controversy, and the biometry-Mendelism controversy.

Factors affecting the development and evaluation of scientific knowledge have been variously divided between the categories of factors internal and factors external to the rational growth of scientific knowledge. The relative emphasis on factors derived from one of these domains varies among scholars and has been a point of contention for many years among sociologists, philosophers, and historians of science. In most of the essays in *Natural Order*, contextual factors considered "external" to the so-called progressive and rational development of scientific knowledge and to the actual content of this knowledge are seen to impinge on its historical development. The editors of this book state that the distinction mentioned above is spurious and hope that with the application of the new approach it will be dropped eventually. However, they continue to use the words "internal" and "external" to distinguish between contextual factors. "Internal" refers to contextual factors within the boundaries of the scientific community and "external" to contextual factors outside those boundaries.

In several of the cases presented, notably those by Donald MacKenzie and Barry Barnes, Steven Shapin, Jonathan Harwood, John Dean, and Christopher Lawrence, the concept of "interest" is used as a heuristic device to locate contextual influences on knowledge that are external as well as internal to the scientific community and to interpret the connections between cognitive content and social context. Interests are said to "constrain and structure technical commitments and procedural choices" (p. 201), which in turn influence scientific evaluations.

The concept of "interest" stems from other "interest theories" which maintain that, by virtue of belonging to certain groups or occupying certain social positions, people share interests, aims, and goals which make their activities meaningful; it is the authors' belief that scientific knowledge is not immune to the influence of these interests. This concept, although well known in sociology, has not been generally applied to an analysis of those factors affecting the development and evaluation of scientific knowledge. According to this view, it is advantageous to scientists to make certain cognitive choices. Two types of interests affect these choices: interests in prediction and control and social interests. In most of the essays mentioned above, the influence of Jürgen Habermas is clear and acknowledged, but his ideas are somewhat modified. According to Habermas, natural scientific knowledge is produced and evaluated only in terms of interests in prediction and control because it can be appraised rationally in terms of an "objective reality." Such "cognitive interests," as they are labeled by MacKenzie in an earlier but related work, motivate and lead scientists to distinct evaluations of scientific knowledge; but the focus in these essays is on "social interests." Unlike Habermas, the authors in this book take these social

interests to influence the development and evaluation of scientific knowledge quite as much as do interests in prediction and control. These social interests stem both from elements of the wider social context (such as the economic, political, or religious sectors of the surrounding society) and from elements within the social context of the scientific community (such as professional socialization and training in various local settings and communication networks). In short, knowledge is evaluated with a "context of use."

The processes of scientific controversy are used by some of the authors as strategic research sites to illustrate the influence of interests. In their thought-provoking analysis of the biometry-Mendelism controversy, MacKenzie and Barnes argue that social interests led these two research groups to pursue "distinct historically specific projects" (p. 202) while pursuing their interests in prediction and control. According to the authors, this led the two research groups to arrive at particular predictions, techniques, and evaluations. In addition, the authors maintain that adherence to the biometric approach was "intimately bound up with" a sympathetic stance toward the eugenic movement and the social and political interests of the middle class, while the adherents of the Mendelian approach represented traditional conservative interests in British society. Evidence is drawn mainly from the careers of the founders of the biometric school, Karl Pearson and W. F. R. Weldon, and from the main proponent of Mendelism, William Bateson. However, such terms as "related to," "intimately bound up with," "sustained," or "reflect," used to describe the nature of the relationship between social interests and scientific knowledge, remain vague and unspecified.

In the section on scientific controversy, that concept remains largely unexplicated, being used to refer to conflicting evaluations by research scientists; as a result, the interpretative scheme remains too narrow. The interpretations might have benefited from drawing on developing efforts by Helga Nowotny, Gernot Böhme, and Stephen Toulmin, among others, to clarify the concept and to identify types of controversies and polemics.

MacKenzie and Barnes are not alone in their stress on the influence of class interests. Shapin too focuses on class interests as influencing the development of certain theories of the brain; and Harwood argues that U.S. social and economic policies have influenced the course and intensity of the current race/IQ controversy. Lawrence stresses the connection between views of the body in Scottish medicine and Scottish social and economic life in the 18th century. The influence of external cultural factors on the development and evaluation of scientific knowledge is demonstrated in the essay by Joan Richards who, in discussing the reception of non-Euclidean geometry, stresses the influence of the naturalist movement on its acceptance by certain English mathematicians. However, internal contextual influences are not neglected in this volume either. Dean focuses on the influence of differential disciplinary locations of groups with different professional interests on the formulation of two separate classifications of the plant genus *Gilia* in botany.

Two themes emerge from this collection of essays. One is the influence of predominantly social and cultural factors external to the scientific community on the growth of scientific knowledge. The other, however, is a focus on the influence of these factors on the actual cognitive content of scientific knowledge. This theme is new to the sociology of science.

Thus *Natural Order* is a welcome addition to the historical sociology of scientific knowledge, particularly for its elaboration of an interest doctrine. However, there remains the persistent problem of imputation. The next step, a difficult one, would be to specify further the nature of the relationship between interests and scientific knowledge. In other words, it remains to be seen how much of the observed variance in the development and evaluation of scientific knowledge is explained by "interests." Such a valuable research program need not deflect attention from alternative approaches that emphasize different social influences on the growth of knowledge. In fact, such a program should not replace but complement other approaches.

The Writer as Social Seer. By Robert N. Wilson. Chapel Hill: University of North Carolina Press, 1979, Pp. xiv+172. \$14.00.

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Robert Wilson quotes novelist Walker Percy: "The novelist is like the canary that coal miners used to take down into the shaft to test the air. When the canary gets unhappy, utters plaintive cries and collapses, it may be time for the miners to think things over" (p. xiv). If this is so, the sociologists of literature must analyze the canary's message, placing it in the context of society and culture and comparing it with other sources of sociological knowledge.

In his introductory chapter, Wilson distinguishes literature as a descriptive technique and an expression of society, and alludes to the pitfalls of a sociological approach that limits itself to stressing the impact of society on art while neglecting the reciprocal influence of art on society. But unfortunately he does not attend systematically to this precept in the bulk of *The Writer as Social Seer*, which consists instead of a series of relatively brief essays devoted to the thematic analysis of selected works by F. Scott Fitzgerald, Ernest Hemingway, Arthur Miller, Eugene O'Neill, James Baldwin, Boris Pasternak, Albert Camus, and Samuel Beckett.

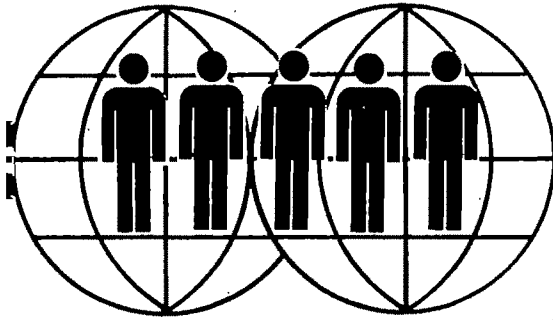
Wilson commands a fine prose style. He is a craftsman with highly developed literary sensibilities. I found each of these essays illuminating and perceptive. But again and again I asked myself: In what way do these essays differ in their approach from the work of literary critics such as Edmund Wilson, Lionel Trilling, Irving Howe, or Alfred Kazin? They do not indicate that the sociology of literature can contribute insights that

have not been the common coin of literary criticism. To be sure, Wilson refers occasionally to the work of social scientists such as Henry Murray, Gordon Allport, Karen Horney, Robert K. Merton, Erving Goffman, or Georg Simmel. But, in the first place, critics such as Kazin or Howe also draw on sources from social psychology or sociology, and, in the second place, references to social scientists in Wilson's book seem not particularly illuminating. I am not sure that one gains much insight into Willy Loman by learning that he fits neatly into the box of conformity in Merton's anomie paradigm, nor do I think that understanding of the pathologies of O'Neill's Tyrone family is enhanced by a fleeting reference to Goffman's strategies concerning the presentation of self.

What this book offers, and it is a rich offering, is a personal and sensitive account of the author's encounter with literary texts that have moved him. Some themes that attract him concern the ambiguities of success as well as failure in American society, the counteracting pulls of ideology and privacy, the tormented web of family, and the dialectics of love and race. In each case, Wilson has something worthwhile to say.

Frederick Engels once wrote that the works of Balzac provided a splendid description of the society of Louis Philippe. But Oscar Wilde made the witty remark that (I quote from memory) almost all we know about the July monarchy is an invention of Balzac. The juxtaposition of these two statements could provide an entry to discussion of some of the problems of the sociology of literature. To what extent do the literary canaries (or, to vary the metaphor, those antennae of the race of whom Ezra Pound wrote) describe the social world of their actors, and to what extent do they distort that world by their own angle of vision? Furthermore, to what extent do the audiences of literary works distort the author's intended message by their own perception and intentions? I hope that in his future work Wilson will address himself to some of these basic questions, and that, in particular, he will pay attention to audiences, the public, and the reception of works of art. Thematic analysis is not enough.

In summary, readers of this slim volume will be likely to derive a great deal of pleasure from it, but they will have to wait for Wilson to supply proof of the contention that sociologists of literature can do more than follow in the wake of literary critics.



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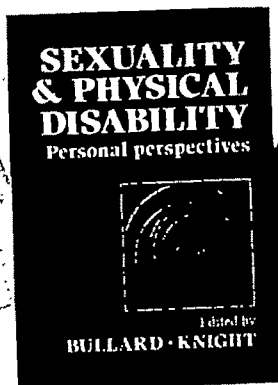
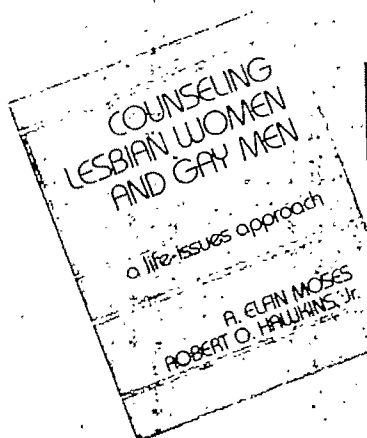
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IN THIS ISSUE

MELVIN L. KOHN is chief and CARMi SCHOOLER a senior investigator in the Laboratory of Socio-environmental Studies, National Institute of Mental Health. Their article in this issue will be included in a book about their longitudinal investigations of social stratification, job conditions, and personality. Related areas of investigation include cross-national studies, being done in collaboration with Polish and Japanese sociologists, and a study of "educational self-direction" and the personality development of children, being done with Karen A. Miller.

IDA HARPER SIMPSON is professor of sociology at Duke University. She is currently studying effects of changes in the industry of agriculture on farmers, farming, and rural communities.

RICHARD L. SIMPSON is Kenan Professor of Sociology at the University of North Carolina at Chapel Hill. He is currently exploring the connections between types of intraoccupational labor market structures and on-the-job control systems, power relations, and worker behavior.

MARK EVERS is visiting assistant professor in the College of Business Administration at the University of Oregon. His current research centers on the use of log-multiplicative models in the analysis of ordinal variables.

SHARON SANDOMIRSKY POSS, senior computer programmer for the Department of Sociology, Duke University, is interested primarily in methodology and computer applications. Her most recent work focuses on multiple-cause mortality models, discrete data classification, and demographic microsimulation.

DAVID KNOKE is professor of sociology at Indiana University, where he specializes in the study of political behavior. With Edward O. Laumann, he is investigating the social organization of the national energy and health policy domains. His most recent books are *Organized for Action: Commitment in Voluntary Associations* (with James R. Wood) and *Statistics for Social Data Analysis* (with George W. Bohrnstedt).

DAVID P. PHILLIPS, associate professor of sociology at the University of California, San Diego, continues to focus his research on two major issues in public policy: (1) the effect of the mass media on violence, and (2) the effect of capital punishment on homicide. More generally, he is concerned with elucidating the social and psychological factors affecting mortality.

RONALD ANGEL is a postdoctoral fellow in Mental Health Services Research at Rutgers University. His current research involves the influence of the family and social support networks on the mental health and mental health care utilization of various Hispanic groups.

Information for Contributors

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Job Conditions and Personality: A Longitudinal Assessment of Their Reciprocal Effects¹

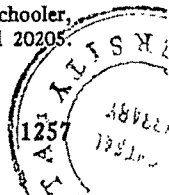
Melvin L. Kohn and Carmi Schooler
National Institute of Mental Health

In earlier work, we assessed a longitudinal causal model of the reciprocal effects of the substantive complexity of work and intellectual flexibility. In this paper, we greatly expand the causal model to consider simultaneously several structural imperatives of the job and three major dimensions of personality—ideational flexibility, a self-directed orientation to self and society, and a sense of distress. The analysis demonstrates that the structural imperatives of the job affect personality. Self-directed work leads to ideational flexibility and to a self-directed orientation to self and society; oppressive working conditions lead to distress. These findings strongly support a learning-generalization model. Personality, in turn, has important consequences for an individual's place in the job structure and in the system of social stratification. In particular, both ideational flexibility and a self-directed orientation lead, over time, to more responsible jobs that allow greater latitude for occupational self-direction.

In this paper we assess the effects of men's working conditions on their personalities and the effects of their personalities on their working conditions. Our interest in this problem was aroused some years ago, when we tested the hypothesis (proposed in Kohn 1963) that the conditions of work associated with social class position are a principal mechanism by which class exerts its psychological impact (see Kohn and Schooler 1969; Kohn 1969, chaps. 9 and 10; Pearlin and Kohn 1966). Subsequently, Kohn (1971) similarly analyzed bureaucratization, attempting to specify which of the conditions characteristic of bureaucratic employment account for

¹ We are indebted to several people for essential help: to Ronald Schoenberg for repeatedly and ingeniously modifying the computer program to meet our ever-increasing needs and for advice on identifying complex reciprocal-effects models; to Carrie Schoenbach, Bruce Roberts, and Margaret Renfors for conscientious and thoughtful computer programming and data analysis; to Virginia Marbley for uncomplainingly and effectively transcribing innumerable revisions of this paper; and to Carrie Schoenbach, Ronald Schoenberg, Paul D. Allison, Jeffrey K. Liker, Jeylan T. Mortimer, Karen A. Miller, Kazimierz Slomczynski, and Joanne Miller for critical readings of earlier versions of this paper. The models in this paper were estimated by MLIS, an advanced version of LISREL (Jöreskog and van Thillo 1972) developed by Ronald Schoenberg. Requests for reprints should be sent to Melvin L. Kohn/Carmi Schooler, National Institutes of Health, Building 31, Room 4C-11, Bethesda, Maryland 20205.

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the correlations between personality and employment in bureaucracy. In those studies, we recognized explicitly that correlations between job and personality do not necessarily mean that conditions of work have a causal impact on personality. The correlations may reflect entirely processes of selective recruitment, selective retention, and the ongoing efforts of workers to mold their jobs to match their needs, values, and abilities.

Still using cross-sectional data, we next attempted to analyze empirically whether work affects or only reflects personality (Kohn and Schooler 1973; Kohn 1976). We used two-stage least squares as our method of estimating the reciprocal effects of one crucial condition of work—its substantive complexity—and several facets of psychological functioning. Those analyses yielded *prima facie* evidence that job conditions actually do affect psychological functioning, thus justifying the considerable investment of time, effort, and money needed to secure the longitudinal data necessary for a more definitive assessment.²

With longitudinal data, using confirmatory factor analysis and linear structural equations causal analysis, we did a prototypic longitudinal analysis of the reciprocal effects of the substantive complexity of work and intellectual flexibility (Kohn and Schooler 1978). That analysis provided convincing evidence that the substantive complexity of work both affects and is affected by this one, obviously important, facet of psychological functioning. In the present analysis, we enlarge the causal model to take into account not only a broader range of job conditions (as we did in Kohn and Schooler 1981) but also a broader range of psychological variables. The goal of this paper is to develop and assess a general model of the reciprocal effects of job conditions and major dimensions of personality.

DATA

The baseline data come from interviews conducted in 1964 with a sample of 3,101 men, representative of all men employed in civilian occupations in the United States. (For more specific information on sample and research design, see Kohn [1969], pp. 235–64.) In 1974 the National Opinion Research Center (NORC) carried out a follow-up survey for us, interviewing a representative subsample of approximately one-fourth of those men who were still under 65 years of age. (For an assessment of the rep-

² Further evidence of job conditions affecting critical aspects of personality is provided by subsequent studies. Many of these studies are reviewed in Kohn (1977, 1981). The most pertinent are the longitudinal studies by Mortimer and Lorence (1979a, 1979b). Also pertinent are the longitudinal studies by Andrisani and Abeles (1976), Andrisani and Nestel (1976), and Brousseau (1978), and the cross-sectional studies by Coburn and Edwards (1976), Grabb (1981), Hoff and Grüneisen (1977), St. Peter (1975), and our colleagues' and our study of women's job conditions and psychological functioning (Miller et al. 1979).

representativeness of the original sample, see Kohn [1969], appendix C. For an assessment of the follow-up sample, see Kohn and Schooler 1978.)

JOB CONDITIONS

The present analysis focuses on 14 job conditions that have been shown to have substantial impact on men's psychological functioning, independent of other pertinent job conditions and of education (see Kohn and Schooler 1973). Together they identify a man's place in the organizational structure, his opportunities for occupational self-direction, the principal job pressures to which he is subject, and the principal extrinsic risks and rewards built into his job.

Specifically, we consider these aspects of an individual's place in the organizational structure: ownership, bureaucratization, and hierarchical position. The facets of occupational self-direction that we measure are the substantive complexity of the work, the closeness of supervision, and the degree of routinization. The job pressures are time pressure, heaviness, dirtiness, and the number of hours worked in the average week. The extrinsic risks and rewards are the probability of being held responsible for things outside one's control, the risk of losing one's job or business, job protections, and job income. We call these conditions *structural imperatives* of the job.³ They are "structural" in two senses: they are built into the structure of the job, and they are functions of the job's location in the structures of the economy and the society. These job conditions are "imperatives" in that they define the occupational realities that every worker must face.

A Measurement Model of Occupational Self-Direction

We begin with a measurement model of *occupational self-direction*, by which we mean the use of initiative, thought, and independent judgment in work. Since the three job conditions that facilitate or inhibit the exercise of occupational self-direction—substantive complexity, closeness of supervision, and routinization—reflect one overarching concept, and since we have multiple indicators of two of them, we have developed a combined model, presented in figure 1, that encompasses all three.⁴

³ In an earlier analysis (Kohn and Schooler 1973) we used the term "structural imperatives of the job" to describe a similar but not identical set of job conditions. For an explanation of the changes, see Kohn and Schooler (1981).

⁴ In fig. 1, in all subsequent figures, and in the text, all paths and correlations are expressed in standardized form. Standardized values are more easily comprehended than are metric values when the metric is not inherently meaningful. All computations have been based on unstandardized variance-covariance matrices.

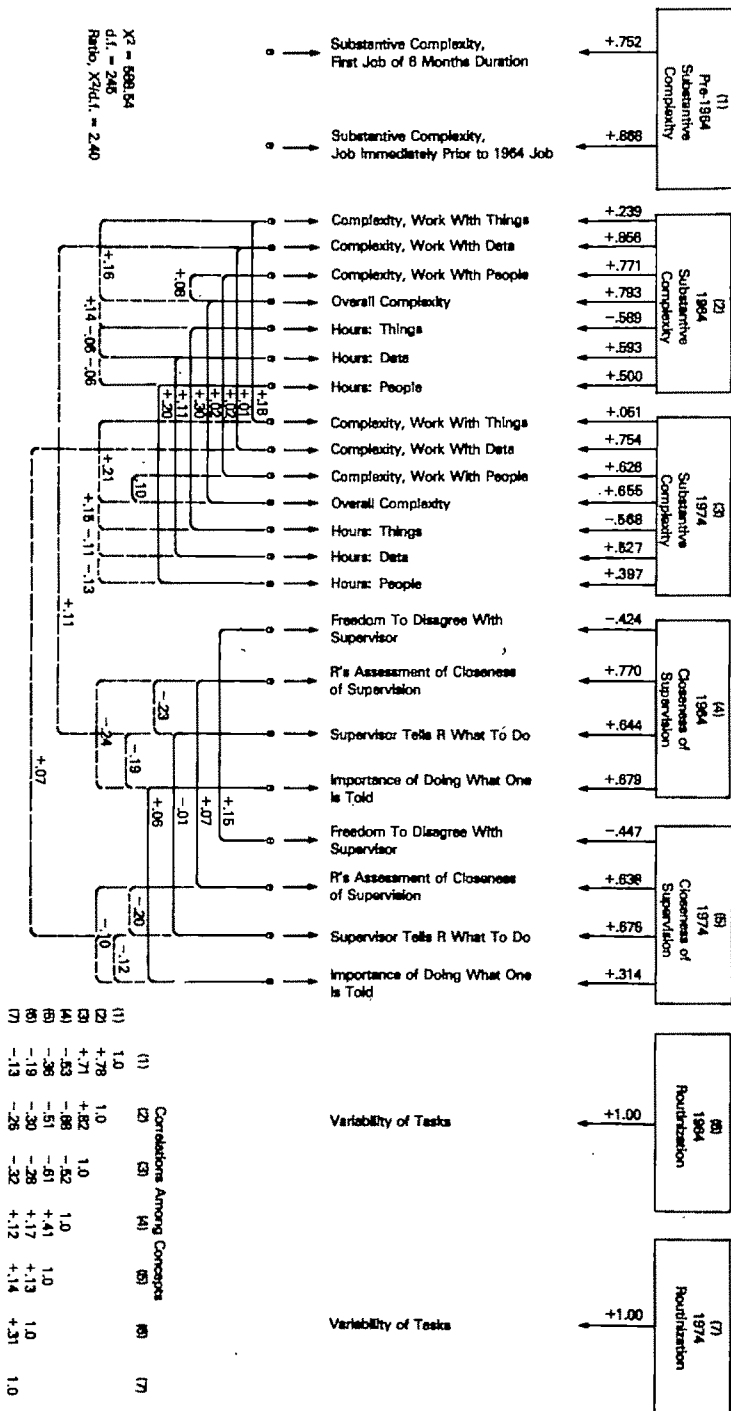


FIG. 1.—Measurement model for occupational self-direction (figures shown are standardized).

We define the *substantive complexity of work* as the degree to which performance of the work requires thought and independent judgment. Substantively complex work by its very nature requires making many decisions that must take into account ill-defined or apparently conflicting contingencies. Detailed questioning of each respondent in 1964 and again in 1974 (see Kohn 1969, pp. 153–55, 271–76; or Kohn and Schooler 1978) provides the basis for seven ratings of the substantive complexity of each job: appraisals of the complexity of the man's work in that job with things, with data, and with people; an overall appraisal of the complexity of his work; and estimates of the amount of time he spends working at each type of activity. The ratings are treated in figure 1 as indicators of the underlying but not directly measured concept, the substantive complexity of that job. We have information also about the complexity of each man's work in two earlier jobs, based on extrapolations from job history information (see Kohn and Schooler 1973, pp. 111–12 and n. 21), extrapolations that we use as indicators of a single concept, earlier (pre-1964) substantive complexity.

Closeness of supervision limits one's opportunities for occupational self-direction: a worker cannot exercise occupational self-direction if he is closely supervised, although not being closely supervised does not necessarily mean that he is required or even free to use initiative, thought, and independent judgment. Closeness of supervision is measured by a worker's subjective appraisals of his freedom to disagree with his supervisor, how closely he is supervised, the extent to which his supervisor tells him what to do instead of discussing it with him, and the importance in his job of doing what one is told to do.

Routinization is the final facet of occupational self-direction; highly routinized (repetitive and predictable) jobs restrict possibilities for exercising initiative, thought, and judgment, while jobs with a variety of unpredictable tasks may facilitate or even require self-direction. Respondents' work was coded from most variable (the work involves doing different things in different ways and one cannot predict what may come up) to least variable (the work is unvaryingly repetitive).

The measurement model for occupational self-direction, including the error correlations depicted in figure 1, provides a good fit to the variance-covariance matrix of the indicators. The $\chi^2 = 588.54$, with 245 degrees of freedom (df), for a ratio of 2.40.

A Model of Job Structure

Now we expand the measurement model of occupational self-direction into a causal model of job structure. As shown in figure 2, this model includes

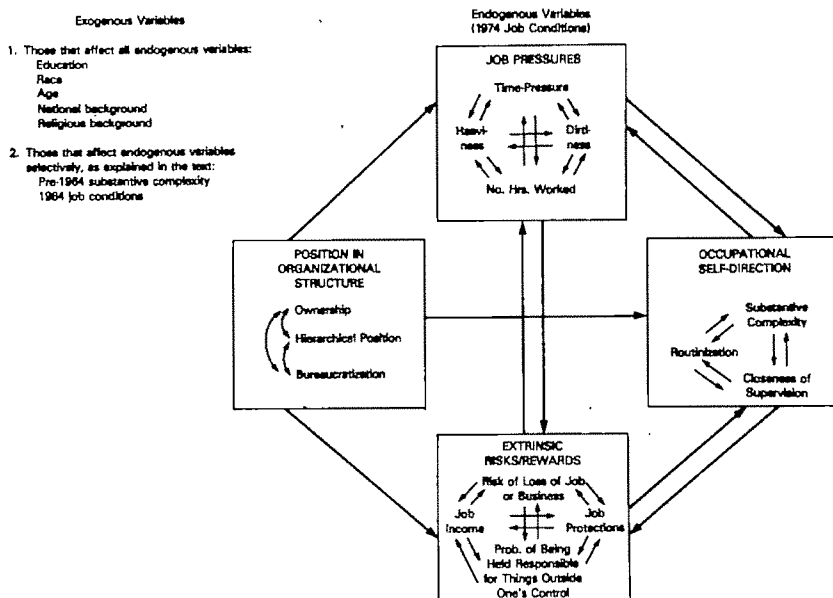


FIG. 2.—A priori model: job structure

several other structural imperatives of the job.⁵ Since we do not have multiple indicators of any of these job conditions, it is not possible to develop measurement models for any of them.⁶ We therefore use single-indicator measures of these job conditions, recognizing that each is subject to some unknown degree of measurement error, which may result in our under-

⁵ Of the three aspects of a man's position in the organizational structure that we measure, bureaucratization of the firm or organization in which he is employed is indexed on the basis of the number of formal levels of supervision and the size of the organization (see Kohn 1971), ownership/nonownership is based on his self-report, and position in the supervisory hierarchy is measured in terms of the number of people over whom he says he has direct or indirect supervisory authority. Of the four job pressures, three are measured by the respondent's appraisals—frequency of time pressure, how dirty he gets in his work, and the number of hours he works in an average week. The fourth, heaviness of work, is our appraisal, based on his description of his work with things. Extrinsic risks and rewards are measured by the individual's perceptions of the likelihood of being held responsible for things outside his control and of the risk of losing his job or business, his reported income, and a simple additive index of whether his job provides such benefits as job security and sick leave. These measures are described more fully in Kohn and Schooler (1973).

⁶ Nor do we wish to incorporate the specific job conditions into more general measurement models of organizational structure, job pressures, or extrinsic risks and rewards, because to do so would take away our ability to assess the separate effects of particular job conditions. For example, if we used time pressure, heaviness, dirtiness, and number of hours worked as indicators in a measurement model of job pressure, we would no longer be able to examine the separate effects of each of the four types of job pressures.

estimating the effects of these job conditions on each other, on other job conditions, and, in later models, on personality.

The causal model includes as potentially pertinent exogenous variables not only the pre-1974 job conditions but also those social characteristics that the research literature and our own earlier analyses give us reason to believe might have affected the job placement of men who are at least 10 years into their occupational careers. These are the respondent's level of education, race, age, national background, and religious background.⁷ Our model should therefore be thought of as a depiction of men-in-jobs rather than as an abstract picture of job structure per se. We deliberately omit other social characteristics (e.g., parental socioeconomic characteristics) that might have affected job placement at earlier career stages but should not directly affect job placement once men are well into their careers.

With some exceptions, to be discussed below, there is no reason in principle why the 14 job conditions might not affect each other both contemporaneously (albeit not necessarily instantaneously) and over time. Therefore we should prefer to assess causal models that simultaneously allow both "contemporaneous" effects, that is, the effects of present job conditions on each other, and "lagged" effects, that is, the effects of the conditions of work in the job held 10 years earlier on present job conditions. (There are two types of lagged effects: "stabilities," i.e., the effects of job conditions in 1964 on the same job conditions in 1974, and "cross-lagged" effects, i.e., the effects of job conditions in 1964 on other job conditions in 1974.) Unfortunately, if our model were to allow both contemporaneous and cross-lagged effects, the number of parameters to be estimated would surpass what is possible with the information available.

To identify the equations, we must assume that some effects cannot be direct but only indirect. (A variable that identifies an equation by not being allowed to have a direct effect on the dependent variable of that equation is called an instrument.) If we had pre-1964 measures of all structural imperatives of the job—as we have for substantive complexity—those measures would provide the instrumentation for simultaneous assessment of the contemporaneous and cross-lagged effects of job conditions on one another. Lacking such instrumentation, we must give priority to testing either contemporaneous or cross-lagged effects. Our choice is to give priority to contemporaneous effects, using the 1964 job conditions as instruments to identify the effects of 1974 job conditions on one another.

We give priority to contemporaneous effects for two reasons. First, we must take seriously the structural integrity of the job. It would deny the

⁷ The indices of national background and religious background are linear approximations to these concepts (see Schooler 1972, 1976; Kohn and Schooler 1978).

reality of job structure to assume, for example, that the likelihood of being held responsible for things outside one's control depends more on the time pressure experienced in some past job than on the time pressure experienced in the present job. Second, the simultaneous assessment of contemporaneous effects provides a straightforward way of accomplishing our principal intent—to decompose the correlation between, for example, the substantive complexity of men's current jobs and the closeness with which men are supervised in these jobs. Testing the effects of 1964 substantive complexity on 1974 closeness of supervision and of 1964 closeness of supervision on 1974 substantive complexity—while clearly pertinent—does not deal as directly with the critical correlation. If the interval between measurements were 10 days or even 10 weeks, giving priority to lagged effects might very well be justified. But with a 10-year interval between measurements, an unequivocal test of reciprocity can be accomplished only by allowing reciprocal contemporaneous effects.

When in a model that tests only contemporaneous effects we find that some job condition (say, closeness of supervision) significantly affects some other job condition (say, substantive complexity), we can reasonably assume that the effect is real, although we do not know whether it is entirely contemporaneous. When, however, that effect proves not to be statistically significant in such a model, we have no basis for concluding that supervision really has no impact on substantive complexity. A full model, depicting both contemporaneous and lagged effects, might show that 1964 closeness of supervision has had a significant lagged effect on 1974 substantive complexity; a full model might even reveal a significant contemporaneous effect that was not previously apparent.

Being unable to test a full model, we search now for effects that we may have missed, by fixing at zero the paths that were nonsignificant in the contemporaneous-only model and estimating the corresponding lagged paths.⁸ If we were again to find that closeness of supervision has no statistically significant effect on substantive complexity, we could conclude that closeness of supervision would not affect substantive complexity even in a full model. However, if we were now to find a significant lagged effect, we could be reasonably confident that closeness of supervision does affect substantive complexity. We could not, however, be certain that this effect is entirely lagged, for in a model that does not simultaneously allow a contemporaneous effect, a lagged effect has two components. One is the truly lagged effect of 1964 closeness of supervision on 1974 substantive

⁸ Fixing the nonsignificant contemporaneous paths at zero in no way distorts the models presented here. In all instances, those paths are so small that removing them from the models does not change the magnitudes of the remaining parameters to more than a trivial extent. Nor does adding lagged paths affect the magnitudes of the contemporaneous paths in any of our models.

complexity. The other is the combination of the lagged effect of 1964 closeness of supervision on 1974 closeness of supervision and the contemporaneous effect of 1974 closeness of supervision on 1974 substantive complexity. Thus we cannot be confident that what appear to be lagged effects of job conditions on one another are truly lagged, but, whether they are in fact lagged, contemporaneous, or both, they are real.

In assessing the model of job structure, we must recognize that the individual's place in the organizational structure—as defined by ownership, bureaucratization, and hierarchical position—cannot be contemporaneously affected by any other job condition. This is definitionally true, for a change from ownership to nonownership or the reverse, or from a more bureaucratic to a less bureaucratic firm or organization, or the reverse, or from a higher to a lower position in the supervisory hierarchy, or the reverse, signifies a change in job. It is not even possible for the three aspects of organizational structure to affect each other without the job becoming a different job.⁹ But if there can be no contemporaneous effects of job conditions on ownership, bureaucratization, or hierarchical position, then all lagged effects on ownership, bureaucratization, and hierarchical position are identified and can be tested.

The model, thus identified, is presented in table 1. From this table, we learn the following.

1. On the most general level: job conditions are intricately linked; all structural imperatives of the job affect and are affected by one or more of the others.

2. Position in the organizational structure has a widespread impact on other conditions of work. Ownership results in doing substantively more complex work, at higher levels of income, but with a greater risk of losing one's job or business. Bureaucratic firms and organizations provide substantively more complex work, more extensive job protections, higher income, physically lighter work, and fewer hours of work. Higher position in the supervisory hierarchy results in substantively more complex, less routine, less closely supervised, and physically lighter work; higher levels of pay; and more hours of work per week. In short, the three facets of position in the organizational structure have similar effects on substantive complexity and job income but decidedly different effects on the number of hours worked and on job protections and job risks, with ownership maximizing risk and bureaucratization maximizing job protections.

3. The substantive complexity of work stands out as the keystone of the entire job structure—affected by and, in turn, affecting many other job conditions. Not only do ownership, bureaucratization, and hierarchical

⁹ Because we do not allow ownership, bureaucratization, or hierarchical position to affect each other, we allow their residuals to correlate.

TABLE 1—A MODEL OF JOB STRUCTURE

STATISTICALLY SIGNIFICANT EFFECTS OF:							
ON 1974	Substantive Complexity	Routinization	Closeness of Supervision	Ownership	Bureaucratization	Position in Hierarchy	Time Pressure
Substantive complexity.....	.36(L)	-.11(C)	-.24(C)	.09(C)	.13(C)	.18(C)	.06(C)
Routinization.....	.0	.27(L)	.0	.0	.0	-.09(C)	.0
Closeness of supervision.....	-.31(L)	.0	.08(L)	.0	.0	-.15(C)	.0
Ownership.....	.08(L)	.0	.0	.55(L)	.0	.0	.0
Bureaucratization.....	.0	.0	.0	-.13(L)	.48(L)	.0	.0
Position in hierarchy.....	.20(L)	.0	.0	.0	.0	.30(L)	.10(L)
Time pressure.....	.11(C)	.0	.0	.0	.0	.0	.30(L)
Heaviness.....	.0	-.12(C)	.0	.0	-.09(C)	-.10(C)	.0
Dirtiness.....	-.40(C)	-.11(C)	.0	.0	.0	.0	.0
Hours of work.....	.10(C)	.0	.0	.0	-.23(C)	.06(C)	.0
"Held responsible".....	.17(C)	-.08(L)	.0	.0	.0	.0	.17(C)
Risk of loss of job.....	.0	.0	.0	.32(C)	.0	.0	.0
Job protections.....	.0	.11(L)	.0	.0	.35(C)	.0	.0
Job income.....	.17(C)	.0	.0	.20(C)	.18(C)	.15(C)	.0
	Heaviness	Dirtiness	Hours of Work	"Held Responsible"	Risk of Loss of Job	Job Protections	Job Income
Substantive complexity.....	.0	.0	.0	.0	.0	.08(L)	.0
Routinization.....	.0	.0	.0	.0	.0	.0	-.10(L)
Closeness of supervision.....	-.11(C)	.10(L)	.0	.11(C)	.19(C)	.06(C)	-.12(L)
Ownership.....	.0	.0	.0	.0	.0	-.09(L)	.0
Bureaucratization.....	.0	.0	.0	-.05(L)	.0	.0	.0
Position in hierarchy.....	.0	.0	.0	.0	.0	.0	.0
Time pressure.....	.0	.0	.0	.0	.0	.0	.14(C)
Heaviness.....	.21(L)	.26(C)	.09(L)	-.07(C)	.0	.0	.0
Dirtiness.....	.0	.39(L)	.15(C)	.0	.0	.0	.0
Hours of work.....	.0	.15(L)	.16(L)	.0	.0	.0	.24(C)
"Held responsible".....	.0	.0	.08(C)	.24(L)	.0	.0	.0
Risk of loss of job.....	.0	.0	.0	.0	.21(L)	.0	.0
Job protections.....	.13(C)	.0	.0	.0	.0	.34(L)	.0
Job income.....	.0	-.11(C)	.0	.0	.0	.0	.34(L)

NOTE.—Paths from pre-1964 substantive complexity and from social background characteristics are not shown in this table. L = a lagged effect (e.g., of 1964 substantive complexity on 1974 substantive complexity); C = a contemporaneous effect (e.g., of 1974 substantive complexity on 1974 time pressure); .0 means a nonsignificant effect that has subsequently been fixed at zero.

position increase the substantive complexity of work, but so, too, do non-routinized working conditions, freedom from close supervision, greater time pressure, and, over time, job protections. The substantive complexity of work, in turn, affects several other aspects of work: doing substantively more complex work results in doing work that is less dirty and increases the probability of being held responsible for things outside one's control, of receiving higher income, working under greater time pressure, and working longer hours. Over time, doing substantively more complex work results also in being less closely supervised, rising in the supervisory hierarchy, and becoming an owner. In short, substantively complex work lies at the core of highly placed, responsible, demanding but rewarding jobs. It is the key link between the position of a job in the organizational structure and the other, more proximate, conditions of work. No other job condition is as intricately bound to the entire set of structural imperatives of the job as is substantive complexity.

4. Finally, with only one possible exception, all the statistically significant effects of job conditions on one another are consonant with past knowledge about the organization of work. They can be seen as the outcomes either of direct processes (e.g., close supervision decreasing substantive complexity) or of well-known indirect processes (e.g., routinization decreasing heaviness and dirtiness through mechanization). The only finding that may contradict expectations is that heavy work results in more freedom from supervision; but, then, the expectation may be stereotypic, based perhaps on our not distinguishing heavy from dirty work. It is pertinent that jobs requiring heavy work tend to be performed out of sight of supervisors—jobs such as farming, construction carpentry, and long-haul trucking (see Kohn and Schooler 1973, table 2).

Overall, then, the model demonstrates that job conditions are linked intricately and meaningfully, with position in the organizational structure having widespread effects on other job conditions, and with the substantive complexity of work pivotal to the entire job structure.

JOB CONDITIONS AND IDEATIONAL FLEXIBILITY

By enlarging the model of job structure to include one or another facet of personality, we can assess the reciprocal effects of job conditions and each facet of personality. We begin by assessing the reciprocal effects of job conditions and ideational flexibility.

In our surveys, intellectual flexibility is evidenced by performance in handling cognitive problems that require weighing both sides of an economic or a social issue, in differentiating figure from ground in complex color designs, and in drawing a recognizably human figure whose parts fit together in a meaningful whole. We also include the interviewers' evalua-

tions of the respondents' "intelligence" and a simple count of the respondents' propensity to agree with agree-disagree questions. All these we take to reflect, in some substantial part, intellectual flexibility. We use the measurement model for intellectual flexibility that was developed in Kohn and Schooler (1978). There are two components to intellectual flexibility, one ideational, the other perceptual. The ideational aspect concerns us here.

Identifying the effects of job conditions on ideational flexibility poses the same problems that we faced in the model of job structure, and we follow the same procedures. Identifying the effects of ideational flexibility on job conditions, however, is greatly facilitated if we posit that social characteristics that employers (even discriminatory ones) would not interpret as job credentials do not directly affect the job conditions of men who are at least 10 years into their work careers. The rationale is that maternal and paternal education, paternal occupational status, maternal and paternal grandfathers' occupational status, urbanness and region of origin, and number of children in the parental family may have affected job placements earlier in the men's careers, but that by the time of the follow-up study these noncredentialing social characteristics would no longer have any direct bearing on job placement. Thus they can be used as instruments to identify the contemporaneous effects of ideational flexibility on job conditions, a procedure that permits us to test the lagged effects of ideational flexibility even when its contemporaneous effects are statistically significant. We can therefore be much more confident about distinguishing the contemporaneous from the lagged effects of ideational flexibility on job conditions than we can about distinguishing the contemporaneous from the lagged effects of job conditions on other job conditions or on ideational flexibility.

As in the model of job structure, we must recognize that ownership, bureaucratization, and hierarchical position cannot be contemporaneously affected by anything else. This poses a dilemma: how best to model the effects of ownership, bureaucratization, and hierarchical position on ideational flexibility? On the one hand, the logic of assessing the directions of effects in the relationships between job conditions and ideational flexibility requires the simultaneous testing of pairs of reciprocal paths—from some job condition to ideational flexibility and from ideational flexibility to that same job condition. Since ideational flexibility cannot have direct contemporaneous effects on ownership, bureaucratization, and hierarchical position, allowing these job conditions to have direct contemporaneous effects on ideational flexibility would assume the very unidirectionality of effects that our entire analysis is designed to transcend. We would be prejudging the issue we are trying to evaluate. Fortunately, there is nothing to preclude our allowing ownership, bureaucratization, and hierarchical

position to have either direct lagged effects or indirect contemporaneous effects on ideational flexibility. On the other hand, to say that ownership, bureaucratization, and hierarchical position may have direct lagged or indirect contemporaneous effects on ideational flexibility does not necessarily prove that, in reality, they do not have direct contemporaneous effects. We deal with the dilemma by testing and comparing two alternative models—initially, a model that allows position in the organizational structure to have only indirect contemporaneous and direct lagged effects, later a model that allows organizational position to have direct contemporaneous effects on ideational flexibility.

The initial model of job conditions and ideational flexibility is presented in table 2. Since the effects of job conditions on one another are essentially unchanged by adding ideational flexibility (or any other facet of personality) to the model of job structure, we do not repeat in table 2 that part of the model already presented in the previous table.¹⁰ Three things stand out in this model.

1. Several conditions of the current job have statistically significant effects on current ideational flexibility, even with earlier ideational flexibility, pertinent social characteristics, and other job conditions statistically controlled. Doing substantively complex work, receiving higher income, and being at risk of losing one's job or business all increase ideational flexibility, whereas being closely supervised, doing heavy work, and thinking that one may be held responsible for things outside one's control tend to decrease ideational flexibility. In addition, time pressure in the job one held 10 years ago increases current ideational flexibility, while routinization and job protections in the earlier job decrease current ideational flexibility. The magnitudes of these effects are only small to moderate (ranging from 0.03 to 0.13), but it is nevertheless impressive that so many of the structural imperatives of the job significantly affect ideational flexibility, even under such stringent statistical controls. Overall, job conditions that facilitate or require intellectual alertness seem to increase ideational flexibility as measured in a nonwork situation; job conditions that minimize the necessity or desirability of intellectual alertness seem to decrease ideational flexibility. It is noteworthy that the substantive complexity of work, which we found to be central to the entire job struc-

¹⁰ Although adding ideational flexibility or any other facet of personality to the model of job structure never greatly affects the estimates of job effects on one another or the effects of social characteristics on job conditions, in a few instances an effect does become statistically nonsignificant. When this happens, we follow a practice used throughout this paper, fixing the nonsignificant effect at zero. We do so primarily to keep the number of estimated parameters within the capacity of the computer program. With an *N* of 687 and a robust model, statistically nonsignificant paths are ordinarily so small that it makes no difference to the remainder of the model whether they are left in or fixed at zero.

ture, has the strongest direct effect on ideational flexibility of any job condition.

2. Although this model does not permit ownership, bureaucratization, and hierarchical position to have direct contemporaneous effects on ideational flexibility, and although we find that they do not have statistically significant lagged effects, they do have modest indirect effects. All three aspects of position in the organizational structure affect conditions of work that bear on ideational flexibility; in particular, all three affect the substantive complexity of work. The alternative model—which permits po-

TABLE 2
RECIPROCAL EFFECTS OF JOB CONDITIONS AND IDEATIONAL FLEXIBILITY

	STATISTICALLY SIGNIFICANT EFFECTS OF:	
	Job Conditions (and Background Variables) on Ideational Flexibility	Ideational Flexibility on Job Conditions*
Job conditions:		
Substantive complexity.....	.13(C)	.31(L)
Routinization.....	-.04(L)	.0
Closeness of supervision.....	-.03(C)	-.17(L)
Ownership.....	.0	.07(L)
Bureaucratization.....	.0	.07(L)
Position in hierarchy.....	.0	.0
Time pressure.....	.10(L)	.0
Heaviness.....	-.07(C)	.0
Dirtiness.....	.0	.0
Hours of work.....	.0	-.23(L)
"Held responsible".....	-.06(C)	.0
Risk of loss of job or business.....	.03(C)	.0
Job protections.....	-.09(L)	-.14(L)
Job income.....	.06(C)	.0
Background variables:		
Education.....	.0	...
Race.....	.0	...
Age.....	-.14	...
National background.....	.04	...
Religious background.....	.0	...
Number of siblings.....	.0	...
Region of origin.....	.08	...
Urbanness of place raised.....	-.03	...
Mother's education.....	-.03	...
Father's education.....	.07	...
Father's occupational status.....	.0	...
Maternal grandfather's occupational status.....	-.03	...
Paternal grandfather's occupational status.....	.0	...
Stability of ideational flexibility.....	.70(L)	...

NOTE.—C = a contemporaneous effect; L = a lagged effect; .0 means a nonsignificant effect that has subsequently been fixed at zero.

* Effects of background variables and other job conditions on job conditions are not shown in this table.

sition in the organizational structure to have direct, unidirectional effects on ideational flexibility—sets upper limits for our estimates of the direct effects on ideational flexibility of ownership, bureaucratization, and hierarchical position and lower limits for our estimates of the direct effects on ideational flexibility of all other job conditions. Such a model shows that neither ownership nor bureaucratization would have a statistically significant direct effect on ideational flexibility. Hierarchical position would have a statistically significant direct effect, a path of 0.09. The direct contemporaneous effects of other job conditions would be somewhat reduced; in particular, the direct effect of substantive complexity on ideational flexibility would be reduced from 0.13 to 0.10. Juxtaposing the two models, we conclude that hierarchical position has a direct contemporaneous effect on ideational flexibility of no more than 0.09, while substantive complexity has a direct contemporaneous effect on ideational flexibility of no less than 0.10 and no more than 0.13. The unresolved issue is simply the degree to which the effect of hierarchical position is mediated through substantive complexity. In any case, both substantive complexity and hierarchical position are clearly pertinent to ideational flexibility.

3. The effects of ideational flexibility on job conditions are impressive, albeit entirely lagged, suggesting that the process by which ideational flexibility affects conditions of work is primarily one of selective recruitment and retention, not one of job molding, which we would expect to be more contemporaneous. Greater ideational flexibility in 1964 is conducive to working at jobs of greater substantive complexity in 1974, with less supervision, fewer hours of work, and fewer job protections. Greater ideational flexibility also increases the probability of becoming an owner or working in a bureaucratic firm or organization. In sum, the long-term consequence of greater ideational flexibility is the increased likelihood of attaining a self-directed position. Although this process occurs only gradually, over the course of time its cumulative impact is far from negligible.

SELF-CONCEPTION AND SOCIAL ORIENTATION

Do job conditions affect, and are they affected by, other facets of personality in the same way that they affect and are affected by ideational flexibility? Does the substantive complexity of work play a pivotal role vis-à-vis self-conception and social orientation? Alternatively, might time pressure or job protections or dirtiness be more important than substantive complexity for such facets of personality as anxiety or self-esteem? To answer these questions, we substitute for ideational flexibility each of several aspects of self-conception and social orientation that a priori logic and our past research give us reason to think might influence job recruit-

ment and retention or might be affected by job experience. Specifically, we consider authoritarian conservatism, anxiety, trust, self-confidence, self-deprecation, idea conformity, fatalism, and standards of morality. The measurement models for these concepts are summarized in the Appendix.

The findings derived from the causal models for self-conception and social orientation are presented in table 3, which shows the following.

1. All of the proximate conditions of work except heaviness affect directly one or more aspects of self-conception and social orientation. With only a few exceptions (to be discussed below), the effects of job conditions on personality are readily interpretable as a learning-generalization process—learning from the job and generalizing the lessons to off-the-job realities. In particular, occupational self-direction leads to self-directed orientations to self and society: men who are self-directed in their work are consistently more likely to become nonauthoritarian, to develop personally more responsible standards of morality, to become self-confident and not self-deprecatory, to become less fatalistic, less anxious, and less conformist in their ideas. Job pressures are much less consistent in their impact on self-conception and social orientation. Heaviness has no statistically significant effects; dirtiness is demoralizing; time pressure is in the main self-enhancing; and working longer hours tends, in the long run, to be reassuring. Certainly, one cannot conclude that job pressures, as we have measured them, are uniform in their psychological import. Extrinsic risks and rewards have mainly predictable consequences—risks are threatening, rewards are reassuring. But here we encounter the few real anomalies: higher income leads over time to less self-confidence and to greater self-deprecation; job protections are conducive to authoritarian conservatism and to distrustfulness. It is noteworthy that the anomalies involve risks and rewards attached to the job, instead of the actual conditions of work.¹¹ Overall, the results are most clear-cut for those job conditions that are most central to the work itself; in particular, they demonstrate the importance for self-conception and social orientation of occupational self-direction.

2. Ownership, bureaucratization, and position in the supervisory hier-

¹¹ We have resolved a few other minor anomalies that we think are based on inadequate identification. In particular, when the reciprocal paths between some job condition and some facet of self-conception or social orientation were both statistically significant but of opposite sign, we tested each path alone, fixing the other path at zero. The tested path was generally reduced in magnitude, usually to statistical non-significance, in which case it, too, was fixed at zero. In a few instances, one or the other path (never both) remained statistically significant, and we kept it. This procedure may have resulted in our losing some real, opposite-signed effects, but since the identification on which the model is based is not strong enough to ensure that such effects are not simply a statistical artifact, we prefer to lose them rather than claim effects that may not be real and risk distorting the rest of the model.

TABLE 3 (Continued)
B. STATISTICALLY SIGNIFICANT EFFECTS OF EACH FACET OF SELF-CONCEPTION AND SOCIAL ORIENTATION ON JOB CONDITIONS†

	Substantive Complexity	Routinization	Closeness of Supervision	Ownership†	Bureaucratization†	Position in Hierarchy†	Time Pressure	Heaviness
Authoritarian conservatism.....09(L)	-.09(L)
Personally responsible criteria of morality.....	.05(L)	...	-.09(L)
Trustfulness.....
Self-confidence.....
Self-deprecation.....18(L)	-.07(L)10(C)§
Fatalism.....09(C)	...
Anxiety.....12(C)
Idea conformity.....	-.05(L)
Authoritarian conservatism.....
Personally responsible criteria of morality.....	-.09(L)
Trustfulness.....	...	-.15(L)	-.13(C)10(C)
Self-confidence.....	-.06(L)	.10(L)
Self-deprecation.....12(L)
Fatalism.....
Anxiety.....10(C)
Idea conformity.....	-.15(C)
Dirtyness		Hours of Work	"Held Responsible"	Risk of Loss of Job	Job Protections	Job Income		

† Other job conditions, including the 1964 analog of that particular job condition, and "credentialing" background variables are controlled.

§ There is also a lagged effect of .08.

TABLE 3

RECIPROCAL EFFECTS OF JOB CONDITIONS AND EACH OF SEVERAL FACETS OF SELF-CONCEPTION AND SOCIAL ORIENTATION
A. STATISTICALLY SIGNIFICANT EFFECTS OF JOB CONDITIONS ON SELF-CONCEPTION AND SOCIAL ORIENTATION*

	Substantive Complexity	Routinization	Closeness of Supervision	Ownership†	Bureau-cratization†	Position in Hierarchy†	Time Pressure	Heaviness
Authoritarian conservatism.....	-.11(C)
Personally responsible criteria of morality.....	.27(C)
Trustfulness.....	...	-.08(C)	-.07(C)	-.05(C)	...
Self-confidence.....	-.14(L)10(C)	...
Self-deprecation.....	-.25(C)	-.07(L)
Fatalism.....	-.12(L)
Anxiety.....18(C)
Idea conformity.....10(C)	.12(C)	-.10(L)	...
Authoritarian conservatism.....
Personally responsible criteria of morality.....	-.08(L)
Trustfulness.....	-.05(C)	-.08(L)	-.09(C)	.11(C)
Self-confidence.....13(C)	-.08(L)
Self-deprecation.....	...	-.11(L)07(L)
Fatalism.....	-.09(C)
Anxiety.....	.12(C)	-.08(L)	-.13(C)
Idea conformity.....	-.10(C)
Authoritarian conservatism.....
Personally responsible criteria of morality.....
Trustfulness.....
Self-confidence.....
Self-deprecation.....
Fatalism.....
Anxiety.....
Idea conformity.....

* Other job conditions, the 1964 level of that facet of self-conception or social orientation, and all background variables are controlled.

† The effects on and of ownership, bureaucratization, and position in the supervisory hierarchy can only be lagged in these models.

archy affect the job conditions that have the most widespread effects on self-conception and social orientation—substantive complexity, closeness of supervision, job protections, and job income. Alternative models that allow position in the organizational structure to affect directly the several facets of self-conception and social orientation show that both ownership and bureaucratization would affect trustfulness negatively. Bureaucratization would have a contemporaneous, instead of a lagged, negative effect on self-deprecation. Nevertheless, the effects of proximate job conditions depicted in table 3 would be unchanged.

3. Almost every facet of self-conception and social orientation we have examined affects at least one job condition. These effects appear to be preponderantly lagged. Although there are a few contemporaneous effects of self-conception and social orientation on job conditions, we cannot be altogether sure of their meaning, because some of them may reflect, not personality affecting actual conditions of work, but personality affecting one's perceptions of those conditions. Thus, for example, seeing oneself as being held responsible for things outside one's control is a highly subjective appraisal. Similarly, although anxious people may behave in ways that result in greater pressure of time, perhaps they simply feel more time pressured. Still, some of the affected job conditions—notably, the heaviness of work, job protections, and closeness of supervision—are based on more objective indices. In any case, there is no reason to doubt the validity of the lagged effects, which pertain primarily to more objectively measured characteristics of the job, such as hours of work, the substantive complexity of work, and closeness of supervision. These lagged effects are not dramatic in magnitude, but they demonstrate that the personalities of workers sooner or later do affect their conditions of work. In particular, men who have self-directed orientations—who take personal responsibility for their own moral standards, who do not have authoritarian conservative beliefs, who are not fatalistic, who are not conformist in their ideas—are more likely over the course of time to attain responsible, self-directed positions.

TOWARD AN OVERALL MODEL OF JOB CONDITIONS AND PERSONALITY

An obvious limitation of the analysis thus far is that, while we have considered a number of job conditions simultaneously, we have dealt with ideational flexibility and the several facets of self-conception and social orientation only in separate models. A full assessment requires a model of several job conditions and several dimensions of personality. Such a model would permit us to consider such questions as whether some aspects of personality play a mediating role between job conditions and other aspects of personality (as is hypothesized about intellectual flexibility in Kohn

1980), whether job conditions continue to affect particular aspects of personality even when other aspects of personality are controlled statistically, and whether particular aspects of personality continue to affect job conditions even with other aspects of personality controlled statistically.

Attempting to deal with all eight facets of self-conception and social orientation in one causal model would not only be unduly complex but would also result in serious problems of linear dependency. Instead, we have performed a "second-order" confirmatory factor analysis, based on the hypothesis that there are two principal underlying dimensions: self-directedness versus conformity to external authority and a sense of distress versus a sense of well-being.

Self-directedness implies the beliefs that one has the personal capacity to take responsibility for one's actions and that society is so constituted as to make self-direction possible. In our earlier work (Kohn and Schooler 1969; Kohn 1969, chaps. 5 and 11), we interpreted the relationships of social class with self-conception and social orientation as reflecting the propensity of men in higher social class positions to believe that their own capacities and the nature of the world around them make self-direction seem both possible and efficacious. Men of lower social position, on the contrary, are more likely to believe that conformity to external authority is all that their own capacities and the exigencies of the world allow. Implicit in these interpretations is the hypothesis that several of the facets of self-conception and social orientation measured in this study reflect self-directedness or conformity to external authority, albeit imperfectly and in varying degrees.

The hypothesis that a sense of well-being or of distress constitutes a second principal dimension underlying the several facets of self-conception and social orientation is based in the main on our original intent, to index all major aspects of psychological functioning that might affect job placement or be affected by job conditions. We strove to measure not only a man's assessment of personal efficacy but also his feelings of comfort or pain. Self-directedness and conformity may each have distinct psychic costs and rewards.

The second-order model, presented in figure 3, confirms our expectations. Self-directedness is reflected in not having authoritarian conservative beliefs, in having personally responsible standards of morality, in being trustful of others, in not being self-deprecatory, in not being conformist in one's ideas, and in not being fatalistic—all of which is certainly in accord with our premises. Distress is reflected in anxiety, self-deprecation, lack of self-confidence, nonconformity in one's ideas, and distrust—a combination which certainly appears valid. This model, which allows over-time correlations of the residuals of the first-order concepts but deliberately does not permit any intra-time correlated residuals, fits the data

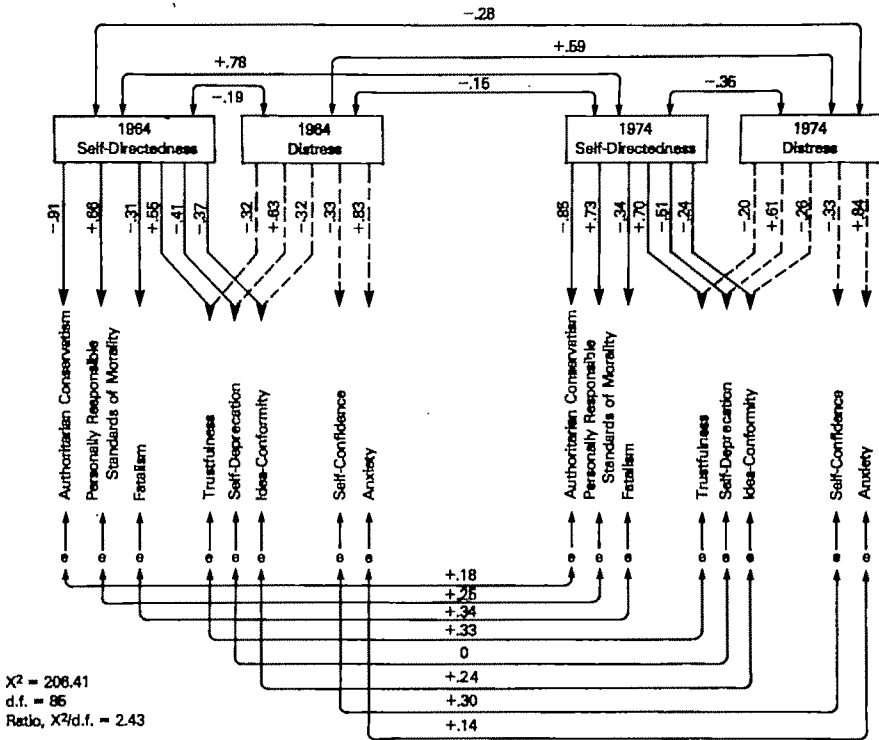


FIG. 3.—Second-order confirmatory factor analysis of self-conception and social orientation.

reasonably well: $\chi^2 = 206.41$, with 85 df, for a ratio of 2.43. Together with ideational flexibility, the model provides a partial but useful conceptualization of personality.

JOB CONDITIONS, IDEATIONAL FLEXIBILITY, SELF-DIRECTEDNESS, AND DISTRESS

Treating personality in terms of three basic dimensions—ideational flexibility, self-directedness, and distress—enables us to develop a general model of job conditions and some principal dimensions of personality.¹² In testing this model, we face a new problem of identification: how best to identify the effects of the three dimensions of personality on one an-

¹² One of the indicators of ideational flexibility is the respondent's propensity to agree with "agree-disagree" questions. Because such questions are built into the mode of inquiry used in assessing self-conception and social orientation, there is linear dependence between ideational flexibility and self-directedness. We solve the problem by using a modified measurement model of intellectual flexibility that does not include the "agree score" as an indicator. Factor scores of ideational flexibility based on the two models correlate .94.

other? We use the same procedure as for identifying the effects of job conditions on one another and on personality, namely, giving priority to contemporaneous effects and using the cross-lagged effects as instruments. As it turns out, there are two nonsignificant contemporaneous intrapsychic effects (those relating distress and ideational flexibility), and their lagged analogs are also nonsignificant (see table 4).

The causal model presented in table 4 depicts a dynamic system in which job conditions affect all three dimensions of personality, all three dimensions of personality affect job conditions, and the three dimensions of personality affect one another. More concretely: the effects of job conditions on personality are essentially the same as those we have seen in the separate analyses of the relationships of job conditions with ideational flexibility and with the eight facets of self-conception and social orientation. Thus ideational flexibility is increased by job conditions that facilitate intellectual alertness, with substantive complexity having the strongest effect of any proximate condition of work. The main job determinant of self-directedness, too, is substantive complexity. Job conditions that result sooner or later in feelings of distress are lack of job protections, dirty work, close supervision, a low position in the supervisory hierarchy, and fewer hours of work—all but the last suggesting oppressive working conditions, and the entire set typical of unskilled employment in the secondary labor market.¹⁸ These findings support a learning-generalization model: self-directed work leads to ideational flexibility and to a self-directed orientation to self and society; oppressive working conditions lead to distress.

The effects of personality on job conditions also are essentially the same as those found in the prior analyses. Ideationally more flexible men are more likely to achieve, in time, self-directed positions. A self-directed orientation results, over time, in being less closely supervised, having greater income, and doing physically lighter work, in short, in more advantageous jobs. Psychic distress results in working under greater time pressure and in a greater likelihood of being held responsible for things outside one's control. (Both of these subjective appraisals may, of course, be objectively inaccurate.) In time, distress leads also to doing physically lighter work. The only possible discrepancy between these findings and what we have seen before is that self-directedness does not have quite as wide a range of direct effects on job conditions as one might have expected. The reason becomes clear when we examine intrapsychic effects:

¹⁸ There is also an anomalous positive lagged path from 1964 job income to 1974 distress, a path opposite in sign to the zero-order correlation. This path may be artifactual; it is not statistically significant unless all the paths from current job conditions to distress present in the final model are allowed. Including or excluding this path does not affect the magnitudes of other paths in the model.

TABLE 4

RECIPROCAL EFFECTS OF JOB CONDITIONS, IDEATIONAL FLEXIBILITY,
SELF-DIRECTEDNESS/CONFORMITY, AND DISTRESS/WEEL-BEING

	STATISTICALLY SIGNIFICANT EFFECTS OF:					
	Job Conditions and Dimensions of Personality on:			Ideational Flexibility on Job Conditions		
	Ideational Flexibility	Self-directedness	Distress	Ideational Flexibility on Job Conditions	Self-directedness on Job Conditions	Distress on Job Conditions
Job conditions:						
Substantive complexity.....	.11(C)	.12(C)	.0	.26(L)	.0	.0
Routinization.....	-.03(L)	.0	.0	.0	.0	.0
Closeness of supervision.....	.0	.0	.09(C)	.0	-.13(L)	.0
Ownership.....	.0	.0	.0	.06(L)	.0	.0
Bureaucratization.....	.0	.0	.0	.07(L)	.0	.0
Position in hierarchy.....	.0	.0	-.10(L)	.0	.0	.0
Time pressure.....	.05(C)	.0	.0	.0	.0	.11(C)
Heaviness.....	-.07(C)	.0	.0	.0	-.11(L)	-.07(L)
Dirtyness.....	.0	.0	.10(C)	.0	.0	.0
Hours of work.....	.0	.0	-.08(C)	-.22(L)	.0	.0
"Held responsible".....	-.04(C)	-.06(C)	.0	.0	.0	.11(C)
Risk of loss of job or business.....	.0	-.04(L)	.0	.0	.0	.0
Job protections.....	-.08(L)	-.05(C)	-.16(C)	-.14(L)	.0	.0
Job income.....	.07(C)	.0	.11(L)	.0	.08(L)	.0
Dimensions of personality:						
Ideational flexibility.....	.57(L)	.13(C)	.0
Self-directedness.....	.24(C)	.43(L)	-.25(C)
Distress.....	.0	-.08(C)	.54(L)

NOTE.—Pertinent background variables are controlled. C = a contemporaneous effect; L = a lagged effect; 0 means a nonsignificant effect that has subsequently been fixed at zero.

self-directedness affects some job conditions, not directly, but indirectly through ideational flexibility.

The intrapsychic effects, the heretofore-missing component of the job-personality system, are impressive. Ideational flexibility both positively affects and is positively affected by self-directedness. Self-directedness both negatively affects and is negatively affected by distress. Noteworthy among these intrapsychic effects are the strong effects of self-directedness on both ideational flexibility and distress. Self-directedness affects both ideational flexibility and distress decidedly more strongly than they affect self-directedness or than they affect each other. If one of the three dimensions of personality is pivotal, it is self-directedness.¹⁴

DISCUSSION

Before summarizing the findings of these analyses and discussing their theoretical and practical import, it is necessary to point out the principal limitations of our data and of our methods of analysis.

The most important limitation of the analysis is one that we did not fully comprehend before actually doing longitudinal analysis—namely, that we have measurements at only two times and that there is a long interval between them. Both aspects pose serious problems. Identifying the models would be simpler and more certain with measurements at three times, for three measurements would provide instruments for assessing contemporaneous and lagged effects simultaneously. The 10-year time interval poses related yet different problems. Even in determining the effects of personality on job conditions, a process in which we could test contemporaneous and lagged effects simultaneously, we had to hedge the meaning of “contemporaneous” and “lagged.” To appraise the actual timing of effects, we should ideally have measurements taken at frequent intervals (for this purpose, three is probably not enough). Only then would we be able sharply to differentiate truly contemporaneous from truly lagged effects.

Another limitation of our analysis is the inadequacy, particularly the subjectivity, of several of our indices of job conditions. We are fearful, for example, that we may have given short shrift to routinization, which

¹⁴ We must ask again, What would be the consequence of allowing ownership, hierarchical position, and bureaucratization to affect directly the three dimensions of personality? Their effects on ideational flexibility would be as shown earlier. None of them would significantly affect self-directedness. Bureaucratization would increase distress and hierarchical position would decrease it, neither to any great extent; and the direct effect of bureaucratization would be opposite to its indirect effect through job protections. Most important, the effects of other job conditions on personality would not be greatly affected, the principal difference (as before) being a diminished, but still substantial, effect of substantive complexity on ideational flexibility.

is measured by a single, rather subjective indicator, in contrast to the substantive complexity of work, the best indexed of the job conditions we have studied. Clearly, multiple-indicator measures of job conditions are needed. As for the issue of subjectivity, the solution is not readily apparent. What is the best way to measure time pressure? Should the ideal index of time pressure include a subjective component or should it be, insofar as possible, a measure solely of the external requirements of the job?

There are other limitations, some of them discussed at sufficient length in our earlier papers (in particular, Kohn and Schooler 1973, 1978) that they need only be enumerated here: we have not been able to measure the organizational, technological, and interpersonal contexts of work as well as we have the actual conditions of work; we have done no systematic analysis of career patterns; we do not know whether the effects we have found are essentially the same for all age cohorts and for all segments of the work force; and we have not taken into account other important events that may have occurred in the lives of these men during the 10-year interval between the baseline and the follow-up interviews. A limitation emphasized in earlier papers no longer applies: our colleagues and we now have done an analysis of women's job conditions and psychological functioning (Miller et al. 1979), with findings strikingly consistent with those for men. There is a final issue that seems to us even more important than we had earlier recognized it to be—the desirability of validating our interview-based methods and findings with observation-based studies.

These limitations notwithstanding, this analysis does take us considerably beyond our original approach (Kohn and Schooler 1969; Kohn 1969), which allowed us only to assume that class-associated conditions of life affect the psychological functioning of individuals. We now have strong evidence that job conditions actually do affect personality, and also that personality affects job conditions. Moreover, these reciprocal processes are embedded in an intricate and complex web in which job conditions also affect each other and some aspects of personality affect others.

Our longitudinal analysis repeatedly demonstrates the importance for personality of occupational self-direction—especially the substantive complexity of work, the job condition most strongly related to social class in our earlier analyses. Jobs that facilitate occupational self-direction increase men's ideational flexibility and promote a self-directed orientation to self and to society; jobs that limit occupational self-direction decrease men's ideational flexibility and promote a conformist orientation to self and to society. The analysis further demonstrates that opportunities for exercising occupational self-direction—especially for doing substantively complex

work—are to a substantial extent determined by the job's location in the organizational structure, with ownership, bureaucratization, and a high position in the supervisory hierarchy all facilitating the exercise of occupational self-direction. These findings provide strong empirical support for the interpretation that class-associated conditions of work actually do affect personality. The longitudinal analysis also provides evidence of other job-to-personality effects, the most important being that oppressive working conditions produce a sense of distress. Implicit in all these findings is the consistent implication that the principal process by which a job affects personality is one of straightforward generalization from the lessons of the job to life off the job, instead of such less direct processes as compensation and reaction formation.

The longitudinal analysis demonstrates also that, over time, personality has important consequences for the individual's place in the job structure. Both ideational flexibility and a self-directed orientation lead, in time, to more responsible jobs that allow greater latitude for occupational self-direction. Feelings of distress lead to actual or perceived time pressure and uncertainty. We think it noteworthy that so many of the personality-to-job effects—particularly the effects of personality on the most objectively measured conditions of work—are lagged rather than contemporaneous. The implication is that job conditions are not readily modified to suit the needs or capacities of the individual worker. Over a long enough time, though, many men either modify their jobs or move to other jobs more consonant with their personalities. Thus the long-term effects of personality on job conditions are considerable. The process of job affecting man and man affecting job is truly reciprocal throughout adult life.

Finally, this analysis depicts a set of intrapsychic effects which contribute to the dynamic impact of the entire system. Ideational flexibility increases and is increased by a self-directed orientation; a self-directed orientation decreases and is decreased by distress. Self-directedness has particularly strong effects on the other two dimensions of personality.

However complex the system and however diverse the effects, the findings highlight the centrality for job and personality of a mutually reinforcing triumvirate—ideational flexibility, a self-directed orientation to self and society, and occupational self-direction. Ideational flexibility is both responsive to and productive of occupational self-direction. A self-directed orientation increases ideational flexibility and decreases a sense of distress. Occupational self-direction—especially substantive complexity, the keystone of the job structure—decidedly affects both ideational flexibility and a self-directed orientation.

The interrelationship of ideational flexibility, a self-directed orientation, and occupational self-direction is integral to the stratification system of

the society.¹⁵ Occupational self-direction is substantially determined by such class-linked aspects of organizational position as ownership and hierarchical level; ideational flexibility and a self-directed orientation, in turn, affect the likelihood of an individual's achieving a highly placed organizational and social position. In short, occupational self-direction, ideational flexibility, and a self-directed orientation are intertwined in a dynamic process through which the individual's place in the stratification system both affects and is affected by his personality.

¹⁵ All three are highly correlated with social stratification position. Using a confirmatory factor-analytic model similar to those employed in this paper (see Kohn and Schoenbach 1980), we find social stratification position to be correlated .85 with ideational flexibility, .70 with self-directedness/conformity, and .90 with occupational self-direction.

APPENDIX

MEASUREMENT MODELS OF SELF-CONCEPTION AND SOCIAL ORIENTATION

CONCEPT/INDICATORS	STANDARDIZED PATH FROM CONCEPT TO INDICATOR	
	1964	1974
Authoritarian conservatism ($\chi^2=120.98$, $df=111$, ratio=1.09):		
The most important thing to teach children is absolute obedience to their parents.....	.61	.68
Young people should not be allowed to read books that are likely to confuse them.....	.50	.46
There are two kinds of people in the world: the weak and the strong.....	.57	.62
People who question the old and accepted ways of doing things usually just end up causing trouble.....	.51	.47
In this complicated world, the only way to know what to do is to rely on leaders and experts.....	.58	.54
No decent man can respect a woman who has had sex relations before marriage.....	.43	.51
Prison is too good for sex criminals; they should be publicly whipped or worse.....	.46	.50
Any good leader should be strict with people under him in order to gain their respect.....	.41	.54
It's wrong to do things differently from the way our forefathers did.....	.45	.52
Personally responsible criteria of morality ($\chi^2=12.92$, $df=15$, ratio=.86):		
It's all right to do anything you want as long as you stay out of trouble.....	-.59	-.64
If something works, it doesn't matter whether it's right or wrong.....	-.44	-.30
It's all right to get around the law as long as you don't actually break it.....	-.58	-.61
Do you believe that it's all right to do whatever the law allows, or are there some things that are wrong even if they are legal?.....	-.33	-.26
Trustfulness ($\chi^2=5.29$, $df=5$, ratio=1.06):		
Do you think that most people can be trusted?.....	.51	.53
If you don't watch out, people will take advantage of you... Human nature is really cooperative.....	-.52	-.61
	.20	.11
Self-esteem two-factor model ($\chi^2=166.77$, $df=112$, ratio=1.49):		
Self-confidence:		
I take a positive attitude toward myself.....	.60	.71
I feel that I'm a person of worth, at least on an equal plane with others.....	.47	.45
I am able to do most things as well as other people can... I generally have confidence that when I make plans I will be able to carry them out.....	.43	.34
	.54	.45
Self-deprecation:		
I wish I could have more respect for myself.....	.76	.71
At times I think I am no good at all.....	.49	.51
I feel useless at times.....	.43	.40
I wish I could be as happy as others seem to be.....	.58	.59
There are very few things about which I'm absolutely certain.....	.29	.43
(Correlation: self-confidence/self-deprecation).....	(-.29)	(-.28)
Fatalism ($\chi^2=2.15$, $df=5$, ratio=.43):		
When things go wrong for you, how often would you say it is your own fault?.....	-.65	-.57
To what extent would you say you are to blame for the problems you have—would you say that you are mostly to blame, partly to blame, or hardly at all to blame?.....	-.59	-.70
Do you feel that most of the things that happen to you are the result of your own decisions or of things over which you have no control?.....	-.37	-.38

APPENDIX (Continued)

CONCEPT/INDICATORS	STANDARDIZED PATH FROM CONCEPT TO INDICATOR	
	1964	1974
Anxiety ($\chi^2=213.94$, $df=159$, $ratio=1.35$):		
How often do you feel that you are about to go to pieces?...	.55	.58
How often do you feel downcast and dejected?.....	.68	.68
How frequently do you find yourself anxious and worrying about something?.....	.49	.47
How often do you feel uneasy about something without know- ing why?.....	.45	.50
How often do you feel so restless that you cannot sit still?...	.47	.39
How often do you find that you can't get rid of some thought or idea that keeps running through your mind?.....	.41	.44
How often do you feel bored with everything?.....	.58	.57
How often do you feel powerless to get what you want out of life?.....	.54	.52
How often do you feel guilty for having done something wrong?	.36	.36
How often do you feel that the world just isn't very under- standable?.....	.48	.44
How often do you feel that there isn't much purpose to being alive?.....	.45	.37
Idea conformity ($\chi^2=15.43$, $df=13$, $ratio=1.19$):		
According to your general impression, how often do your ideas and opinions about important matters differ from those of your relatives?.....	-.69	-.64
How often do your ideas and opinions differ from those of your friends?.....	-.63	-.51
How about from those of other people with your religious background?.....	-.50	-.44
Those of most people in the country?.....	-.68	-.35

NOTE.—(1) A high score on the indicator generally implies agreement or frequent occurrence; where alternatives are posed, the first alternative is scored high. (2) In all models, the error (or residual) of each 1964 indicator is allowed to correlate with the error of the same indicator in 1974. In several models, some intra-time error correlations are also allowed. (Error correlations are not shown in the table.) (3) The correlations of the concepts over time (1964-74) are: authoritarian conservatism, .78; criteria of morality, .65; trustfulness, .81; self-confidence, .52; self-deprecation, .55; fatalism, .61; anxiety, .53; and idea conformity, .39.

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Occupational Recruitment, Retention, and Labor Force Cohort Representation¹

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Occupations differ in recruiting and retaining labor forces from different cohorts of workers. The measure *cohort representation* is constructed here to summarize outcomes of the different processes. The paper proposes an occupational competition model consisting of work qualifications, opportunities, rewards, and shelters to account for cohort representation in occupations. United States census data are used to follow four male and four female cohorts in 63 occupations over 20-year periods from 1920–40 through 1950–70. The model predicts recruitment from young cohorts better than retention or recruitment from aging cohorts. High work qualifications and wide opportunities favored recruitment. This pattern developed progressively from each cohort to the next for men but appeared only in 1950–70 for women. Contrary to expectations, shelters and high rewards generally did not favor retention. Occupational characteristics rooted directly in the division of labor (qualifications and opportunities) structured the male and female labor markets in similar ways. Characteristics arising from the organization of labor markets within an existing division of labor (shelters and rewards) did not. Male labor markets were relatively structured; female labor markets were more amorphous.

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The literature has viewed the labor market chiefly as a system of allocating job opportunities to workers. For example, dual labor market analysts have shown how ascribed characteristics consign workers to secondary markets (Harrison 1972; Bonacich 1976). Other writers have identified industry segments across which there is little movement (Reynolds 1951; Averitt 1968). Still others have shown how employers and unions create exclusive internal labor markets (Kerr 1954; Doeringer and Piore 1971). Less attention has been given to occupations as active components of labor markets, especially to the processes through which occupations maintain their labor forces. Our study focuses on this aspect of labor markets.

Occupations compete for labor forces. They develop distinctive labor force maintenance patterns involving recruitment and retention of workers from different cohorts. An occupation may recruit young workers and retain them throughout their work lives. It may recruit young workers, lose them or push them out, and replace them with other young workers in a pattern of intercohort succession. It may recruit older workers, maintaining its labor force through intracohort movement out of other occupations.

Although recruitment can occur at any stage of a cohort's work life, it tends to center chiefly on workers who are newly entering the labor force. This is increasingly true as more and more occupations require specialized training. Such training is normally acquired when workers are young, and it may become obsolete rather quickly. For this reason, much competition among occupations is concerned with attracting newly trained young workers. In occupations that do not need recently trained workers or cannot get them, the main competition for labor forces involves retention of workers as they age or recruitment of older workers.

These differing ways of maintaining labor forces create distinctive patterns of *cohort representation*: the relative extent to which an occupation's labor force is from a given cohort at successive times. If an occupation recruits workers from a cohort when it is young and replaces them later with young workers from subsequent cohorts, the representation of the cohort in the occupation declines as it ages. An occupation that retains or recruits more workers from an aging cohort than it recruits from younger cohorts exhibits growth of representation of a cohort as it ages. An occupation that succeeds in recruiting from a young cohort entering the labor force, and keeps its share of the cohort as it ages, exhibits stability of cohort representation.

Occupational characteristics that help in recruiting labor forces may not be the same as those that help in retaining them. Some characteristics are rooted directly in the division of labor. They include work qualifications, which arise from the specialization of work tasks, and opportunities, which reflect the growth of work functions and their dispersion across industries and communities. They determine the parameters within which labor mar-

kets are created and operated. We would expect occupational competition for labor forces on the basis of characteristics inhering in the division of labor to be directed primarily at assuring inflows of specialized workers. Hence, we expect work qualifications and opportunities to influence chiefly the recruitment of young workers. We designate these characteristics as *recruitment factors*.

Other characteristics of occupations arise from the organization of labor markets, within a given division of labor, by employers, occupational associations, unions, and providers of professional services, and from the interactions of these persons and groups within the markets. They include rewards and shelters. These characteristics are generally subject to more influence by purposive actions of the groups involved than are characteristics inhering in the division of labor. We expect rewards and shelters to influence chiefly the retention of labor forces from aging cohorts, and we designate them as *retention factors*. Some occupational characteristics may, of course, serve either to recruit or to retain labor forces. Our analysis will clarify the relative contributions of occupational characteristics to recruitment and retention.

Work qualifications such as educational credentials specialize an occupation's labor force and fit it into the division of labor (Temme 1975, pp. 140-51; Spaeth 1979). Specialized qualifications favor recruitment from young cohorts because the educational system, where qualifications are acquired, is geared to the young (Ryder 1968; Lane 1975). For the same reason, specialized qualifications restrict intracohort movement into an occupation during the later work years. They may or may not limit movement out of an occupation, but high qualifications make it unlikely that an occupation will be a dumping ground for workers pushed out of other occupations. Growing and widely accessible opportunities also are integral parts of the division of labor and should help occupations recruit young workers. Growth and decline in the sizes of occupations take place chiefly through changes in rates of recruitment from young cohorts (Clark 1902, p. 279; Bogue 1957, pp. 501-2; Duncan 1966, pp. 7, 15). Once workers are in an occupation, its rewards can retain them. Rewards act mainly on retention. The filtering of recruits by qualifications and the limited availability of opportunities for workers with the required qualifications narrow the pools of potential recruits and severely limit the direct effects of rewards on recruitment (Cairnes 1874, pp. 67-68). Rewards may, of course, affect recruitment indirectly by providing incentives to obtain qualifications (Davis and Moore 1945). Shelters (Freedman 1976) such as licensure and unionization are intended to protect workers who are already in an occupation, sometimes by excluding or discouraging new entrants. The effect of shelters should be to retain aging cohorts.

In this paper, we study effects of work qualifications, opportunities,

rewards, and shelters on cohort representation in 63 occupations. To see whether the ways in which occupations compete to maintain labor forces are institutionalized and persist over time, we trace four cohorts over 20-year periods from 1920-40 through 1950-70.

SEX-DIFFERENTIATED LABOR MARKETS

Men and women constitute separate labor forces (Oppenheimer 1970; Wolf and Rosenfeld 1978). Traditionally, women have had insecure, low-paying, dead-end jobs. Such jobs make up the secondary labor market. Occupations in this market can recruit workers when there is a demand for them but lack the rewards and shelters to retain stable labor forces over a cohort's work life. For female cohorts of workers, entry into the labor force has not always been concentrated at the times when the cohorts were young. The employment of women has been traditionally intermittent because they have entered, left, and reentered the labor force in response to changing pushes and pulls of their family roles. These conditions were more prevalent during the period we will analyze than they are today. Since work qualifications, opportunities, rewards, and shelters differ between the primary and secondary labor markets and women are overrepresented in the secondary market, the influence of these factors on the recruitment and retention of male and female labor forces may differ. Therefore we study the sexes separately.

RECRUITMENT AND RETENTION FACTORS

Occupational characteristics affecting recruitment and retention of labor forces are variables operating in the labor market over the work lives of cohorts. As mentioned above, we expect work qualifications and opportunities to influence chiefly recruitment, and rewards and shelters to influence primarily retention.

Work *qualifications* affecting an occupation's recruitment are the skill it demands, the educational attainment of its labor force, and its physical strength requirements. Occupations with high educational or formally acquired skill requirements recruit young workers because older ones are less educated. Occupations with low needs for education or skill can select workers of any age. Occupations calling for great strength, such as lumbermen, recruit mainly young workers, while those that recruit many older ones—for example, watchmen—generally demand little strength.

The *opportunity* structure of an occupation includes its industrial dispersion, product or service market, and growth. Broad dispersion across industries provides entry opportunities for any age group. Examples of in-

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dustrially dispersed occupations are secretaries, janitors, and accountants. These occupations are less subject to restricted entry due to economic fluctuations or narrow geographic concentration than are occupations highly concentrated in a few industries. The market for an occupation's products or services affects the availability of its opportunities. Occupations with local markets, such as barbering, offer more geographically dispersed opportunities than those in manufacturing or commerce.

Growing occupations create new opportunities. Generally they do not draw their labor forces indiscriminately from all ages; they recruit mainly young workers (Bogue 1957, pp. 501-2). Declining occupations may retain many workers as they age but recruit few young workers (Clark 1902, p. 279; Duncan 1966, pp. 7, 15).

Two occupational attributes that affect chiefly labor force retention pertain to *rewards*. Occupations with earnings above the labor force median retain more workers than those with lower earnings. Occupations in which earnings rise with age are better able to keep workers than those with declining earnings curves.

Two other retention variables, licensure and unionization, are types of protective *shelters* (Freedman 1976). Licensure may retain workers by conferring advantages on those who stay in an occupation (Friedman and Kuznets 1945; Shimberg, Esser, and Kruger 1973). Unionization provides a shelter and also gives seniority rights, nonportable pensions, and other benefits that can retain work forces.

MEASURES OF RECRUITMENT AND RETENTION FACTORS

Occupational skill is defined by the nine-category scale of Specific Vocational Preparation (SVP) given in the *Dictionary of Occupational Titles* (U.S. Department of Labor 1968). The same source provides a five-category scale of required physical strength. We used the 1968 edition for all occupations for all cohorts. Skill and strength requirements of many occupations changed during the years covered by our study, but the changes had little effect on the skill or strength rankings of occupations in our sample. For instance, a typical physician was a general practitioner in 1920 and a specialist in 1970, but physicians ranked very high in skill and low in strength at both times. The occupations whose skill and strength changed most were those that underwent marked technological change. They are underrepresented in our sample of occupations because extreme technological change generally led to noncomparability of occupational titles across census years and we could not include them.

Educational level, computed separately for men and women, is a mean of three ratios. For each of the three census years in a 20-year period, we computed the ratio of the median education of women (or men) in an occu-

pation to that year's total labor-force median education of that sex. When the census did not report a median for an occupation, we calculated it if the necessary data were given. Then we averaged the three years' ratios to construct our measure of educational level. The 1940 and earlier censuses did not give the needed information, so we use the education variable only for the cohort we trace from 1950 to 1970. (Education data are from U.S. Bureau of the Census 1953, 1963*a*, 1973*a*.)

We use Abbott's (1975) threefold classification of the product or service markets of occupations: manufacturing; commerce, consisting of transportation, mercantile and financial activities, and clerical services; and locally oriented activities such as personal services. In the analysis, we combine manufacturing and commercial markets because they tend to be geographically concentrated, whereas local markets offer more widespread recruitment opportunities.

Data for the other two opportunity variables are derived from the census. To measure industrial dispersion of occupations, we used Gibbs and Martin's (1962) industry diversification formula, separately for males and females, for each census year of a cohort period and computed the mean of the three decennial measures to get a mean industrial dispersion figure. Censuses before 1950 did not give the information needed for the measure, so we have it only for 1950-70 (U.S. Bureau of the Census 1954, 1963*b*, 1973*b*). Sex-specific growth—occupational growth calculated separately for males and females—is the ratio of an occupation's percentage of a sex's total U.S. labor force in the last year of a 20-year period to its percentage in the first year.

The earnings measure is the ratio of median earnings of women or men in an occupation to the median earnings of the total labor force of that sex, averaged for the three times covered by a cohort (U.S. Bureau of the Census 1956*a*, 1963*a*, 1973*a*). When the censuses did not report medians, we computed them if enough data were given. Censuses before 1950 did not give earnings or income data by occupation and sex; and the 1950 data pertain to income, not earnings. We have the measure only for 1950-70.

Our earnings curve uses only 1970 data because the 1950 income data are not broken down by age; lacking information on all three years, we decided to use only 1970. The measure is the percentage difference between the median earnings of the age categories 45-54 and 35-44, with a positive figure indicating higher earnings in the older category. We use only these two categories because earnings rise until age 35 and fall after age 54 in nearly all occupations. Much of the analysis omits this variable because the necessary data are not provided for 23 of our sample occupations for men and 46 for women.

Our measures of licensure and unionization are the same for both sexes with the exceptions explained in footnote 2 below. Licensure is defined as

laws forbidding the exercise of an occupation without a state license obtained by passing an examination or earning specified educational credentials. Licensure data are from Angel's (1970) book, cross-checked for agreement with a publication of the Council of State Governments (1968). Our measure is the dichotomy of licensure in 46 or more versus six or fewer states. Only one occupation fell between these extremes, being licensed in 21 states; we classified it unlicensed. We could find no reliable information on licensure prior to 1952 so we use this variable only for 1950-70.

Data on unionization by occupation are incomplete and scattered. By applying assumptions and adjustment rules to information from a variety of sources, we arrived at reasonable estimates for all four cohorts.² The measure of unionization is a moving average of the percentages of an occupation's workers who belonged to unions at the beginning, the middle, and the end of each period.

COHORT REPRESENTATION INDEX

Our index of cohort representation, the extent to which representation of a cohort in an occupation grows or shrinks as the cohort ages, is developed using census data. The index compares a cohort's representation when its members are 45-64 years old with its representation when they were 25-44 years old. We chose these 20-year age intervals because they fit the available census data and are theoretically meaningful. The 25-44 interval takes a cohort from early in its work life after the settling down "trial" period (Miller and Form 1964, pp. 539-73) to midcareer. The 45-64 interval takes it from midcareer to retirement or close to it. We can obtain 20-year cohort boundaries, but not smaller ones, from decennial censuses since 1920.

² Sources were Bortz 1939; Labor Research Association 1938, pp. 137-41, 1972, pp. 129-32; Troy 1965; U.S. Bureau of the Census 1924, 1935, 1942, 1949, 1961, 1976; U.S. Department of Labor 1972. Assumptions and decision rules were: (1) We assumed that equal percentages of women and men in an occupation were unionized with the single exception of barbers, hairdressers, manicurists, and cosmetologists. For this category we assumed that all "barbers" union members were male and that no female belonged to a union. (We also assigned the barbers' SVP to males and the cosmetologists' slightly higher SVP to females in this category.) (2) We excluded company unions. (3) If a craft union included more than one craft, we assumed that the proportions of crafts were the same in the union as in the total labor force. (4) If members of a blue-collar occupation were employed predominantly in settings where there was an industrial union, we assumed that the occupation had the same percentage of the union's members as of the industry's blue-collar workers. (5) When we had figures for only two of the three times in a period, we estimated the third by linear interpolation or extrapolation. This was necessary for only two occupations. (6) If we could not make satisfactory estimates, we excluded occupations from analysis that used the union variable. Female unionization estimates were missing for one occupation in 1920-40, three in 1930-50, four in 1940-60, and five in 1950-70. Male unionization estimates were missing for one occupation in 1920-40 and five in each other period.

(The 1920 census does not give data on occupations for 10-year age categories.) We extract four cohorts by following birth cohorts through age categories over census years: 1920-40, 1930-50, 1940-60, 1950-70.³

A rough measure of the cohort representation concept is the ratio UCRI (unstandardized cohort representation index), defined as

$$\left(\frac{m_{11}}{m_{12}} \right) / \left(\frac{m_{21}}{m_{22}} \right),$$

where m_{11} is the number of male or female workers in the cohort of interest in an occupation in a given year, m_{12} is the number of workers of that sex in all other cohorts in the occupation in the given year, m_{21} is the number of workers of that sex in the cohort of interest in the occupation in an earlier year (in our example, 20 years earlier), and m_{22} is the number of workers of that sex in all other cohorts in the occupation in the earlier year. In figure 1 we display the terms of this ratio as the cells of a 2×2 contingency table whose dimensions are year (year x and 20 years earlier) and cohort (cohort y and all other cohorts).

Unfortunately, this index could be confounded by growth or decline of the number of workers in the total labor force, growth or decline of the specific occupation's labor force, cohort attrition due to mortality, cohort growth through immigration, and cohort growth or attrition due to changing labor force participation rates during a cohort's work life. For example, the thinning of a cohort through deaths as it goes from ages 25-44 to 45-64 would produce ratios showing declines in cohort representation, other things being equal, while the greater increase in labor force participation among

		COHORT	
		Y	All Other Cohorts
YEAR	X	m_{11}	m_{12}
	X-20	m_{21}	m_{22}

FIG. 1.—Terms used in developing unstandardized cohort representation index

³ Unfortunately, our cohorts are not mutually exclusive. In the three pairs that are 10 years apart, the successive ones included some of the same workers. In the two pairs that are 20 years apart, workers who were 25-44 in either year of the earlier time were 45-64 in the corresponding year of the later time; no worker was in both cohorts in these pairs, but some were in both total labor forces used to construct cohort representation indexes (CRIs). Some overlap also affected the 1920-40 and 1950-70 indexes. The overlap of cohorts does not seriously damage our analysis because our main interest is in observing each cohort separately to ascertain patterns for occupations, not for cohorts.

older women than among younger women would produce ratios showing growth of female cohort representation. We want to examine cohort representation as a reflection of occupational characteristics, not as an artifact of population processes or general rates of labor force participation. We need a standardization technique to remove possible confounding influences.

We standardize the index by relating it to a measure which takes account of these influences. The average growth or decline of cohort representation in occupations in the total labor force serves as a standard for assessing change in a cohort's representation in a particular occupation. We compute this average, XCRI (average cohort representation index) by taking the geometric mean of the UCRI's of the occupations in our sample (see n. 4 below) plus a "residual" occupation which consists of the labor force not included in our sample. With the residual category added, the index accounts for the total labor force. We define XCRI as follows:

$$e^s \text{ where } s = \left[\sum_i \ln (\text{UCRI}) \right] / N,$$

$1 \leq i \leq N$, where N is the number of occupations in the sample plus the residual category. We divide each UCRI by XCRI to get a cohort representation index for each occupation. This index measures change in the representation of a cohort in the occupation relative to change in the representation of the cohort in all occupations. An occupation with below-average growth in representation of a cohort, or above-average decline, will have an index less than 1; an occupation with above-average growth or below-average decline in representation of a cohort will have an index greater than 1. We rescale the index to make it negative for occupations showing shrinkage of cohort representation and positive for occupations showing growth. We do this by taking one-fourth of the logged ratio. Our new standardized index, which we designate CRI and use in our analysis, is

$$\frac{1}{4} \ln (\text{UCRI}/\text{XCRI}).$$

Readers familiar with Goodman's (1970, 1972) log-linear technique may recognize that our CRI is identical to the three-way interaction effect parameter produced by fitting a saturated model to the data formed by tabulating cohort \times year \times occupation. (Cohort and year are defined as in fig. 1. Occupation consists of the occupations in our sample for a given cohort plus the residual category.) The properties of the log-linear model assure that the measure eliminates confounding by demographic or labor force processes. The three-way effect adjusts for all lower-order effects in the model. It is not confounded by growth of the total labor force (the year effect), growth or decline of an occupation's labor force (the occupation \times year effect), or cohort attrition or growth due to demographic processes or changing labor force participation rates (the cohort \times year effect).

Although the definition of CRI is based on log-linear analysis techniques, and we used Fay and Goodman's (1973) log-linear analysis program *ECTA* to calculate the CRIs and all the lower-order effects, we emphasize that *ECTA* was used only for computational convenience. The CRI index is intuitively and mathematically satisfying quite apart from its relation to log-linear analysis and can be calculated directly from the data table according to the formula given above. We used *ECTA* simply as a tool for obtaining the CRIs. We do not apply any of the model-fitting or hypothesis-testing capabilities of the technique but use it in a purely descriptive way: it summarizes the observed census counts to allow occupation-specific frequencies in the three-way contingency table to be decomposed into separate effects due to year, cohort, and their interactions.

In our entire data set, CRIs range from $-.4749$ to $+.6705$, with a positive value indicating relative growth of a cohort's representation in an occupation over a 20-year period, a negative value indicating relative shrinkage, and a coefficient of 0.0 indicating perfect relative stability of representation. All measures are net of changes in size of the occupation's labor force (the occupation \times year effect), the total labor force (the year effect), and the total labor force within the cohort (the cohort effect).

To help in understanding the interpretation of CRIs, table 1 shows all the effects pertaining to four occupations from the analysis of the female cohort that we traced from 1920 to 1940. It shows shrinkage of the cohort's representation among nurses (CRI = $-.2294$), less shrinkage among teachers ($-.0808$) and telephone operators ($-.0420$), and growth of representation among farmers ($+.4295$). Looking further at nurses and farmers, we see that cohort representation for nurses shrank substantially while the occupation was growing at about the same rate as the total labor force (the occupation \times year effect) and that cohort representation for farmers grew sharply while the occupation declined. (Substantively, this is what we would expect. Growing occupations generally recruit many young

TABLE 1
LOGGED EFFECTS FOR FOUR OCCUPATIONS FOR THE 1920-40 FEMALE COHORT

Effect	Nurses	School- teachers	Telephone Operators	Farmers
Grand mean.....	8.1756	8.1756	8.1756	8.1756
Cohort.....	-.3032	-.3032	-.3032	-.3032
Year.....	.1789	.1789	.1789	.1789
Occupation.....	3.2119	4.4965	2.9926	3.2781
Cohort \times year.....	-.2153	-.2153	-.2153	-.2153
Cohort \times occupation.....	-.1134	-.0602	-.4437	.1626
Year \times occupation.....	.0309	-.1703	-.2931	-.4135
Occupation \times cohort \times year (CRI).....	-.2294	-.0808	-.0420	.4295

Occupational Recruitment and Retention

workers. Declining occupations maintain labor forces chiefly by retaining workers from aging cohorts.)

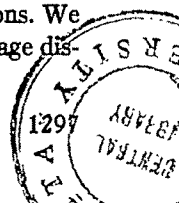
SAMPLE OF OCCUPATIONS AND COHORTS

Data on occupations are from decennial censuses (U.S. Bureau of the Census 1923, 1933; 1943, 1956*a*, 1963*a*, 1973*a*). The census definition of the work force changed in 1940. The 1920 and 1930 censuses counted "gainful workers," who included "all persons who *usually* worked at gainful labor, regardless of *when* they worked," but the 1940 "labor force" consisted of persons "who were working, or with a job, or seeking work" in the last week of March (Edwards 1943, p. 7). The 1940 census also raised the lower age limit for inclusion from 10 to 14 years. Both changes led the 1940 census to identify fewer persons as workers than the earlier definition would have identified, but the difference is unlikely to have been great enough to affect our findings materially. Censuses after 1940 kept the 1940 labor force definition.

Occupational titles were not always comparable across censuses. Changed definitions of occupations were most numerous in 1940 and 1970. We used comparability estimates by census officials as criteria in selecting occupations. Edwards (1943, pp. 35-48) gives comparability indexes for 1930 and 1940, and Priebe et al. (U.S. Bureau of the Census 1972, pp. 20-59) show comparability between 1960 and 1970. For an occupation to be included in our sample, a change in its definition could not misclassify more than 10% of the 1930 or 1960 enumeration using the 1940 or 1970 definition, respectively. The 1920 and 1930 censuses used essentially the same classifications, as did those of 1940 and 1950 with few exceptions (U.S. Bureau of the Census 1956*b*). Some refinements were made in 1960. If they led to non-comparable enumerations (U.S. Bureau of the Census 1968), we excluded the affected occupations from the sample.

Some occupations were defined similarly from one census to another but were inconsistently listed. Occupations reported separately in some censuses were combined into broader categories in others. We aggregated the separate listings to correspond to the inclusive ones, with one set of exceptions. The 1940 census used numerous combined categories composed of occupations listed separately in all other censuses from 1920 through 1970. In these instances we disaggregated the 1940 categories, for each age group, by averaging the percentages of the combined categories that the separate occupations constituted in 1930 and in 1950. We made 15 interpolations of this kind for men and nine for women.

The 1940 census did not give age distributions for some occupations. We estimated them by linear interpolation based on the 1930 and 1950 age dis-



tributions. We estimated the 1940 age distributions of 32 occupations for females and nine for males.

These limitations resulted in a sample of 63 occupations to follow through the six decennial censuses.⁴ It is not a random sample of all occupations or of any known universe but a convenience sample composed of all occupations for which we could obtain the necessary data. Our use of significance tests in interpreting the findings is for heuristic purposes only. The data on the 63 occupations are complete only for males. Female labor forces in some occupations were so small that we did not think reliable interpolations could be made. Also excluded from analysis of some cohorts are fully reported female labor forces with fewer than 50 workers. (It is noteworthy that in selecting the sample we found no all-female occupations but several all-male occupations.) Female labor forces left out of the sample because of missing data or small numbers include 21 for 1920, 23 for 1930, and 11 for 1940.

Noncomparable designation of occupations in different censuses, especially the 1920 and 1930 versus later ones, and the impossibility of using new occupations prevented accurate representation of the major Edwards (1943) census categories. The sample underrepresents managers, officials, and proprietors; sales workers; operatives and kindred workers; and laborers. It overrepresents professionals and farmers. It represents craft, clerical, and service workers fairly adequately. Our 63 occupations included about a third of the United States male labor force in 1920; this figure fell to less than a fifth by 1970, owing mainly to the decline in farming (table 2). The occupations included somewhat more than a fourth of the total female labor force in every year, with no consistent pattern of increase or decrease.

⁴ Occupations in the sample, by major occupational groups, were as follows. *Professional*: accountants and auditors; architects; authors; chemists, assayists, and metallurgists; civil engineers; clergymen; college presidents and professors; dentists; electrical engineers; funeral directors and embalmers; lawyers and judges; librarians; nurses; pharmacists; photographers; medical and osteopathic physicians; schoolteachers; social welfare workers; surveyors; veterinarians. *Managers*: postmasters, railroad conductors. *Clerical*: mail carriers; shipping and receiving clerks; stenographers, typists, and secretaries; telephone operators. *Sales*: hucksters and peddlers, newsboys, real estate agents. *Craft*: cabinetmakers; compositors and linotypers; electricians; electrotypers and stereotypers; locomotive engineers; locomotive firemen; machinists, millwrights, and toolmakers; millers (grain, flour, feed); molders (metal); pattern and model makers; piano and organ tuners and repairmen; pressmen and plate printers (printing); roofers and slaters; stonecutters and carvers; tinsmiths, copper-smiths, and sheet-metal workers. *Personal and protective service*: barbers, hairdressers, manicurists, and cosmetologists; boarding and lodging house keepers; bootblacks; elevator operators; firemen (i.e., fire fighters); guards, watchmen, and doorkeepers; marshals and constables; policemen and detectives; sheriffs and bailiffs. *Operatives*: dyers, laundry and dry cleaning operatives, railroad brakemen, railroad switchmen, street railroad and bus conductors, tobacco manufacturing operatives. *Laborers*: fishermen and oystermen; longshoremen and stevedores; lumbermen, raftsmen, and woodchoppers. *Farm*: farm proprietors and tenants.

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COHORT REPRESENTATION: RANGE AND PERSISTENCE

When we examine the range of CRI and its persistence over time, two clear patterns stand out. First, the CRIs of occupations differed considerably (table 3). The ranges were fairly similar across cohorts, but there were noticeable sex differences in this respect. The male CRI range increased gradually but consistently from the earliest through the latest cohort. In contrast, the pattern of change of female CRI ranges was U shaped. The female range was smaller than the male range in all cohorts but the earliest.

The second main characteristic of the CRIs is their persistence across cohorts. For each sex, the CRIs of different cohorts intercorrelate highly (table 4). Most intercohort correlations are higher than the correlations of male with female CRIs (table 5). These two sets of correlations are not strictly comparable because the cohorts are not independent (see n. 3 above) and the sexes are, but the different magnitudes suggest that re-

TABLE 2
PERCENTAGES OF U.S. MALE AND FEMALE
LABOR FORCES IN SAMPLE
OCCUPATIONS BY YEARS

Year	Male (%)	Female (%)
1920.....	32.2	26.0
1930.....	29.1	28.8
1940.....	25.8	27.2
1950.....	24.3	25.6
1960.....	19.9	25.4
1970.....	18.7	29.1

TABLE 3
RANGES OF COHORT REPRESENTATION INDEX SCORES
FOR MALE AND FEMALE COHORTS

COHORT	MALE		FEMALE	
	Highest and Lowest	Range	Highest and Lowest	Range
1920-40.....	.4893 — .3434	.8327	.4295 — .4648	.8943
1930-50.....	.4850 — .4088	.8938	.3903 — .4334	.8237
1940-60.....	.6472 — .3364	.9836	.4217 — .3125	.7342
1950-70.....	.6705 — .3557	1.0262	.5132 — .4749	.9881

cruitment and retention patterns are more alike for different cohorts of a sex than for the sexes within cohorts.

Together, these findings suggest that occupations establish distinctive, enduring, and partially sex-differentiated patterns of maintaining labor forces from young cohorts, old cohorts, or both. Occupations differ substantially in cohort representation, their differences persist over time, and the patterns are more alike across cohorts of each sex than they are for the male and female labor forces of given cohorts. Having found that cohort representation is relatively stable, we are in a position to examine its correlates and see whether their effects, too, persist across cohorts.

RECRUITMENT AND RETENTION VARIABLES AND SEX-SPECIFIC OCCUPATIONAL LABOR MARKETS

The means of the recruitment and retention variables give a general picture of the labor markets of the occupations in our sample. Table 6 shows these means, their standard deviations, and intercorrelations. Taken one by one, the variables were generally similar for the male and female labor forces.

TABLE 4
INTERCORRELATIONS OF COHORT
REPRESENTATION INDEXES FOR
MALE AND FEMALE COHORTS

Cohorts	Male	Female
Overlapping years:		
1920-40 × 1930-50.....	.86	.82
1930-50 × 1940-60.....	.86	.87
1940-60 × 1950-70.....	.92	.83
Nonoverlapping years:		
1920-40 × 1940-60.....	.87	.77
1930-50 × 1950-70.....	.86	.76
1920-40 × 1950-70.....	.79	.68

TABLE 5
ZERO-ORDER CORRELATIONS OF MALE AND
FEMALE COHORT REPRESENTATION
INDEXES

MALE	FEMALE			
	1920-40	1930-50	1940-60	1950-70
1920-40.....	.69	.72	.65	.65
1930-50.....	.49	.58	.62	.49
1940-60.....	.60	.66	.62	.60
1950-70.....	.57	.62	.62	.57

TABLE 6

ZERO-ORDER CORRELATION COEFFICIENTS, MEANS, AND STANDARD DEVIATIONS OF RECRUITMENT AND RETENTION
VARIABLES FOR MALES AND FEMALES (Females Above the Diagonals, Males Below)
A. 1920-40 THROUGH 1940-60

	Maize (N = 63)†							
	CRI	STRENGTH	SVP	SEX GROWTH	MKT	UNION	Mean	SD
1920-40:								
CRI.....	...	-.03	-.07	-.26	.04	-.11	.001	.172
Strength.....	-.13	...	-.29	.06	-.18	.35	2.41	.91
SVP.....	-.17	-.19	...	-.04	.19	-.36	5.78	2.00
Sex-specific growth.....	-.36	-.22	.1609	-.12	.16	.62
Market.....	.15	-.10	.14	.20	...	-.43	1.49	.50
Unionization.....	.01	.36	-.05	-.27	-.37	...	16.38	26.71
Female (N = 39):*								
Mean.....	.001	2.26	5.64	.42	1.54	6.18
SD.....	.197	.97	2.11	1.00	.51	15.08
1930-50:								
CRI.....19	-.13	-.15	.08	-.02	.000	.188
Strength.....	-.22	...	-.24	.25	-.11	-.11	2.41	.91
SVP.....	-.15	-.09	...	-.11	.17	-.34	5.78	2.00
Sex-specific growth.....	-.42	-.18	.15	...	-.32	-.15	.12	.56
Market.....	.14	-.10	.14	.07	...	-.41	1.49	.50
Unionization.....	-.08	.33	-.11	-.27	-.40	...	19.87	29.30
Female (N = 40):*								
Mean.....	-.0004	2.18	5.68	1.09	1.55	6.17
SD.....	.190	.84	2.10	2.68	.50	14.21
1940-60:								
CRI.....	...	-.04	-.06	-.15	.31	-.22	.001	.201
Strength.....	-.03	...	-.21	-.10	-.18	.36	2.41	.91
SVP.....	-.20	-.19	...	-.07	.10	-.27	5.78	2.00
Sex-specific growth.....	-.60	-.19	.32	...	-.06	-.13	.08	.54
Market.....	.06	-.10	.14	.11	...	-.46	1.49	.50
Unionization.....	.09	.32	-.16	-.25	-.42	...	24.59	33.95
Female (N = 51):*								
Mean.....	-.001	2.35	5.80	.60	1.51	17.54
SD.....	.174	.91	2.08	1.79	.50	31.03

B. 1950-70

	CRI	Strength	Ed	SVP	Sex Growth	Mkt	ID	Lic	Unlon	Earn	EC
CRI.....	...	-.17	-.21	-.07	-.09	.00	-.17	-.10	-.19	-.18	...
Strength.....	-.09	...	-.48	-.20	-.00	-.11	-.07	-.25	.34	-.20	...
Education.....	-.43	-.4753	.02	.26	.04	.55	-.34	.62	...
SVP.....	-.25	-.19	.62	...	-.22	.14	-.01	.40	-.17	.46	...
Sex-specific growth	-.52	-.26	.66	.3312	-.06	-.05	-.01	-.21	...
Market.....	-.03	-.10	.24	.14	.28	...	-.17	.23	-.41	.00	...
Industrial dispersion.....	-.21	-.24	.11	.05	.11	-.0204	-.15	.14	...
Licensure.....	-.22	-.24	.57	.39	.30	.24	.06	...	-.40	.36	...
Unionization.....	.03	.31	-.38	-.19	-.24	-.41	-.23	-.3807	...
Earnings.....	-.20	-.21	.69	.56	.29	.01	.00	.53	-.01
Earnings curve....	-.38	-.27	.39	.34	.30	-.25	.09	-.16	.04	.31	...
Female (<i>N</i> = 62):*											
Mean.....	-.001	2.40	1.01	5.82	.42	1.50	.32	1.23	25.58	1.32	4.42
SD.....	.212	.93	.20	2.00	2.01	.50	.25	.42	36.60	.54	4.70
Male (<i>N</i> = 63):†											
Mean.....	.001	2.41	1.10	5.78	.08	1.49	.27	1.22	27.40	1.14	-.97
SD.....	.221	.91	.26	2.00	.69	.50	.27	.42	37.40	.52	4.78

NOTE.—Ed = education, SVP = specific vocational preparation, Mkt = market, ID = industrial dispersion, Lic = licensure, EC = earnings curve.

* Female *N*'s vary from period to period because of missing data or because there were fewer than 50 women in some occupations. Female unionisation data were missing for one occupation in 1920-40, three in 1930-50, four in 1940-60, and five in 1950-70. Female data were missing for earnings for seven occupations, education for three occupations, and industrial dispersion for two. The female *N* for earnings curve was 16, too small to compute *R*'s.

† There were 63 occupations for males for all years for nearly all variables. Male unionisation data were missing for one occupation in 1920-40 and five in the other periods. Male data were missing for 23 occupations' earnings curves and two occupations' industrial dispersions.

Occupational Recruitment and Retention

But their intercorrelations differed for the sexes. The correlations, though modest, show opportunity structures favoring men over women. Occupations with fast-growing male labor forces (i.e., high male sex-specific growth) tended to be desirable ones. They were characterized by extensive SVP and education, low strength, local markets, high earnings, and licensure. Slow-growing and declining male labor forces tended to be in blue-collar occupations with the opposite characteristics, though with union protection.

The intercorrelations for women show a markedly different picture. Occupations with fast-growing female labor forces were not, in general, the most desirable ones. Education and SVP were negatively or barely correlated with growth of female labor forces. Occupations with high rates of female growth tended to be unlicensed and to have earnings below the total female labor force median. (As n. 2 above explains, the measure of unionization is not identical for the sexes. But the difference is not great enough to account for the sex differences in relationships of other variables to unionization.)

While the correlations show patterned occupational markets for both sexes, the variables do not intercorrelate so highly as to cause problems of multicollinearity. Their correlations with CRI are about the same as their intercorrelations or lower. Their distributions are good except that unionization is highly skewed because many occupations had no union members.

FINDINGS: RECRUITMENT AND RETENTION VARIABLES AND COHORT REPRESENTATION

Table 7 gives regression coefficients of CRI on the recruitment and retention variables and their standard errors. A positive coefficient means that a variable indicating high work qualifications, broad opportunities, high or rising rewards, or shelters favored retention or recruitment from an older cohort. A negative coefficient means that it favored recruitment from a young cohort.

We proposed that market characteristics of occupations influence recruitment and retention of labor forces. The findings show that occupations competed to recruit young workers more than to retain older ones. Characteristics that we termed recruitment variables aided recruitment from young cohorts as we expected. But some characteristics we termed retention variables also acted to recruit workers from young cohorts. Nearly all significant findings for men, and all of them for women, show occupational characteristics contributing to recruitment from young cohorts.

Occupations that maintained their labor forces mainly by disproportionate recruiting of the young tended to have the work qualifications of high strength requirements and educational level, opportunities consisting of high sex-specific growth and high industrial dispersion, earnings curves that

TABLE 7

REGRESSION COEFFICIENTS OF COHORT REPRESENTATION INDEX ON RECRUITMENT AND RETENTION VARIABLES FOR MALES AND FEMALES

	1920-40				1930-50				1940-60				1950-70			
	β	b	SE	β	b	SE	β	b	β	b	SE	β	b	SE	β	SE
Males:																
Strength.....	.261	-.049*	.024	-.302	-.062*	.025	-.148	-.033	-.483	-.117*	.025	-.483	-.117*	.037	-.483	.037
Education.....	-.532	-.444*	.011	-.532	-.444*	.260	-.532	.260
SVP.....	.184	-.016	.010	-.179	..	.017	-.036011019	..	.019
Sex-specific growth.....	.420	-.116*	.034	-.482	-.163*	.041	-.627	-.232*	-.236	-.076*	.043	-.236	-.076*	.030	-.236	.030
Market.....	.267	.091*	.040	.137	.051	.047	.140	.056	-.019	-.008	.067	-.019	-.008	.067	-.019	.067
Industrial dispersion.....	-.242	-.194*	.107	-.242	-.194*	.107	-.242	.107
Licensure.....	-.345	-.182*	.101	-.345	-.182*	.101	-.345	.101
Unionization.....	.082	.001	.001	-.076	-.001	.001	.034	.000	-.236	.001	.001	-.236	.001	.001	-.236	.001
Earnings.....400	.171*	.094	.400	.171*	.094	.400	.094
Earnings curve ^a	-.416	-.019*	.008	-.416	-.019*	.008	-.416	.008
R515663	..	.767676	..
R^2263240	..	.585858	..
N	61	58	58	..	40	40	40	..
Females:																
Strength.....	.003	.001	.037	.088	.020	.042	.026	.005	-.389	-.093*	.040	-.389	-.093*	.040	-.389	.040
Education.....	-.590	-.643*	.273	-.590	-.643*	.273	-.590	.273
SVP.....	.158	-.015	.017	-.159	-.014	.018	-.133	-.011	-.011	..	.013	-.011	..	.019	-.011	.019
Sex-specific growth.....	.295	-.058*	.033	-.189	-.013	.014	-.165	-.016	-.049	-.005	.015	-.049	-.005	.015	-.049	.015
Market.....	.012	.005	.072	.023	.009	.080	.235	.081	-.113	-.049	.066	-.113	-.049	.066	-.113	.066
Industrial dispersion.....	-.286	-.129	.129	-.286	-.129	.129	-.286	.129
Licensure.....	-.052	-.026	.085	-.052	-.026	.085	-.052	.085
Unionization.....	-.208	-.003	.002	-.080	-.001	.003	-.177	.001	-.362	-.002*	.001	-.362	-.002*	.001	-.362	.001
Earnings.....197	.081	.087	.197	.081	.087	.197	.087
R332438	..	.545454	..
R^2110614	..	.292929	..
N	38	37	47	..	50	50	50	..

^a Occupations with data missing for earnings curve were dropped for the 1950-70 analysis.

* Significant at .10 level of probability on a two-tail test.

Occupational Recruitment and Retention

rose with age, and the shelter of licensure (for men) or unions (for women). Occupations less able to compete for young cohorts recruited or retained labor forces disproportionately from older cohorts. Only local markets and high earnings contributed significantly to retention or recruitment from an aging cohort.

Recruitment and Retention of Male Labor Forces

Occupational market characteristics of all four kinds—work qualifications, opportunities, rewards, and shelters—helped occupations recruit young workers from one or more male cohorts. The only occupational characteristics that contributed to recruitment or retention of aging male labor forces were local markets for the 1920–40 cohort and high earnings for the 1950–70 cohort.

High education and strength requirements favored recruitment from young male cohorts. In the case of education, the mechanism probably derived from continuous educational upgrading of successive cohorts. Occupations with above-median education recruited young men in preference to men from older, less educated cohorts. Opportunities from male sex-specific occupational growth and industrial dispersion also went disproportionately to the young; declining occupations and industrially concentrated ones, such as farming, retained many aging men. Licensure led occupations to recruit young men more than to retain aging men. Licensed occupations call for training that workers normally acquire when young. Licensure protects an occupation from unlicensed competitors but does not appear to protect its older men from incoming young men. Unionization was unrelated to male CRI. Unions could shelter aging men without preventing occupations from recruiting numerous younger men.

Recruitment and Retention of Female Labor Forces

The model is considerably less powerful in explaining female than male CRI (compare R^2 s in table 7). Not as many occupational market characteristics made significant contributions to female CRI as to male CRI; those that were significant for females favored recruitment from young cohorts. Only sex-specific growth contributed to female CRI for the first cohort, and no variable did for the two middle cohorts. For the 1950–70 cohort, three characteristics made similar contributions to the recruitment of young workers of both sexes. These were education, strength, and industrial dispersion.

Unionization, which had no effect on male CRI, shrank the representation of older women of the 1950–70 cohort. Unions served occupations' recruitment of young women but provided insufficient shelter to retain aging

women. Unions' emphasis on seniority ladders may even have hindered recruitment of older women. Many unionized occupations develop internal labor markets in which entry is reserved for young workers who enter at the bottom levels and move up slowly.

Cohort Differences

Comparison of cohorts is hampered by their lack of independence and by the addition of variables for 1950-70. Within these limitations, the data for men suggest that the depression of the 1930s may have modified the effects of occupational market characteristics on CRI. Local markets contributed to retention or recruitment of aging labor forces during the 1920-40 depression period but not in later periods.

The explanatory power of our model differs for the cohorts. The addition of variables for 1950-70 accounts in part for its efficacy in explaining that cohort's CRI, but for men the model improves from each cohort to the next (see R^2 s in table 7). Patterns of male labor force recruitment were becoming more developed. It appears that occupations gained more control over male labor markets as workers became more specialized. This is clearly suggested by rank ordering the significant coefficients in table 7. For the 1950-70 cohort, work qualifications variables ranked first and opportunity variables last, a reversal of the earlier patterns. It is unclear from our data whether female labor force recruitment patterns also were becoming more developed. The model does not improve consistently over time for females. It performs best for 1950-70, but we do not know to what extent this improvement is due to addition of variables to the model.

Sex-matched Subsamples of Occupations

The occupations in the male and female samples were not identical because missing or noncomparable data reduced the female samples. To see whether sex differences in the findings might have resulted from the use of different samples of occupations, we extracted for each cohort a subsample composed of the occupations for which we had the same information for both sexes. We dropped the earnings curve as a variable because data on it were incomplete for females.

Table 8 gives regression coefficients and their standard errors for the male sex-matched sets of occupations. (The findings on women are not shown; they are the same as in table 7 because the same occupations make up the full and sex-matched female samples.) The findings on men resemble those for the full male sample. Only two variables shift from significant to insignificant contributions to CRI; these are strength for 1920-40 and licensure for 1950-70. Only SVP for 1920-40 and 1930-50 and local mar-

TABLE 8
REGRESSION COEFFICIENTS OF COHORT REPRESENTATION INDEX ON RECRUITMENT AND RETENTION VARIABLES:
SEX-MATCHED MALE SUBSAMPLES OF OCCUPATIONS

VARIABLES	1920-40			1930-50			1940-60			1950-70		
	β	b	SE	β	b	SE	β	b	SE	β	b	SE
Strength.....	-.173	-.029	.028	-.298	-.062*	.032	-.028	-.005	.022	-.524	-.128*	.032
Education.....	-.584	-.492*	.212
SVP.....	-.322	-.025*	.013	-.333	-.028*	.013	-.171	-.014	.010	-.005	-.001	.015
Sex-specific growth.....	-.355	-.088*	.040	-.551	-.158*	.043	-.606	-.185*	.037	-.403	-.135*	.052
Market.....	.261	.086*	.052	.165	.058	.054	.235	.079*	.041	.083	.037	.054
Industrial dispersion.....	-.218	-.184*	.093
Licensure.....	-.121	-.063	.073
Unionization.....	-.091	-.001	.002	-.178	-.002	.002	-.078	-.000	.001	.077	.001	.001
Earnings.....274	.117*	.067
R.....	.50647173
R ²25415154
N.....	38	37	47	50

* Significant at .10 level of probability on a two-tail test.

kets for 1940–60 shift from insignificant to significant contributions. The overall importance of the variables for males in the sex-matched samples is far more similar to the full-sample male pattern than to the female pattern. In general, findings on the sex-matched samples confirm those on the full samples.

Male and Female Occupations

Are there unexamined variables that account for the differences between the male and female patterns? Conceivably, occupations' sex compositions might explain the differences. We define an occupation's sex composition in each period as the mean of the percentages of its workers who were female in the three decennial census years included in the period. By this measure, the occupations in the male samples from 1920–40 through 1950–70 included, on the average, 16.0%, 16.8%, 17.2%, and 18.9% females. The mean percentages female in the female samples of occupations were 25.8%, 26.0%, 21.4%, and 19.2%. (The differences between the male and female means, and the decline in the percentages of women in the female sample occupations, result from the omission of some occupations from the female samples. More had to be omitted for early cohorts than for later ones, and those omitted tended to be heavily male occupations.)

We computed zero-order correlations of the recruitment and retention variables, and of CRI, with sex composition for the male and female samples of occupations (not shown). In general, sex composition correlated more highly with recruitment and retention variables than with CRI. The relevant sex comparisons are for variables measured separately for the sexes: education, sex-specific growth, industrial dispersion, earnings, and earnings curve. There was only one substantial sex difference: the more female an occupation, the greater its male sex-specific growth but the smaller its female sex-specific growth—that is, labor forces of each sex grew fastest in occupations with high percentages of workers of the other sex. This pattern was reversed for the 1950–70 female cohort, with a small positive correlation between percentage female and sex-specific growth of occupations; but even in that period, the positive correlation of these two variables was greater for men than for women.

To examine possible mediating effects of sex composition, we did a path analysis with the recruitment and retention variables as exogenous variables, sex composition as an intervening variable, and CRI as the dependent variable. Table 9 gives the results pertaining to sex-specific growth, the only variable whose relation to CRI differed substantially on the basis of sex composition. Table 9 shows that sex composition mediated little of the total effect of sex-specific growth on CRI for either sex. Nor did it mediate a significant percentage of the total effect of any other variable (figures not

Occupational Recruitment and Retention

TABLE 9

EFFECTS OF SEX-SPECIFIC GROWTH ON MALE AND FEMALE COHORT
REPRESENTATION INDEXES MEDIATED BY PERCENTAGE OF
OCCUPATION FEMALE

EFFECTS	MALE		FEMALE	
	β	b	β	b
1920-40:				
Total.....	-.420*	-.1156	-.295*	-.0580
Direct.....	-.393*	-.1081	-.325*	-.0639
Indirect.....	-.027	-.0075	.030	.0059
Percentage mediated.....	6.4	...	8.5	...
1930-50:				
Total.....	-.482*	-.1628	-.189	-.0134
Direct.....	-.510*	.1709	-.194	-.0138
Indirect.....	.028	.0082	.005	.0004
Percentage mediated.....	5.2	...	2.5	...
1940-60:				
Total.....	-.627*	.2322	-.165	-.0160
Direct.....	-.610*	.2262	-.184	-.0179
Indirect.....	-.017	.0060	.019	-.0019
Percentage mediated.....	2.7	...	9.4	...
1950-70:				
Total.....	-.236*	.0761	-.050	-.0053
Direct.....	-.211	.0681	-.042	-.0044
Indirect.....	-.025	-.0080	-.008	-.0009
Percentage mediated.....	10.6	...	16.0	...

NOTE.—We used Alwin and Hauser's (1975) procedure for decomposing effects.

* Significant at .10 level of probability on a two-tail test.

shown). Sex compositions of occupations do not explain the differences between their patterns of recruiting and retaining male and female labor forces. This suggests that females in male occupations are more like other females than like males in their labor market behavior.

SUMMARY AND DISCUSSION

This paper develops the index of *cohort representation* to measure the extent to which an occupation's labor force is from a given cohort of workers at successive times in the cohort's work life. We follow four male and four female cohorts over 20-year periods. We show that occupations differ in their recruitment and retention of labor forces from given cohorts, and that their distinctive patterns persist over time across cohorts. Some occupations recruit workers from a cohort when it is young but replace them later with young workers from subsequent cohorts. Others retain and/or recruit more workers from aging cohorts. We develop a model of occupational competition for labor forces to account for the differing patterns of cohort representation. The model consists of work qualifications, opportunities, rewards, and shelters.

We found that the model predicted recruitment better than retention. We had expected occupational rewards and shelters to contribute to retention of a labor force from a cohort but found that only the reward of earnings and the opportunity of local markets (the latter for the first cohort only) had that effect for males and that no occupational characteristic did for females. All other occupational characteristics in the model, if they had effects, helped to recruit young workers. It appears that occupations compete more to recruit young workers than to retain the aging. The occupational characteristics that differentiate labor markets do so primarily by affecting the inflow of young workers.

Occupations' male and female cohort representations were highly correlated, but not all occupational characteristics operated in the same ways for both sexes. The model was more powerful in explaining male than female cohort representation, and its explanatory power increased progressively from each cohort to the next for males but was stable for females until it rose for the latest cohort. Four variables made similar contributions to recruitment from one or more young cohorts of both sexes: the work qualifications of education and strength, and the opportunities provided by occupational growth (in 1920-40) and by broad dispersion of an occupation across industries. These are the occupational characteristics rooted in the division of labor, which we had designated recruitment variables.

Other characteristics (and occupational growth for the three most recent cohorts) affected male and female cohort representation differently if they affected either. Licensure favored recruitment of young males but did not affect female cohort representation. The same was true of occupational growth for three of four cohorts. Unionization favored recruitment of young female workers over retention of aging women but was unrelated to male cohort representation. Local markets (in 1920-40) and high earnings helped occupations retain aging male labor forces but made no contribution to retention or recruitment from female cohorts. (Rising earnings curves helped to recruit young males. We lacked information on this variable for females.) Sex compositions of occupations did not account for the sex differences in contributions of occupational characteristics to cohort representation.

These findings suggest that even when occupations draw workers from more than one distinct labor force—in this case, from male and female labor forces—occupational characteristics based directly on the division of labor give rise to recruitment patterns that operate similarly for the separate labor forces. The characteristics that contributed in similar ways to male and female recruitment—two work qualifications and the growth and industrial dispersion of work opportunities—are of this kind.

On the other hand, organizational characteristics of occupations, which are not integral parts of the division of labor, may not operate alike for

separate labor forces. The characteristics that contributed to cohort representation for one sex but not for the other (except local markets for the first cohort and occupational growth for the last three) are of this kind. Unionization, licensure, and earnings arise from actions of occupational groups or employers which organize labor markets within an existing division of labor. These characteristics differentiated the markets for male and female labor forces. With the exception of unionization, which discriminated against older women in favor of young women, organizational characteristics of occupations tended to channel male but not female labor forces. Female labor markets are somewhat amorphous and would be even more so were it not for the constraints imposed on labor markets by the specialization of occupations.

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The Spread of Municipal Reform: Temporal, Spatial, and Social Dynamics¹

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The adoption of commission and managerial administrative structures and their subsequent abandonment by the 267 largest American cities during the period 1900–1942 is studied using event-history methods. Full-period equations show no significant direct effect of percentage Catholic (culture clash) or population size (hierarchical diffusion) and only a modest impact of city age (modernization). A strong effect from average annual manufacturing wages is found, but in a direction contrary to that posited by the class-conflict hypothesis. The variable affecting transition rates most strongly appears to be regional adoption percentages, an indicator of neighborhood diffusion. These findings suggest revisions of the conventional image of the municipal reform experience.

Among the legacies of the Progressive era was the dramatic transformation of American municipal government. National movements in the late 19th and early 20th centuries sought improved conditions in urban life through such reforms as home rule for cities, direct government (initiative and referendum), nonpartisan elections-at-large, the short ballot, the merit system, employment of professionally trained personnel, and strict supervision of city contracts (Griffith 1974*b*, pp. 25, 34, 59). While one reform tradition emphasized substantive improvements in housing, transportation, sanitation, crime control, and the like, the “structural” or administrative reformers put their faith in altering the formal machinery of government as a solution to major urban ills (Rice 1977, pp. xii, 63). In practice, of course, many reformers and their programs took both approaches.

This paper concentrates on the spread of administrative reform among city governments in this century. It attempts to blend theoretical and empirical approaches extant in the research literature using a methodology that renders a more faithful representation of the dynamic process under-

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lying the spread of innovation. To set the stage, a brief historical recapitulation is in order.

Two major waves of municipal executive innovation swept the United States in the four decades from 1900 to the Second World War. Prior to that time, all cities may be considered to have had the traditional mayor-council form of administration (aside from a few town-meeting governments in smaller New England communities). In response to a devastating tidal flood in 1900, the city of Galveston invented the commission form of city government. Called the "pre-eminent municipal expression of the short ballot during the Progressive Era" (Griffith 1974*b*, p. 56), it usually consisted of a half-dozen elected commissioners, each heading a major functional department. Although one commissioner might be the nominal head, this compartmentalization of responsibility and the absence of control by a representative council proved to be a fatal weakness of the structure. Despite frequent adoptions among medium-sized cities, the commission form had peaked by the First World War when its limitations became increasingly apparent (Griffith 1974*b*, p. 61) and structural reform turned toward the council-manager form.

Appearing first in Staunton, Virginia (1908), the council-manager innovation combined an elected city council (often elected at large rather than by wards, and without party identification) with the centralized expertise of a professionally trained manager to oversee day-to-day business of the various municipal bureaucracies. In 1919, the National Municipal League gave its imprimatur by making the council-manager structure part of its model city charter. The form began replacing both the commission and the older mayor-council government until it was the most popular type among all cities over 25,000 in 1960 (Alford and Scoble 1965).

Municipal reform has been studied extensively, both by historians using a case-study approach (e.g., Stone, Price, and Stone 1940; Stillman 1974; Fox 1977) and by social scientists using quantitative comparative techniques (e.g., Kessel 1962; Dye and MacManus 1976). However, a new effort in the latter vein is warranted. Previous efforts to understand the innovation process relied mainly on inappropriate data and static, cross-sectional analysis methods, thereby reaching erroneous conclusions about the dynamics of the municipal reform process. The research reported here uses data contemporary to the social change process and applies recent event-history methods that are ideally suited to modeling the spread of municipal reform. To develop the background for these analyses, the following two sections review two distinct, but not inherently mutually exclusive, theoretical and empirical traditions in the study of municipal change. The two main foci discernible in the literature are studies that employ a spatial-temporal diffusion framework and studies that emphasize one or another form of sociodemographic causation.

DIFFUSION OF INNOVATION

Theoretical and empirical work on the diffusion of innovations has been conducted mainly by geographers and anthropologists and to a lesser extent by sociologists. Overwhelmingly, this literature concentrates on the spatial and temporal aspects of the spread of new ideas, customs, laws, and technology among individuals and social organizations.

A fundamental assumption underlying most approaches is that the adoption of innovations is primarily a learning process: the "gain in adoptions is due to nonadopters' emulation of adopters" (Gray 1973, p. 1176). Critical variables that affect the growth process are the number of adopting units, the number with potential to adopt, and their distributions in social and physical space. Patterns of communication and interpersonal interaction between adopters and potential acceptors result in innovations spreading "outward from their point of origin in a wave-like manner" (Hudson 1972, p. 4; also Hagerstrand 1967). The cumulative distributions of many types of innovation tend toward the S-shaped cumulative normal curve (Rogers 1962, p. 312).

Two basic generalizations reflect the influence of nonrandom communication networks and the presumed decay functions of physical and social distance:

1. Neighborhood effect. "The closer a potential adoption unit to the source of innovation, or to another unit that had already adopted the innovation, the greater the probability that it will adopt before potential adopters that are further away" (Cohen 1972, p. 14). In geography, neighborhood effects are often assumed to operate through proximity in physical space, with adjustments for geographical barriers. More generally, social interactions involving communication, persuasion, and imitation tend to follow a spatial gradient.

2. Hierarchical effect. "The higher the ranking of a potential adoption unit in a [size, social status, or other] hierarchy, the greater the chance of adoption before units that are lower on the hierarchy" (Cohen 1972, p. 15). This hypothesis may be a generalization of the tendency for higher-status social units to be "opinion leaders" from which information about innovation trickles down to lower-status units (e.g., Katz and Lazarsfeld 1955). A hierarchical effect may also operate in a social space where communication and influence are not mixed randomly but confined within networks organized along status rankings.

Although quite complex (and often untestable) mathematical models have been designed from these concepts (see Brown 1968; Hudson 1972), the few empirical applications to the diffusion of political innovations

among states and cities have been relatively simple.² McVoy (1940) plotted the locations and dates of council-manager adoptions on a U.S. map and interpreted the pattern as evidence for a diffusion gradient extending from the innovation's source in Staunton, Virginia. But an evident early "jump" of the form to the Pacific Coast implies a hierarchy effect, possibly due to California's propensity to take the lead in launching or adopting many types of political innovations. Scott's (1968) study of council-manager adoptions by suburbs in Ohio, Illinois, and California found state differentials in the rate of change between 1950 and 1960, suggesting possible neighborhood effects within regions.

Other municipal diffusion studies examined specific programs or technological adoptions. Crain (1966) found both neighborhood and hierarchy effects in the fluoridation of drinking water among a thousand cities during the 1950s. He also showed that the acceptance of and resistance to innovation was not invariant across time or region. Agnew, Brown, and Herr (1978) also found strong hierarchy effects for adoption of fluoridation, urban renewal, public housing, and automated data processing by 460 central cities. Larger, higher-ranked urban places were more likely to be adopters and to adopt earlier than other cities. However, no consistent evidence of geographical neighborhood effects was found. Feller et al.'s (1976) analysis of 43 technological innovations (in solid waste, air pollution, firefighting, and traffic control) likewise found that larger cities adopt more innovations. However, neither social and geographic proximity nor interaction were examined for evidence of neighborhood effects.

At the state level, Walker (1969) and Gray (1973) examined adoption patterns for a large variety of policies and programs. Walker constructed an index of a state's propensity to innovate by averaging the speed of adoptions for 88 different programs. He discerned a complex hierarchical pattern of pioneering states, regional leaders, and laggards, yielding the image of spreading ink blots on a map. Larger, wealthier, and more industrialized states seemed more prone to early adoptions than their lower-status sisters. But Walker also emphasized regional cue taking and evaluation, implying a persistent neighborhood effect on program diffusion. Gray's concern was with the shape of the cumulative diffusion curves and the effect of federal intervention on alteration of the typical S-shaped cumulative patterns over time. She also found a tendency for higher-status and politically competitive states to be early adopters, but she concluded

² Rogers and Shoemaker (1971) listed some 2,000 studies of innovation diffusion, but the number which dealt with social organizations as the adopting units is only a small fraction of that total. Feller, Menzel, and Kozak (1976) asserted that local and state governments were the subject of fewer than 150 diffusion studies. Many of these studies concerned technological adoptions rather than political and administrative practices.

that "innovativeness" was not a stable attribute for all types of policies for long periods.

The theoretical and empirical literature on political innovation among municipalities leaves unanswered questions about the spatial and temporal diffusion of the commission and council-manager forms. But even if the relative impact of hierarchy and neighborhood on administrative structural reform can be determined, we will not have gained a complete picture of the change process. As Cohen remarked about the geographers' approach, "Space and time are elements that are external to the diffusion process, while social, economic, cultural, psychological, or other behavioral factors are endogenous causes of acceptance, rejection, and spread of innovations" (1972, p. 14). A fuller understanding of the processes of municipal reform requires the identification of social forces shaping diffusion. This effort can draw on a modest, but flawed, research literature on the social correlates of city government form.

SOCIAL INFLUENCES

The literature on social factors that influence municipal political change can be organized under three distinct, but not mutually exclusive, theoretical perspectives. After each approach is discussed separately, the empirical findings relevant to all three approaches will be summarized and their shortcomings noted.

Culture Clash

Histories of the immigrant experience present a vivid wealth of detail on living conditions, employment experiences, and political adjustments faced by diverse ethnoreligious peoples immigrating to America (e.g., Handlin 1973). During the late 19th century a major political cleavage developed between pietistic groups—often older, established northern European Protestants—and liturgical groups—the more recent immigrants from Catholic and eastern European nations (Kleppner 1970; Jensen 1971). At the core of this cleavage were value conflicts over the pietistic attempts to regulate private morality by public laws on temperance, sexual conduct, and religious observances. Hofstadter's (1955, p. 184) classic account of the Progressive movement emphasized these efforts by upper- and middle-class native-born Protestants to preserve their social status in the face of growing immigrant concentrations that threatened to wrest political control of the urban centers (see also Handlin 1973, pp. 180–202). Immigrant and native Americans faced each other uncomprehendingly across a gulf between political styles based on divergent conceptions of the basis of political order.

Elazar (1970, 1972) posited a detailed relationship among three distinct political subcultures and various ethnoreligious communities (see also Wilson and Banfield 1964, 1971). Moralistic, individualistic, and traditionalistic subcultures originated in Old World differences among peoples coming to America over the centuries. The tendency of members of these groups to settle together and to move across the country in distinct migration "streams" while keeping intact their political beliefs over the generations resulted in a complex "cultural geology" (Elazar 1972, p. 103). Sectional (i.e., regional) concentrations of specific cultural groups were overlaid by migration streams (Elazar delineated 15 major westward movements) in response to three historical frontiers (rural-land; urban-industrial; and metropolitan-technological).

The political subcultures had institutional manifestations. The moralistic subculture favored such reformed urban government structures as nonpartisanship, at-large elections from a short ballot, and professional civil administration with a merit system and the commission or council-manager structure. The individualistic subculture favored the nonreformed system of the "boss," large councils, party machinery, ward-based representation, and extensive favoritism in patronage and contracts. The chief rationale for political participation was to provide benefits for one's family and ethnic group by electoral control of the city government (see Knoke [1981] for other detailed contrast).³

As the most politically responsive of the three basic administrative structures, the mayor-council form would be most likely retained in cities with heavy ethnic concentrations. The direct election of a mayor enhances electorate control most compatible with the individualistic political subculture dominant among immigrant groups (see Gordon [1968] for a detailed discussion which also includes arguments for ward-based council seats in immigrant-dominated cities). Clark (1975) detailed the crucial role of the Irish Catholics in the urban machine politics of many cities. In contrast, the innovative commission and council-manager forms, touted as a means to get "politics" out of city government, would be favored by cities with homogeneous populations of native-born Americans, where the moralistic subculture was strong and particularistic interests were weak or nonexistent.

Class Conflict

Class-conflict explanations of municipal structural reform stress the struggle between economic groups for local political domination. The massive

³ The third subculture type, traditionalism, was rooted in ambivalence toward the marketplace and a paternalistic view of the public welfare. It survived mainly in Southern plantation agrarianism.

industrialization of America after the Civil War, concentrated in the metropolitan areas, and the accompanying rise of a potentially militant labor union movement were the twin triggers for a business-instigated municipal reform effort. Hays put the relationship bluntly: "The movement for reform in municipal government, therefore, constituted an attempt by upper-class, advanced professional and large business groups to take formal political power from the previously dominant lower- and middle-class elements so that they might advance their own conceptions of desirable public policy" (1964, p. 162; see also Hayes 1972, pp. 10-14).

The structural arrangement of commission and council-manager government was an institutionalization of a set of "business principles"—chiefly centralization of authority, accountability, and efficiency—for managing city hall along lines of the emerging modern business corporation, with minimal interference from working-class concerns. By weakening the party system, especially the political machine, the numerically smaller business and professional classes could maintain their urban ascendancy despite a precarious electoral position. In this perspective, the reform rhetoric urging return of democratic control of city government to the populace was a cover for the industrialists' antidemocratic coup.

Some analysts (e.g., Burnham 1970, pp. 76, 90; Hays 1974, pp. 16-25; Domhoff 1978) placed the middle- and upper-class urban triumph within the larger context of a national trend including state and federal political transformations. The prominence of leading businessmen, lawyers, journalists, and academics in the organizational apparatus of the "good government" movements was cited as evidence of the upper-class domination of reform. Domhoff documented the institutionalization of urban policy-planning organizations that formed a national network to carry out the conversion of city government to the new forms. The result, he concluded, was "to increase the centralization of decision making, remove governmental functions from electoral control, and decrease the percentage of workers and socialists elected to city councils" (1978, p. 161).

Modernization

Both the culture-clash and the class-conflict explanations of the urban reform movement depict municipal change as an outcome of struggles between opposing groups over the allocation of resources within the local community. Thus, both perspectives adopt a competitive-politics model, but they stress different social cleavages as central to the consequences for the community of the struggles. In contrast, a third explanation is less concerned with intra-community conflicts than with functional imperatives of national development. Industrialization, urbanization, immigration, specialization, and professionalization were all social trends of late 19th- and

early 20th-century America that operated at the local as well as the national level. The evolution of bureaucratic forms for managing complex social organizations was present in private enterprise as well as city, state, and federal governments. From this perspective, "struggles over the distribution of political power among classes and factions within cities continue to be important, but not as important as the power and ability of each city's political system *as a whole* to cope with the impact of the transformation of the national social system. . . . When no city possessed a dependable ability to put out fires, fight epidemics and disease, or master the perpetually intractable task of cleaning the streets, it mattered little which class or faction elected its leaders to the official positions of power in the city government. The failure of indigenous innovation was every city resident's municipal problem" (Fox 1977, pp. 4-5).

Community growth and increasing organizational complexity generated social pressures compelling the substitution of "modern" for traditional forms of governmental administration and service delivery. Often, national organizations were a major force propagating municipal innovation. For example, the National Municipal League's 1899 municipal program called for a strong mayor at the head of a functionally departmentalized administrative structure and a single-chamber representative city council. This rough proposal was bolstered after 1902 when the census bureau's urban statisticians began providing comparative city expenditure data on a standard functional basis, with the explicit aim of demonstrating that performance improved under the new government structures (Fox 1977, p. 77). The council-manager form was championed by its proponents as more professional, bureaucratic, and efficient, and less "political" than the traditional mayor-council structure. Under the modernization perspective, urban reform could take root more readily in younger, faster-growing cities with extensive ties to national sources of information about such innovations and their alleged benefits.⁴

Previous Studies

Without exception, previous empirical studies of the earliest municipal administrative structural changes were historical case studies, not quantitative comparative analyses. Municipal government historians, such as Stone

⁴ As noted above, the analytic distinctions in the theoretical perspectives may be empirically intertwined. For example, one might argue that Catholic immigrants tended to predominate in the urban working class and to support a political culture resistant to modern forms of governance. Wilson and Banfield (1964, 1971), in their famous ethos concept, have been the most prominent expositors of this view. But an extensive literature attacking the ethos thesis (see, e.g., Miller and Bennett 1976 and Vanecko and Kronenfeld 1977) suggests that not much utility is gained by treating the three analytic dimensions as wholly coincident.

et al. (1940), Griffith (1974*a*, 1974*b*), Fox (1977), and Rice (1977), employed traditional methods of document analysis, anecdotal recall, and narrative synthesis to paint a richly detailed portrait of the reform movement, its leadership and key events. While deeply informative, such approaches do not easily address the hypotheses presented above about the processes underlying the spread of governmental innovations. However, another stream of research on municipal government structure can be searched for clues to the social factors affecting reform.

A half-dozen studies by sociologists and political scientists investigated the social, economic, and political correlates of city administrative structure in the most recent decades. To the extent that analysts tried to infer, from correlations and regressions based on 1960s data, the causes of change occurring a half century in the past, their analyses are obviously flawed. Much demographic change has intervened, and multiple changes have occurred in some cities which could attenuate the conditions prevailing during the reform movement's greatest influence. Still, these studies provide the nearest analogue to the investigation reported in this paper. Thus, if for no other reason than to demonstrate the difference that a correctly specified analysis can make, the previous findings are briefly summarized.

Alford and Scoble reported the correlates of administrative structures among the 676 largest cities (over 25,000 population) in 1960. They found that "high mobility, low private school population [an indicator of low religious diversity], high white-collar population, low proportion of native-born persons of foreign parentage, high level of population growth, high level of education, and smaller size are all associated with the council-manager rather than the mayor-council form, and in roughly that order of association . . ." (1965, p. 88). Population mobility seemed to be a key variable in controlled cross-tabulations, with high turnover weakening the potential opposition to the adoption of professionalized administration.

Kessel (1962) also found that growing cities were more likely to be managerial, while a large foreign-born population was related to the mayor-council system. But Wolfinger and Field (1966) showed that by controlling for regional location most of the impact of ethnicity on reform structure could be explained. That is, heavily ethnic and mayor-council cities were located in the Northeast while the growing, white-collar communities of the Southwest were more likely to be council-manager cities. Dye and MacManus (1976), using discriminant analysis instead of cross-tabulations, found foreign birth to be a more important variable than region, although each variable when used alone discriminated poorly among types. Socioeconomic indicators predicted structure much better within regions than for the nation as a whole.

Gordon (1968), in perhaps the most careful study of this genre, per-

formed one of the few quantitative analyses to use variables measured before the Second World War. He cross-tabulated the percentage of foreign born with municipal structure in each decade from 1933 to 1960. Despite the historical trend toward greater managerial government among all cities over 30,000 population in 1930, heavily immigrant cities continued to maintain the mayor-council form, a relationship not eliminated by controls for region, economic base, population size, or growth. However, when adoptions and abandonments of the managerial system after 1933 were investigated, the percentage foreign born had no effect. Whether these findings generalize to the earlier reform era was not investigated, since Gordon asserted (incorrectly) that data from that time were not available.

Finally, Clark (1968), using 1967 survey data on the 51 cities of the National Opinion Research Center's Permanent Community Sample (population range from 50,000 to 700,000), performed a multiple-regression analysis to identify the causes of reform structure and to determine its impact on city government expenditures. High poverty level, high education level, and economic diversification were positively related to reform structure (an index of managerial form, nonpartisan elections, and at-large districts), while large population was associated with the mayor-council form. Although the percentage Catholic was negatively correlated with reform structure, controlling for other variables substantially reduced its impact.

Taken together, these quantitative comparative studies offer some support to each of the three perspectives. Their chief shortcoming is that none investigated the initial era of change in municipal government, using characteristics of the cities at the time of adoption. Given the substantial processes of metropolitan growth, suburbanization, black in-migration, and ethnic assimilation, the significant correlates of municipal structure in the 1960s and 1970s cannot be reasonably interpreted as those factors that might have caused adoptions as much as 50 years in the past. The aim of the present paper is to remedy these shortcomings by investigating municipal changes with appropriate data and with a method that more faithfully captures the dynamic change process than does cross-tabulation or multivariate linear models with cross-sectional or panel data.

DATA AND MEASURES

No convenient single source treats changes in municipal government form by year from 1900 to 1942. Ideally, data on all four major dimensions of structure should be analyzed: partisan and nonpartisan elections; at-large and ward constituencies; council size; and the three basic types of admin-

istration: commission, council-manager, and mayor-council. However, no data on year of innovation could be found for the first three structures, so the analysis in this paper is confined to the dynamics of executive structural changes.⁵

Several sources were used to piece together complete dated histories of changes in municipal executive form. The International City Management Association's *Municipal Year Book* began to identify the form and date of adoption for all cities in 1934. Prior editions had listed only managerial adoptions and dates. Chang (1918) and Rice (1977) gave detailed histories of commission form adoptions, while the U.S. Bureau of the Census (1916) listed adoption dates for commissions through 1915 (see also Bradford 1911 and Woodruff 1911). Bromage (1940) listed abandonments of the council-manager form, and Nolting (1969) listed cities abandoning and readopting this form.

Since the test of hypotheses requires complete data on city characteristics, only cities reaching 30,000 population by the 1930 census were analyzed.⁶ The analyses below follow Gordon's practice of eliminating those cities whose municipal government form was prescribed by state law. Also, two cities incorporated after the First World War (Clifton, N.J., and Dearborn, Mich.) were dropped because their municipal histories began much later than those of other qualifying cities. The analysis is thus confined to 267 cities.⁷

During the four decades under study, 61% of the cities changed their form of administration at least once. Of those which changed once, 36% changed a second time (22% of all cities changed twice). Only Trenton,

⁵ The frequent assumption that reform structures covary strongly was questioned by Lyons (1977), who showed that unidimensionality was not present for 1970 data on 285 large cities. However, Gordon's (1968) data from 1933 on 201 large noncommission cities showed substantial covariation, with 85% of council-manager cities having at-large elections and 85% of mayor-council cities having council elections by wards. Four decades later, according to Lyons's data, this association had diminished to about 66% in both cases.

⁶ To use a lower population cutoff would increase the proportion of cities for which earlier censuses did not tabulate social characteristics. The 30,000 population criterion for 1930 has the additional advantage of replicating the universe of cities studied by Gordon (1968). He stated that 306 cities met this criterion, but the true number is 310. The source of the discrepancy is unclear.

⁷ The 13 Indiana cities are required to use the mayor-council form and the three Alabama and two Utah cities are required to use the commission form. Of the 20 Pennsylvania cities, all must use the commission form, except Philadelphia, Pittsburgh, and Scranton, which must remain mayor-council cities. Baltimore and Louisville must also have mayor-council governments, while Washington, D.C., lacked home rule. The starting date for the analyses was taken as 1900, before the invention of the commission and council-manager forms. Hence, all cities were assumed to have the mayor-council form at that time. Nine cities did not come into existence until shortly after 1900; all but two of them (Glendale and Port Arthur, both commission cities) began as mayor-council cities.

New Jersey, changed three times (to commission in 1911, council-manager in 1935, and back to mayor-council in 1939). As table 1 shows, the most typical sequence of changes was from the initial mayor-council to the commission form, prior to the 1920s. About half of the 111 cities adopting the commission plan abandoned it by 1942, twice as often for the council-manager plan as for a return to the mayor-council plan. The council-manager form is something of an absorbing state: once entered it was never left for the commission plan and only dropped eight times in favor of a return to the mayor-council type. By the outbreak of the Second World War almost half of the cities had mayor-council structures, 29% had council-manager, and 22% had commission forms.

Several indicators were constructed to analyze neighborhood effects on administrative structure diffusion. Geographic distance from Galveston, Texas, the origin of the commission form, was measured in a series of 500-mile concentric rings. Similarly, geographic distance from Staunton, Virginia, the initiator of the council-manager form, was measured by another series of concentric rings. Third, a city's location in or near one of the five most innovative states in Walker's (1969) study was measured by three categories: (1) the city is located in New York, Massachusetts, California, New Jersey, or Michigan; (2) the city is located in an adjacent state; (3) the city is located elsewhere. The two final measures were the percentages of cities in the same region that had already adopted the commission form and the percentages that had already adopted the managerial form. The four major census regions were used.

To measure hierarchical diffusion effects, the city's population in thousands of residents at each decade's census was available. These values

TABLE 1
SUMMARY OF MUNICIPAL ADMINISTRATIVE FORM CHANGES
BY DECADE FOR 267 CITIES

TYPE OF CHANGE	DECADE				TOTAL
	1900-9	1910-19	1920-29	1930-42	
Never changed.....	105	105	105	105	105
First change:					
MC to Comm.....	18	89	4	0	111
MC to Mgr.....	0	18	20	11	49
Comm. to Mgr.....	0	1	0	1	2
Second change:					
None.....	104	104	104	104	104
Comm. to Mgr.....	0	3	17	14	34
Comm. to MC.....	0	4	12	1	17
Mgr. to MC.....	0	0	3	5*	8*

NOTE.—MC = mayor-council, Comm. = commission, Mgr. = council-manager.

* Includes Trenton's third change.

were transformed to natural logarithms to reduce the extreme skewness created by very large cities like New York and Chicago.

Census reports provided decade measures of several social variables relevant to the three social forces perspectives on municipal innovation. Four culture-clash measures are (1) the percentage of city population foreign-born white, (2) the percentage black, (3) the percentage native-born white (100% minus the sum of the first two values), and (4) the percentage Catholic.

Class-conflict indicators are (1) the percentage of city population employed as wage earners in manufacturing industries, (2) average annual wages of manufacturing earners, (3) median years of schooling of adults aged 25 years or older (available only for 1940), and (4) the percentage of two-party presidential vote cast in the county for Democratic candidates, averaged over three elections, 1908–16 (data from Robinson [1934]).

Chosen as indicators of modernization forces were (1) city age, in number of years since the census date at which the population reached 10,000; (2) population growth, the percentage change during the preceding 10 years; (3) average number of employees per manufacturing plant, as an index of organizational complexity; and (4) logarithm of the number of national headquarters of voluntary associations located in the city in 1970, with founding dates prior to 1910.⁸

METHOD

A comparative quantitative study of municipal government change calls for analytic methods appropriate to the data. Event-history methods are ideally suited to categoric data referenced by time, since the techniques use all the information on number, timing, and sequence of changes between discrete states of the dependent variable (Tuma, Hannan, and Groeneveld 1979). Space permits only the sketchiest of descriptions, and readers are advised to consult the cited sources for more detail.

In an event-history analysis, the objective is to develop a model of the process that generates the events, in this case the changes to new types of municipal administration. Because such events can occur at any time, a continuous-time model is used, specifically, a finite-state, continuous-time Markov model. The instantaneous rate of transition from state j to state k

⁸ Based on a 40% sample of the 1970 *Gale Encyclopedia of Associations*. Turk (1977, pp. 40–42) argued that national headquarters of voluntary groups approximate the number and variety of all types of organizations, thus indexing a city's organizational complexity. The presence of such headquarters may also indicate integration into national communication networks, which exposes a city to information about municipal innovations and national norms. The measure used is in error to some unknown degree if the 1970 headquarters locations are not the same as those in 1910. Unfortunately, no contemporary directories of associations exist.

(e.g., from mayor-council to commission) at time t is defined as the limit, as Δt approaches zero, of the probability of a change from j to k between t and $t + \Delta t$, per unit of time:

$$r_{jk}(t) = \lim_{\Delta t \rightarrow 0} \frac{p_{jk}(t, t + \Delta t)}{\Delta t}, \quad (1)$$

where $p_{jk}(t, t + \Delta t)$ is an ordinary transition probability for change of state. The rate at which cities leave state j at time t is

$$r_j(t) = - \sum_{k \neq j} r_{jk}(t). \quad (2)$$

In general, the rate may depend not only on time but also on exogenous variables. Indeed, the main empirical interest in this paper is not to estimate the transition rates but to determine how the rates of reform adoption are affected by such factors as spatial location and city characteristics.

The functional form of the relationship between the rate and the exogenous (independent) variables used in this paper is a log-linear specification:

$$\ln r_{jk}(t) = a_0 + a_1 X_1(t') + \dots + a_n X_n(t'), \quad (3)$$

where the a 's are parameters showing the estimated impact of the independent variables (X 's) on the transition rate. The t' denotes the value of the X 's at the time that a city adopted the governmental form in which it is observed at time t . A positive value of an a parameter indicates the amount by which a unit of the independent variable increases the (logged) rate at which cities move into the new form of government per unit of time. Correspondingly, a negative value shows that the exogenous variable reduces the rate of transition to that form of administration. Thus, in its log-linear specification, a rate equation bears a strong resemblance to the more familiar linear-regression equation; readers who are unacquainted with event-history methodology may find the analogy helpful.

Unlike ordinary regression using cross-sectional data, however, the dependent value in a rate analysis is not directly observed. The transition rate is estimated from the data on the states occupied by *all* cities in every time interval—each year in the present study. In the analyses reported below, the transition rates into each of the three types of government (destination states) are estimated without regard to origin state.

Parameter estimation used a maximum-likelihood procedure, implemented in the computer program RATE developed by Tuma. Estimators from this procedure reportedly remain quite good even with small samples and a high degree of censoring, that is, when the mean number of tran-

sitions is low as in these data (Tuma and Hannan 1978). Since RATE provides estimates of standard errors of the parameters for the independent variables in an equation, F -ratios (or t -statistics) can be calculated to test for the statistical significance of the individual parameters. Also, the statistical significance of the set of variables in a model can be assessed (e.g., in order to determine the improvement when additional independent variables are entered into an equation) by the likelihood ratio χ^2 . Unfortunately, no measure of the overall goodness-of-fit of a model to the data, comparable to the proportion of variation explained in the dependent variable as expressed by the multiple R^2 in linear regression, has yet been developed, so there is no way to discern the predictive ability of a rate equation.

Initial model specifications to be discussed below assume a Markovian process with time homogeneity. The Markov property holds that only present state affects future transition, thereby allowing the pooling of all events for all cities, a total of 488 observations. Time homogeneity assumes that the rates of transition during a period under observation are constant, conditional on levels of exogenous variables at the time of entry into a state. Note that even when transition rates are assumed to be constant, the probability that a city will change governmental form declines increasingly as it adheres to a particular form; this fact is a major reason for modeling social processes in terms of transition rates rather than in terms of probabilities of change (see Tuma et al. 1979, p. 826). Cities were assigned the values of the independent variable that were closest to the start of a "spell" in a category of city government structure.⁹ Subsequent efforts to relax the time invariance assumption are reported below.

FINDINGS

The analytic strategy proceeds in three stages. First, the effect of each independent variable on the transition rates is reported without controlling for the effects of other variables. Next, multivariate equations are estimated using the best indicators of the theoretical perspectives. Both these analyses are based on data from the full four decades under investigation. Finally, for the transitions to commission and to council-manager forms, an attempt to estimate models for subperiods, which would allow comparison of early and late change processes, is described. This incremental strategy of data analysis permits a careful uncovering of the processes

⁹ Exceptions are the two indicators of the percentage of a city's regional neighbors which had already adopted either of the two reform structures. Values in the year preceding a city's change were used. For censored spells (when no change of form occurred), the regional percentages at the starting date were used. These measures are theoretically consistent with the neighborhood diffusion hypothesis.

underlying the spread of municipal reform and also allows comparison of these results with those from previous research based on cross-sectional data after the Second World War.

Bivariate Relationships

The first row in table 2 reports the maximum-likelihood estimates of the time-independent transition rates into each of the three municipal administrative structures. These coefficients show the estimated rate per year at which cities moved into each form of government during the entire four decades. Thus, cities adopted the commission form at an annual rate almost a third higher than the transition rate to council-manager form and more than four times the rate of readopting the mayor-council structure. These rates, of course, assume that no other factors influenced the innovation adoption process, a highly improbable situation. Since all except two cities began in the mayor-council form, the last column refers only to the rate at which cities abandoned a reform structure to return to the

TABLE 2
BIVARIATE EFFECTS ON TRANSITION RATES TO THREE
MUNICIPAL ADMINISTRATIVE STRUCTURES

INDEPENDENT VARIABLE	EFFECT ON TRANSITION RATE TO		
	Commission	Council-Manager	Mayor-Council
None.....	.0099	.0076	.0022
Neighborhood:			
Distance from origin.....	-.2814**	.0743
State innovator.....	.2980**	-.0163	-.0454
Regional percentage.....	.0088	.0586***
Hierarchy:			
Population (log).....	-.0266***	-.0218*	.0431**
Culture clash:			
Foreign-born white.....	-.0177*	-.0308**	.0153
Black.....	.0115	.0128	-.0422
Native white.....	-.0007	.0097	.0197
Catholic.....	-.0211**	-.0284***	.0244**
Class conflict:			
Workers.....	-.0528***	-.0218	.0416
Annual wages.....	-.0032***	-.0017**	.0017***
Education (1940).....	.0014	.0012	-.0000
Democratic vote (1908-16).....	.0082	.0149	-.0138
Modernization:			
City age.....	-.0176***	-.0105*	.0240***
Population growth.....	.0001	.0008	-.0016
Plant scale.....	-.0205***	-.0138**	.0093
Association headquarters (log).....	-.1411	-.0697	-.0084

* $P \leq .05$.

** $P \leq .01$.

*** $P \leq .001$.

traditional mayor-council form (as table 1 showed, only 25 such readoptions occurred throughout the entire four decades).

The other rows in table 2 examine the impact of each measured city characteristic on altering the homogeneous transition rates shown in the first row. These parameter estimates may be viewed as analogous to bivariate regression coefficients, in the sense that no other factors are statistically controlled in the same equation. If an independent variable has a significant impact on an estimated transition rate (using the *F*-ratio of a coefficient relative to its standard error), the assumption that cities were homogeneous in their propensity to adopt a reform structure must be rejected.

As expected, a city's geographic distance from Galveston was associated with a lower rate of change to the commission form, as revealed by the significant $-.2814$ coefficient. However, distance from Staunton had a negligible impact on the rate of transition to the council-manager form. (No comparable variable exists for readoptions of mayor-council government.) Location in or adjacent to an innovator state also affected the commission transition rate significantly, but the sign of the parameter was opposite to that expected. The $+.2980$ value means that cities *farthest* from the most innovative states were earlier adopters of commission government. Apparently, the commission form diffused most rapidly to cities located in states lacking a strong political culture of policy innovation and experimentation, particularly cities in the South.

Another neighborhood indicator, the percentage of regional cities which had already adopted the new structures, produced positive coefficients, as expected, although only the regional percentage of managerial adoptions was statistically significant in the bivariate analyses. These relationships are consistent with the hypothesis that cities took their cues about municipal change from their closer neighbors' reform experiences.

Turning to the hierarchical diffusion measure, we see a clear refutation of the hypothesis that larger communities tended to be earlier adopters. The negative parameter values are consistent, however, with previous cross-sectional data analyses that found the largest cities less likely to have reform government practices. The third column supports this finding, as larger cities were quicker to readopt the traditional mayor-council structure. Thus, apparently unlike other municipal innovations, the historical diffusion of municipal administration followed an inverse hierarchy.

The bivariate analyses of the four culture-clash indicators were also consistent with several cross-sectional comparative studies reviewed above. Cities with larger foreign-born white and larger Catholic populations were slower to adopt commission or managerial forms. And abandonments were significantly faster when larger Catholic percentages were present. The insignificant coefficients for the black and native white indicators clearly

support the contention that the ethnic-immigrant composition of early 20th-century American cities was a critical factor in retarding the spread of reform.

Among the class-conflict indicators, neither median education nor county Democratic vote had significant coefficients, and the percentage of manufacturing workers affected only the transition rate to the commission form. Although average annual workers' wages produced robust coefficients for all three types of change, the coefficient signs are opposite to those expected in the class-conflict perspective. The *poorer* communities were quicker to adopt both types of reform structure and were less prone to abandon them for a traditional mayor-council form. This event-history finding contradicts several cross-sectional studies that found communities higher in socioeconomic level tending more often to have reformed administrative structures. When viewed dynamically from a historical change perspective, however, structural reform appears to have been an attempt by communities with presumably fewer financial resources to cope with their municipal burdens. The subsequent multivariate analyses will show that this inverse relationship of wage levels and reform adoption persists when other variables are held constant.

Finally, the modernization indicators gave mixed results. Contrary to previous research using recent data, no evidence emerges here that population growth contributed to municipal reform adoptions early in this century. Similarly, the presence of national voluntary association headquarters did not affect any transition rate significantly (even when the exceptionally large concentrations in New York and Chicago were removed, the relationship remained nil). But city age and manufacturing-plant scale both performed as expected: younger cities and cities with smaller average plant sizes (i.e., presumably more organizationally complex communities) more rapidly adopted municipal reform structures, as shown by the significant negative coefficients. Apparently, the more established cities and those whose manufacturing sector was dominated by a few large plants were less able to break out of the traditional mold of 19th-century municipal governance.

In sum, these bivariate event-history analyses led to mixed conclusions for the hypotheses drawn from the distinct theoretical perspectives. Hierarchical diffusion from larger to smaller cities was clearly absent, although the observed inverse relationship was anticipated from other studies based on post-Second World War data. Neighborhood diffusion, as indexed by two regional percentage indicators, performed as expected. Among the three sets of social factors, ethnoreligious composition and modernization effects were found, but the class-conflict hypothesis was sharply contradicted by the negative coefficients for wage levels. Since several indepen-

dent variables are multicollinear, the next set of analyses assesses the net impact of representative variables on the municipal change process.¹⁰

Multivariate Analyses

Several independent variables are highly intercorrelated, for example, percentage Catholic with percentage foreign born, and percentage workers with plant scale. Obviously, an effort to assess the relative importance of theories of municipal reform cannot indiscriminately throw all indicators into one equation. Instead, the following procedure was used to determine the relative impact of the several explanatory perspectives described above.

From the dozen social forces measures, one indicator was selected to represent each of the three distinct perspectives: percentage Catholic, average annual wages, and city age. These three had the strongest and most consistent bivariate relationships with all three types of transition. Also, they were only moderately intercorrelated: across all 488 spells their r 's ranged from .01 (Catholic and wages) to .38 (Catholic and city age). Logged population and regional percentage measures represent hierarchy and neighborhood-diffusion processes, respectively, even though the bivariate relationships were not all consistent with initial expectations.

Table 3 shows the results from two multivariate equations predicting the transition rates to each of the three destination states. In the first equation of each pair, only the three social indicators were entered, while the second equation adds the two diffusion measures. Although, as noted above, an overall measure of fit of the equations to the data is not available, improvement of the second equation over the first can be assessed by the change in likelihood χ^2 values, and comparisons of parameter estimates reveal information about the net direct effect of independent variables on transition rates.

The multivariate equations for commission and for managerial transition rates are similar. With only the three social factors entered into the equa-

¹⁰ This report would be remiss not to address the question of regional differences in municipal reform. Wolfinger and Field's (1966) test of Banfield and Wilson's ethos theory asserted that the covariation between 1960 city-government structure and ethnic composition could be statistically "explained" by controlling for the regional location of cities. Gordon (1968), using data from the 1930s, showed in contrast that controls for region and other variables did not attenuate the association between nativity and governmental form. In the present analysis, if a four-category census region code (using just three dummy variables, to avoid linear dependency) is added to the multivariate equations in table 3, none of their coefficients achieve statistical significance, while the net effects of the other independent variables are largely unaltered. In short, gross categorization of cities' regional locations contributes nothing further to explaining transition rates. These results underscore Lineberry and Fowler's (1967) criticism that regional controls, because of their compositional collinearity with substantive explanatory variables, are generally theoretically indefensible in multivariate analyses.

TABLE 3
MULTIVARIATE EFFECTS ON TRANSITION RATES TO THREE MUNICIPAL ADMINISTRATIVE STRUCTURES

DESTINATION TYPE OF GOVERNMENT	INDEPENDENT VARIABLES						
	Constant	Catholic %	Annual Wages	City Age	Population (Log)	Regional %	Likelihood χ^2
Commission.....	-2.488***	-.0153*	-.0031***	-.0123**	53.62
Commission.....	-3.176***	-.0067	-.0039***	-.0222**	.0240	.0183***	65.60
Council-manager....	-3.384***	-.0263**	-.0017**	-.0038	26.54
Council-manager....	-4.220***	-.0078	-.0030***	-.0160	.0225	.0672***	112.04
Mayor-council.....	-8.340***	.0205	.0015**	.0182*	22.03
Mayor-council.....	-8.205***	.0201	.0015**	.0196	-.0049	...	22.07

* $P \leq .05$.

** $P \leq .01$.

*** $P \leq .001$.

tions, percentage Catholic and annual wages have significant net effects on both types of change, while city age too is significant for the commission transition. However, when the respective measures of regional percentages of adopters are added, the coefficient for percentage Catholic is reduced to insignificance. The inverse hierarchy effect, as indexed by city population, is not significant in either equation. Thus, after controlling for the effects of other variables, neither the culture-clash nor the hierarchical diffusion explanations remain important for the full four decades under investigation. And the net impact of modernization, as indicated by city age, is important only for the commission transition rate. Class conflict, in the form of the annual wages variable, remains strong in both multivariate equations, as does the neighborhood-diffusion effect captured in the regional percentages indicators. Note that regional percentages became statistically significant in the multivariate equation for commission transition, although in the bivariate analysis they were not significant, implying a suppressor effect.

The equation for mayor-council adoptions differs from the others most noticeably in the absence of any significant net impact of percentage Catholic. Both annual wages and city age have significant direct effects, but the only result of including population is to eliminate the marginal importance of city age without itself attaining significance. As adding population to the equation does not significantly change the χ^2 for the first equation, it can be judged to have no important direct effect on mayor-council re-adoption rate.

In sum, these multivariate analyses for the full period point to the simultaneous net impact of both class-conflict and neighborhood-diffusion processes. The assertion that culture clash played a direct role in municipal reform, particularly as argued by Gordon (1968), is emphatically rejected. Despite a sizable bivariate effect and a net effect when only the three social factors were included, controlling for regional neighborhood diffusion clearly eliminates percentage Catholic as a significant variable in both types of change. Before resting on that conclusion, however, the attempt to conduct subperiod investigations is reported.

Subperiod Analyses

The preceding multivariate equations assumed that the effects of independent variables on the transition rates were invariant over the four decades under study. This time-homogeneity assumption may be unrealistic if the municipal change process itself underwent a substantial change during this era. Event-history methods permit specification of another type of model, in which independent-variable effects differ among subperiods but remain constant within subperiods. Unfortunately, the theoretical literature on

municipal reform contains no advice on where to expect subperiod boundaries to occur, or, indeed, whether to expect any differences at all between "pioneers" and "laggards." Therefore, to examine an exploratory hypothesis that the parameters affecting early and late transitions varied over time, two dividing dates were chosen that correspond roughly to the peaks of the commission and the managerial adoption waves. (Too few transitions back to the mayor-council form occurred to warrant a similar attempt for that type.) The early period for commission changes was assumed to end in 1910, while the early period for council-manager changes was assumed to end in 1920. In both cases, the early period contained about one-third of the total changes in the four decades. (These proportions are not evident in table 1, which uses slightly different dates for classifying changes.)

Unfortunately, the resulting rate equations estimated within these four subperiods are not sufficiently credible to justify displaying the parameter estimates. None of the coefficients for the early period equation for managerial transitions was statistically significant, while the late-period equation basically resembled that estimated for the full period. The early and late period commission transition equations were almost mirror images: negative coefficients in one equation were matched by positive signs in the second equation and vice versa. For example, the Catholic percentage and the regional percentage coefficients were significantly negative for the early period but both significantly positive for the later period.

Although substantive interpretation of these results could be ventured, at this point none seem substantively convincing or theoretically informative. The most defensible conclusion is that serious data problems, in the form of collinearity among variables and too few transitions within the smaller intervals, severely constrained the capacity to conduct meaningful fine-grained event-history analyses of municipal change. Although requiring an assumption that the process itself was unchanging, the results from the analyses of transitions across the entire four decades must stand as the most comprehensible models of the spread of municipal administrative reforms in the early part of this century.

DISCUSSION

The municipal reform movements early in this century linked social innovations to political ideologies. Although their appearance may reflect a unique conjunction of national trends in urbanization, industrialization, and professionalization, the adoption of reform structures among the 267 largest American cities was neither uniform nor random. The analyses reported in this paper are the first comparative quantitative investigations of the spread of municipal reform structures to use data from the era of

change. The application of event-history methods to these data yielded findings that call for modification and revision of prevailing images of municipal reform.

In the conventional image, the reform movements were launched and directed by the native upper and middle classes and were resisted by the working-class and immigrant populations, whose interests were better served by a traditional mayor-council structure with ward-based representation. The comparative event-history analyses challenge this interpretation, which derives from case studies and cross-sectional data analyzed with nondynamic methods. Gordon's (1968) assertion that nativity effects remain strong despite controls for other social factors is clearly refuted. The significance of percentage Catholic, while apparent in bivariate relation to reform transition rates, disappeared from the equations when percentage Catholic was controlled for other independent variables.

Similarly, the prevalent class-conflict explanation also fared poorly. To cite a well-known case study, Oakland's commercial and corporate business interests twice intervened to revise the city charter, first in 1911 to adopt a commission form in the face of an impending Socialist electoral success, and again in 1928 to reduce the tax burden by adopting a council-manager form (Hayes 1972, pp. 10–14). Although there is no basis for questioning this particular example, the comparative event-history results suggest that a different pattern prevailed in the nation as a whole. The negative parameters for annual manufacturing wages suggest that the *least* affluent communities, where working-class interests were presumably stronger, were the most rapid acceptors of both commission and managerial government. Granted that the reform ideology may have been invented and introduced by community elites, the results of event-history analysis imply that it spread most rapidly among communities whose limited economic resources probably made such structural innovations attractive as a possible solution to urban ills.

Reconciling the present findings with earlier research is difficult. The source of the differences is obscured by differences in both methods and types of data.¹¹ Both case histories and cross-sectional data suffer from limited scope and potential idiosyncrasies of interpretation. The event-history approach is not free from problems—particularly the unavailability of certain desired indicators such as union membership and city-level voting data—but it has the decided advantage of being able to model the change process as it unfolded across many cities. When cross-sectional analyses of 1960s data find that middle-class cities are more likely to be

¹¹ Hannan and Carroll (1981) demonstrated that static cross-sectional methods and event-history methods applied to the same nation-state data generated substantively different conclusions about significant effects. They argued, of course, for the superiority of the latter techniques, a theme echoed in the present paper.

reformed, it is impossible to determine whether class structure was a cause of political change or its consequence. But, when an event-history analysis using data from the reform era finds that low-wage cities are more prone to early reform, the causal ordering is not in doubt.

Among the theories of reform discussed at the beginning, the spatial diffusion model held up well. A major finding of the event-history analyses was the importance of neighborhood effects in the spread of municipal reform. The results give substantive content to previous research that used gross regional categories to uncover geographic differences. The present study specifies these regional differences as arising not from social compositional differences of regions' cities but from some type of imitation or contagion effect as represented by the level of neighboring regional cities previously adopting reform government.

Further details of the diffusion process remain to be uncovered. Contagion may have been most rapid among particular types of cities within a region and may have spread to other regions through imitation of administrative change by similar cities in new areas before diffusion was completed within the origin area. The characteristics disposing some cities to imitation of the pioneers remain unclear. But administrative structural change was shown not to be related to population-size hierarchy, however important that status dimension may be for other municipal innovations (Agnew et al. 1978). Walker's (1969) speculations about regional reference groups and nonnational evaluation standards are promising concepts to be pursued. More direct measures of interurban contacts and communication networks, though difficult to operationalize for historical eras, may hold the key to understanding how urban innovations spread during an era when national mass media were rudimentary at best. An intensive investigation of the professionalization of municipal government would appear to be another promising direction for subsequent research.

Much work remains to be done on the social organization of municipal change. The development and popularization of new methods for modeling dynamic processes should promote inquiries into such historical topics as the development of urban planning departments, the reform of civil service, the unionization of police and firemen, the dismantling of urban political machines, and the differentiation of municipal service functions. Much of this research must be preceded by an arduous collection of data long buried in municipal archives and newspaper morgues. The rewards of this enterprise will be immense and well worth the effort.

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The Impact of Fictional Television Stories on Adult Fatalities: New Evidence on the Role of the Mass Media on Violence¹

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This paper presents the first systematic evidence that television stories trigger imitative deaths and injuries in the United States. In 1977, suicides, motor vehicle fatal accidents all rose immediately following television stories. The U.S. female suicides increased proportionally to male suicides. Single-vehicle crashes increased proportionally to all vehicle crashes. All of these increases are statistically significant after one corrects for the presence of television stories, linear trends, seasonal fluctuations, and other variations in the data. These increases apparently indicate that opera suicide stories trigger imitative suicides a significant portion of which are disguised as single-vehicle accidents.

For a generation researchers have sought to determine whether television triggers imitative aggression in viewers. Research on this topic consists of several thousand studies. The literature is summarized ably in a three-volume review of the literature (Comstock and Fisher 1975; Comstock and Lindsey 1977), a bibliography (Gordon and Verna 1978), the Surgeon General's Advisory Committee on Television and Social Behavior's recent reviews (Liebert and Schwartzberg 1977; Comstock and Hearold 1979; Wilhoit and de Bock 1980; Comstock and Bachen 1981).

The great bulk of these studies are laboratory experiments (Comstock 1975, p. 27). Nearly all researchers agree that television triggers aggression *in the laboratory*, but researchers are uncertain about its effects in the real world.

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in the laboratory can be generalized to *the real world*. This uncertainty arises for at least five reasons: (1) Behavior in the artificial setting of the laboratory might be quite different from behavior in real life (Comstock 1980, p. 102; Singer 1971, p. 47). (2) Almost all the literature is confined to the effect of television violence on *children and youths*, despite the fact that the great bulk of television viewers are *adults* (Comstock 1975, p. 27; Singer 1971; Kaplan and Singer 1976; Comstock et al. 1978, p. 413). (3) The aggressive, violent reactions studied by the experimenter (e.g., hitting a doll or administering electric shocks) may not correspond closely with aggressive, violent reactions prevailing in the real world (e.g., rape, murder). (4) The mass media violence excerpted (and often edited) for display in the laboratory may not correspond closely with the actual mass-media violence one is exposed to in the real world (Comstock 1975, p. 30). (5) The samples studied are not chosen randomly. Thus, one cannot necessarily generalize from behavior in the sample to behavior in the population from which the sample was drawn (Comstock 1975, p. 31).

Because of these difficulties, many researchers (Singer 1971; Kaplan and Singer 1976; Andison 1980; Comstock 1980) have felt that laboratory studies do not necessarily imply that television violence triggers violence in the real world. On the other hand, despite these difficulties, other well-known researchers (e.g., Bandura 1973; Berkowitz 1962; Liebert, Neale, and Davidson 1973) feel that it is possible to conclude from laboratory studies that violence in the mass media increases the *probability* of aggression in the real world. Comstock (1975, pp. 30-40) provides a very helpful and illuminating summary of the debate on this topic. In sum, there is consensus that laboratory studies show the effect of television violence in the laboratory, but there is no consensus that they show its effect in the real world.

Some researchers, seeking to avoid the artificiality of the laboratory, have turned to such naturalistic studies of television violence as field experiments and panel studies (Milavsky and Pekowsky 1973; Milgram and Shotland 1973; Eron et al. 1974; Parke et al. 1977; Belson 1978). Unfortunately, these studies lack the rigor of laboratory investigations. As Comstock et al. noted, "The more naturalistic circumstances in which data are collected in the field experiment and the survey . . . solve the artificiality problem but at the price of flexibility and confidence in drawing causal inferences" (1978, p. 492).

In short, one cannot conclude with confidence, either from laboratory experiments or from naturalistic studies,² that television violence triggers seriously harmful violence in the population at large. After reviewing the

² There is also nonsystematic, anecdotal evidence (reviewed by Comstock 1980, p. 109) that television stories trigger imitative violence, but this type of evidence is of course not conclusive.

literature, Comstock concluded that "it cannot be said with certainty whether the contribution of television violence to seriously harmful anti-social behavior is great, negligible, or null" (1980, p. 138). Andison concurred in another review: "What we do not know is whether . . . there are deaths and violence occurring in society today because of what is being shown on the TV screen" (1980, p. 564).

I now present what I believe to be the first systematic, quantitative evidence that fictional violence on television triggers fatal imitative violence in the U.S. population as a whole. The implications of the findings will be discussed after presentation of the data.

STUDY DESIGN

The procedure to be followed builds on earlier work (Phillips 1974, 1977, 1979; Bollen and Phillips 1981) which showed: (1) after nonfictional newspaper suicide stories, suicides and motor vehicle deaths increased significantly; (2) the greater the publicity given to the suicide story, the greater the increase in suicides and motor vehicle deaths; (3) the increase in suicides and motor vehicle deaths occurred mainly in the geographic area where the suicide story was publicized; (4) single-car deaths increased more than multiple-car deaths; (5) the person described in the suicide story was similar to *drivers* who crashed just afterward but not similar to *passengers*; (6) the findings are statistically significant and persist after one corrects for seasonal fluctuations, day-of-the-week fluctuations, and linear trends over time. The findings suggest that newspaper suicide stories trigger imitative suicides, some of which are disguised as automobile accidents.

The above-mentioned studies concern the impact of *nonfictional newspaper* suicide stories. It is natural to ask whether *fictional television* suicide stories also trigger imitative behavior. This question will be addressed in the present study. Related questions on the impact of other types of television story will be discussed at the end of this paper.

In order to determine whether fictional television suicide stories are imitated by the populace at large, one needs to solve the following three problems.

Locating a Source of Fictional TV Suicide Stories

There is no convenient way to derive an exhaustive list of all fictional suicide stories occurring on television programs. Such a list can be derived easily for only one type of television program—soap operas. Weekly summaries of these programs are provided in a newspaper column, "The Soaps," written by Jon Michael Reed and published in the *Los Angeles Times* and other newspapers. These plot summaries identify the week in which a soap

opera suicide occurs, but not the specific day of its occurrence.⁸ Television soap operas appear daily, Monday through Friday, and attract an enormous, devoted, mainly female audience which often identifies strongly with the soap opera characters. (For a review of the sparse empirical literature on this topic, see Tucker [1977] and Buerkel-Rothfuss with Mayes [1981].) Katzman estimated that "roughly 40 percent of all homes with televisions are tuned to at least one [soap opera] serial in a given week" (1972, p. 205). Tucker cites Nielsen figures indicating that soap operas are the dominant force in daytime television, attracting a larger audience than competing formats (1977, table 3). There is much anecdotal evidence that many viewers are emotionally involved with the soap opera characters they watch. According to Edmondson and Rounds, "Much of the mail that the producers receive speaks of the characters as real. Thousands and thousands of letters give advice, warn the heroine of impending doom, caution the innocent to beware . . . , inform one character of another's doings, or reprimand a character for unseemly behavior" (1976, p. 193). Tucker cited a report that television viewers sent approximately one-quarter of a million birthday cards when a character on *As the World Turns* turned 65 (1977, p. 8).

Because they attract large, devoted audiences, soap operas may have the capacity to influence the attitudes and behavior of many people. As Katzman has noted, "The almost-realism of the characters and themes, the repetition due to slow pace, and the extremely large number of hours spent viewing soap operas indicate that these shows have great potential power. They can establish or reinforce value systems. They can suggest how people should act in certain situations. They can legitimate behavior and remove taboos . . ." (1972, p. 212). These considerations suggest that soap operas can serve as a promising research site for determining whether the suicides of fictional characters trigger imitative behavior in the viewing public.

Locating a Source of U.S. Daily Mortality Statistics

The National Center for Health Statistics provided me with computerized death-certificate records of all U.S. suicides and motor vehicle deaths for 1977, which at the time of writing was the latest year for which such records were available. Analysis of these data will be restricted to persons described as "white" on their death certificates. This procedure was followed because the vast majority of soap opera characters are white, and it

⁸ On very rare occasions, the *Los Angeles Times* plot summaries omit mention of one soap opera or another. Thus, it is conceivable that some soap opera suicide stories in 1977 have not been examined in this paper. The period, 1977, was chosen for study because it is the first full year for which weekly soap opera summaries are available. At the time of writing, it was also the latest year for which U.S. computerized death-certificate records were available.

seems plausible that white viewers would be particularly likely to identify with the characters they see.

Developing Procedures for Assessing the Impact of TV Stories

In accordance with previous practice (Phillips 1979), a one-week "experimental period" will be used; this will be defined as the week, Monday-Sunday, in which a television soap opera suicide or suicide attempt is known to occur. If television soap opera suicide stories trigger a rise in suicides and motor vehicle deaths, the number of suicides and motor vehicle deaths in the experimental period should generally exceed the number of such deaths in a matched control period.

This prediction can be sharpened if we take into account the findings of previous research (Phillips 1977, 1979) which showed that deaths rise markedly on the third day after a nonfictional suicide story and relatively little on other days in the experimental week. This evidence suggests that there is a three-day lag in the imitative response to publicized suicide stories. If deaths fluctuate after soap opera suicide stories in the way they do after nonfictional suicide stories, U.S. suicides should increase three days after the soap opera suicide story. As indicated earlier, we do not know the particular day on which a soap opera suicide occurred; however, the earliest possible day of occurrence is the first day of the week, or Monday. Thus, the earliest a third-day peak in deaths can occur is Thursday. Therefore we expect deaths to rise strongly in the second half of the experimental week (Thursday through Sunday) but weakly or not at all in the first half (Monday through Wednesday).

Henceforth the period Thursday through Sunday in the experimental week will be called "the late experimental period," and the period Monday through Wednesday in the experimental week will be called "the early experimental period." The terms "late control period" and "early control period" are defined in a parallel fashion.

Later in this paper we will see that television soap opera suicide stories appear to trigger a rise in U.S. suicides. In order to demonstrate this triggering effect we need to develop unbiased procedures for comparing the number of suicides during the experimental period with the number during the control period. These procedures are described in the next three subsections.

Correcting for the effect of public holidays on suicide.—Phillips and Liu (1980) showed that the level of U.S. suicides fluctuates significantly around five public holidays: New Year's Day, Independence Day, Labor Day, Thanksgiving, and Christmas. Consequently, if a soap opera suicide story occurs near any of these holidays, the effect of the soap opera suicide story and the effect of the holiday are confounded. For this reason I have deleted

from my sample those soap opera suicides whose experimental periods overlapped with the above-mentioned five holidays.⁴

Ordinarily, the control period falls one week before the experimental period. This general procedure must be modified occasionally to correct for the effect of public holidays. If a control period overlaps with one of these five holidays, it is replaced by the nearest available control period which occurred prior to the holiday and did not overlap with it.

Correcting for the effect of nonfictional suicide stories.—It is necessary to avoid confounding the effects of nonfictional and fictional suicide stories. Fortunately, there was only one widely publicized nonfictional suicide story in the United States in 1977—the suicide of Freddie Prinze on January 29.⁵ For about a week Prinze's suicide received heavy publicity from daily and weekly newspapers and news magazines. Prinze's suicide occurred approximately two and a half weeks before the first television soap opera suicide story, which took place during the week of February 14–20. The control period for this story would have been February 7–13, the week before the experimental period. However, because Prinze's suicide story might possibly affect suicides during this control period, an earlier control period was substituted—January 17–23. This period falls before Prinze committed suicide and thus cannot have been affected by the news of his death.

The foregoing procedure for selecting experimental and control periods corrects for (1) the effect of public holidays known to influence the suicide rate and (2) the effect of nonfictional suicide stories. In addition, the procedure corrects for (3) day-of-the-week fluctuations in suicide, because the control period contains the same days of the week as the experimental period. And it corrects for (4) seasonal effects, because the experimental period and its associated control both occur at about the same time of the year.

Correcting for linear trends in deaths.—If there were no trend in suicides over time, the number of dead in the control period would be an unbiased estimate of the number of dead expected in the experimental period, given the null hypothesis. However, because there is in fact a trend in suicides

⁴ Phillips and Liu (1980) showed that public holidays affect significantly the level of suicide for three days on either side of the holiday. For this reason, a control period is considered to "overlap" with a holiday if any day in that period occurs within three days of a holiday. A similar procedure is used to determine if an experimental period overlaps with a holiday.

⁵ A much publicized nonfictional suicide story was defined as a story appearing simultaneously on the front page of the *Los Angeles Times*, the *Chicago Tribune*, and the *New York Times*. Such stories were sought in the following manner. The index heading "suicide" was examined in the *Los Angeles Times* reference library (morgue) to determine which suicide stories appeared on the front page of that newspaper. Then a search was made to determine which of these stories also appeared on the front page of the remaining newspapers. The *Los Angeles Times* morgue index revealed only one front page suicide story, and this story was found to appear also on page 1 of the *Chicago Tribune* and the *New York Times*.

over time, this simple estimation procedure is biased and must be modified, for the following reasons.

The experimental period always occurs after its associated control period. If suicides are increasing gradually throughout the year, the number in the experimental period typically will exceed the number in the control period, *even if soap opera suicide stories have no triggering effect on U.S. suicides*. Hence, in the presence of trend, the number of deaths in the control period is a biased estimate of the number expected in the experimental period, given the null hypothesis.

An unbiased estimate, which is corrected for trend, can be constructed as follows. The number of suicides to be expected on (say) Thursday of the experimental period equals the number on Thursday of the control period, plus the increment due to trend. This increment can be calculated in the following way. Plot the number of suicides each day in 1977, the period under study. Calculate the slope, b , of the linear regression line that best fits these data. For example, suppose that the control period occurs one week prior to the experimental period, and suppose that the value of b is $1/7$ or .143. This figure indicates that suicides are increasing by $1/7$ each day—an increment of one suicide per week. Thus, given the null hypothesis, the number of suicides to be expected on Thursday of the experimental period equals the number of suicides on Thursday of the control period, plus one, the weekly increment due to trend in this particular example.

By similar calculations one can determine the number of suicides to be expected on Friday of the experimental period (= the number of deaths on Friday of the control period + 1), the number of deaths on Saturday of the experimental period, and the number of deaths on Sunday of the experimental period. The total number of deaths to be expected in the late experimental period (Thursday through Sunday) = the number of deaths in the late control period + 1 (the increment for Thursday) + 1 (the increment for Friday) + 1 (the increment for Saturday) + 1 (the increment for Sunday).⁶

We have now discussed the procedures needed to ensure an unbiased test of the hypothesis that television soap opera suicide stories trigger imitative behavior. The next section presents evidence supporting this hypothesis.

⁶ In this example, the control period occurs seven days prior to the experimental period, and thus the increment due to trend is calculated as $7 \times b$. As noted earlier, the control period sometimes occurs more than seven days prior to the experimental period (e.g., to avoid the confounding effect of a holiday). In this situation, when the control period falls, say, Y days prior to the experimental period, the increment due to trend is calculated as $Y \times b$ (rather than as $7 \times b$). The present study could not employ earlier methods of correcting for trend (Phillips 1974, 1979). These earlier methods were developed for a different type of data set, which (1) covered several years and (2) contained relatively few stories per year. Neither of these conditions holds for the data set examined in the present study.

PRESENTATION OF THE DATA

Findings for Suicides

Table 1 presents a list of 13 soap opera suicide stories and their approximate dates. This is an exhaustive list of all 1977 suicide stories that could be selected according to the procedures described in the previous section.⁷ The data in table 1 allow us to assess the plausibility of the null and alternative hypotheses. The alternative hypothesis states that television soap opera suicide stories trigger additional suicides; hence there should be an abnormally large number of suicides in the late experimental period (Thursday through Sunday in the week of a soap opera suicide story). In contrast, the null hypothesis states that soap opera suicide stories do not trigger additional suicides.

Given the null hypothesis, the number of suicides in the control period serves as an estimate of the number of suicides to be expected in the experi-

TABLE 1
U.S. SUICIDES BEFORE AND AFTER SOAP OPERA (SO) SUICIDE STORIES

Person Attempting or Committing Suicide in SO	Deaths in Late Experimental Period (N) (1)	Deaths in Late Control Period (N) (2)	Adjustment Required to Correct for Trend (3)	Adjusted Deaths in Late Control Period (N) (4)	Rise in Suicides after SO Suicide Story (Col. 1- Col. 4) (5)
M. Jeeter.....	299	256	-1.828	254.172	44.828
H. Grant.....	297	307	-.686	275.314	31.686
D. Ryan.....	317				
S. Duval.....	310	301	-.457	300.543	9.457
Dr. Namath*.....	320	293	-.686	292.314	3.686
P. Summers.....	272				
A. Martin.....	301	279	-.457	278.543	22.457
M. Dante.....	254	267	-.457	266.543	-12.543
N. Vernon.....	295	277	-1.371	275.629	19.371
R. Becker.....	286	290	-.914	289.086	6.914
E. Conrad.....	313				
"Meredith"*.....	289				
L. Dallas.....	262	261	-.457	260.543	1.457

NOTE.—All figures are for U.S. white suicides, 1977. Soap opera stories occurring in adjacent weeks (for example, H. Grant and D. Ryan) share the same control period and are treated as one story to avoid problems of statistical dependence. The figure, 307, in the joint experimental period for Grant and Ryan is the average of the number of deaths in the experimental period for Grant (297) and the number of deaths in the experimental period for Ryan (317). A similar procedure has been used with the other soap opera stories occurring in adjacent weeks. The dates of the late experimental periods associated with each story are as follows: Jeeter (Feb. 17-20); Grant (Mar. 3-6); Ryan (Mar. 10-13); Duval (Mar. 31-Apr. 3); Namath (Apr. 14-17); Summers (Apr. 21-24); Martin (May 12-15); Dante (May 26-29); Vernon (July 14-17); Becker (Oct. 13-16); Conrad (Oct. 20-23); "Meredith" (Oct. 27-30); Dallas (Nov. 17-20). The dates of the late control periods are Jan. 20-23; Feb. 24-27; Mar. 24-27; Apr. 7-10; May 5-8; May 19-22; June 23-26; Oct. 6-9; Nov. 10-13.

* The full name of this soap opera character is not evident from the *Los Angeles Times* summaries.

⁷ As noted previously, it was necessary to omit some stories because their experimental periods overlapped with public holidays.

mental period, after one adjusts the control period figure for trend. If the observed number of suicides is significantly greater than the number expected, the null hypothesis can be rejected in favor of the alternative.

Column 1 of table 1 gives the number of suicides observed in the late experimental period; column 2 gives the number of suicides observed in the late control period; column 3 gives the amount by which the control period figure must be adjusted to correct for trend. (Because suicides were tending downward in 1977, these adjustment figures are negative.) Column 4 gives the estimate, corrected for trend, of the number of suicides to be expected in the late experimental period.

The observed number of suicides in the late experimental period (col. 1) nearly always exceeds the number of suicides expected under the null hypothesis (col. 4). The difference between the figures in column 1 and column 4 is statistically significant at .020 ($t = 2.449$, t -test for matched pairs; 8 df, one-tailed test; see Hays [1963] for a discussion of this standard test). Hence, U.S. white suicides increase significantly on and just after the dates of soap opera suicide stories.

This increase cannot be ascribed plausibly to (1) day-of-the-week fluctuations in suicide, (2) seasonal fluctuations, (3) the effects of public holidays, (4) the effects of nonfictional suicide stories, or (5) the presence of a linear trend in suicides because all of these factors were corrected for in the selection and treatment of experimental periods and their associated controls.

In addition, the increase in U.S. suicides cannot be considered plausibly an artifact of the method of statistical analysis because other methods of analysis also yield statistically significant results. For example, one might prefer to substitute the randomization test for matched pairs (Siegel 1956) for the t -test because assumptions required by the t -test may not be met by the data. When the randomization test is applied to the data set above, a significance level of .02 is found. Other testing procedures yield similar results.⁸

There is another reason for believing that the increase in suicides in the late experimental period is not a statistical artifact. If these results were an artifact of the techniques used to analyze them, the same techniques

⁸ Alternatively, one might see no need to correct for trend both because it complicates the analysis and because it happens to increase the statistical significance of the findings (in this particular instance). If one chooses not to correct for trend, and performs a t -test for matched pairs on the data in cols. 1 and 2 (rather than cols. 1 and 4), one finds a significant t -value of 2.352 ($P = .023$; 8 df, one-tailed test). When the randomization test is applied to these data, $P = .02$. Alternatively, one might wish to calculate the statistic (col. 1 - col. 2)/col. 2 for each experimental period, thus calculating a figure for the *proportional* increase in suicides in each experimental period. When a t -test is applied to these proportional increases, one finds a t -value of 2.264 ($P = .027$, 8 df).

should produce a statistically significant increase during the *early* experimental period as well. However, no such increase is found.⁹

In sum, the best available explanation of the findings in table 1 is that television soap opera suicide stories trigger increases in U.S. white suicides. The next section shows that white fatalities from motor vehicle crashes also increase significantly in the late experimental period. In addition, we will see that *single*-vehicle deaths increase much more than *multiple*-vehicle deaths do. These findings would be expected if soap opera suicide stories trigger some suicides which are disguised as motor vehicle accidents.

Findings for Motor Vehicle Fatalities

Examination of recorded suicide rates (U.S. Public Health Service, yearly volumes) reveals that suicide is an adult phenomenon, starting at about age 15. For this reason, in research on suicidal motor vehicle deaths (Phillips 1979, pp. 1160–71) I concentrated attention on those aged 15 and over. This practice will be followed in the present paper, and for convenience these deaths will be termed “adult motor vehicle deaths.”

Correcting for bias in comparison of experimental and control periods.—The methods for correcting bias will be in general the same as the methods used in the foregoing analysis of suicides. However, for one specific type of bias (the effects of public holidays) a minor modification will be necessary.

The analysis of suicides required correction for the effects of five public holidays known to influence suicides: New Year's Day, Independence Day, Labor Day, Thanksgiving, and Christmas. In the analysis of automobile accidents, one additional holiday—Memorial Day—needs to be controlled for. Although this holiday is not known to influence suicide, it is known to influence automobile deaths. Consequently, the confounding effects of Memorial Day must be controlled for in the analysis of motor vehicle deaths.

The eighth story in table 1 (concerning Mary Ellen Dante) has an experimental period which overlaps with Memorial Day.¹⁰ Hence, this story must be omitted in the analysis of motor vehicle statistics. (For the sake of symmetry, one might also wish to omit this story in the analysis of suicide statistics. If this is done, and the eighth story omitted from table 1, the results in that table remain statistically significant: $t = 3.269$, $P = .007$).

In short, one additional holiday is corrected for in the analysis of motor vehicle statistics. Aside from this modification, the procedures for analyzing motor vehicle deaths and suicides are identical.

Findings for motor vehicle deaths.—Table 2 displays the number of

⁹ When the t -test for matched pairs, corrected for trend, is applied to the early experimental and control periods, one finds a t -value of .311; $P = .382$.

¹⁰ The term “overlapped” is defined here as in n. 4 above.

TABLE 2

U.S. MOTOR VEHICLE FATALITIES BEFORE AND AFTER SOAP OPERA (SO)
SUICIDE STORIES

Person Attempting or Committing Suicide in SO	Deaths in Late Experimental Period (N) (1)	Deaths in Late Control Period (N) (2)	Adjustment Required to Correct for Trend (3)	Adjusted Deaths in Late Control Period (N) (4)	Rise in Deaths after SO Suicide Story (Col. 1- Col. 4) (5)
M. Jeeter.....	367	280	12.757	292.757	74.243
H. Grant.....	431	392	4.784	396.784	34.216
D. Ryan.....	431				
S. Duval.....	454	442	3.189	445.189	8.811
Dr. Namath.....	461	447	4.784	451.784	.216
P. Summers.....	443				
A. Martin.....	458	472	3.189	475.189	-17.189
N. Vernon.....	568	534	9.568	543.568	24.432
R. Becker.....	555	526	6.379	532.379	8.954
E. Conrad.....	499				
"Meredith".....	570				
L. Dallas.....	554	523	3.189	526.189	27.811

NOTE.—All figures are for U.S. motor vehicle fatalities among whites aged 15 and over, 1977. For dates of late experimental and late control periods, see footnote to table 1.

motor vehicle deaths before and after television soap opera suicide stories. The format of this table is identical with that of table 1. Column 1 gives the number of white adult motor vehicle deaths in the late experimental period; column 2 gives the number of such deaths in the late control period; column 3 gives the amount by which each control period figure must be adjusted to correct for trend. (Because motor vehicle traffic deaths were increasing in 1977, these adjustment figures are positive.) Column 4 gives the estimate, corrected for trend, of the number of white adult motor vehicle deaths to be expected in the late experimental period under the null hypothesis.

The observed number of white adult motor vehicle deaths in the late experimental period (column 1) generally exceeds the number to be expected under the null hypothesis (column 4). The difference between the figures in columns 1 and 4 is statistically significant ($t = 2.086$, $P = .038$; 7 df, one-tailed t -test for matched pairs). As in the analysis of table 1, one might prefer to replace the t -test with the randomization test for matched pairs, because this test requires fewer assumptions about the data. When the randomization test is used, the results are once again statistically significant ($P = .03$).

In sum, white adult motor vehicle deaths increase significantly on and just after the dates of soap opera suicide stories. For reasons already given in the analysis of table 1, the results cannot be ascribed plausibly to (1)

day-of-the-week fluctuations in motor vehicle deaths, (2) seasonal fluctuations, (3) the effects of public holidays, (4) the effects of nonfictional suicide stories, or (5) a linear trend in motor vehicle deaths. Furthermore, the results cannot be plausibly considered a statistical artifact; if the results *were* an artifact, one would expect to find a significant increase in motor vehicle deaths in the *early* experimental period as well. But no such increase is found. (For the early experimental and control period data, $t = -.657$, $P = .266$; one-tailed test.)

Thus, the best available explanation of the findings in table 2 is that television soap opera suicide stories lead to an increase in U.S. suicides, some of which are disguised as motor vehicle traffic accidents.

Findings for different types of motor vehicle deaths.—Motor vehicle traffic accident deaths (coded E810–E819 in the *Eighth Revision of the International Classification of Diseases*) are divided into four major categories: single-vehicle crash deaths (E815–E818),¹¹ multiple-vehicle crash deaths (E810–E813), pedestrian crash deaths (E814), and a residual category consisting of crashes of unspecified or unknown type (E819).

Researchers have long believed that the first category (single-vehicle crash deaths) is particularly likely to contain misclassified suicides. (For a review of the literature on this topic, see Phillips [1979].) In contrast, multiple-vehicle crash deaths are not believed to have a large suicidal component. If these beliefs are correct, multiple-vehicle deaths should increase very little after soap opera suicide stories, while single-vehicle deaths should increase by much more.

These beliefs are supported by further analysis of the data in table 2. Single-vehicle crash deaths rise by 7.09% after soap opera suicide stories, whereas multiple-vehicle deaths do not rise at all. In fact, they actually fall slightly, by .29%.¹² This evidence provides additional support for the prevailing belief that single-vehicle deaths are the most promising research site for studies of suicidal motor vehicle crashes. In the next section I will examine single-vehicle crashes that do *not* result in deaths.

Findings for Nonfatal Motor Vehicle Accidents

Presumably, some persons attempting suicide in an automobile will not succeed in killing themselves but will only injure themselves severely. If this is so, there should be an increase not only in fatal crashes but also in non-

¹¹ Phillips (1979) was wrong, though only in a minor way, in taking E815–E816 to comprise all single-vehicle accidents. Though these categories comprise the great bulk of all single-vehicle accidents, there are a few single-vehicle fatalities in E817–E818 as well. For more detailed information on the *Eighth Revision of the International Classification of Diseases*, see World Health Organization (1967).

¹² The increase for pedestrian fatalities (3.72%) is intermediate between the increases for single- and multiple-vehicle fatalities.

fatal crashes at and just after the time of suicide stories. This hypothesis will be tested in the next subsections, after a brief discussion of the data sources available.

Source of information on nonfatal motor vehicle crashes.—Unfortunately, daily nonfatal motor vehicle statistics do not seem to be available for the entire United States. However, such data are available for California, and the following analysis will be restricted to that state. The California Highway Patrol has provided me with unpublished daily tables of nonfatal motor vehicle accidents, classified according to three degrees of severity: serious accidents, in which the worst injury involved is a serious wound; moderately serious accidents, in which the worst injury involved is a visible injury which is not a severe wound; and minor accidents, in which there is a "complaint of pain" but no visible injury. These three categories of accident are each divided into two subcategories: single-vehicle accidents and remaining types of accident.¹⁸

Findings for nonfatal motor vehicle crashes.—Some of the accident categories just mentioned are particularly likely to contain misclassified suicides, and auto accidents in these categories should increase more than others just after soap opera suicide stories. For example, single-vehicle accidents probably have a larger suicidal component than other types of accident and therefore should increase more than other types. Indeed, judging from the earlier findings in this paper, one would not be surprised to find that nonsingle-vehicle accidents do not increase at all. Within the category of single-vehicle accidents, serious accidents probably have a greater suicidal component than minor accidents do. If this is so, serious single-vehicle accidents should increase more than minor single-vehicle accidents.

These predictions can be tested with the information in table 3, which

TABLE 3
INCREASE IN NONFATAL MOTOR VEHICLE
ACCIDENTS AFTER SOAP OPERA SUICIDE
STORIES, BY TYPE (%)

	Single- Vehicle	All Other
Serious.....	14.13	1.78
Moderately serious.....	7.52	1.82
Minor.....	-.97	-3.56

NOTE.—For definition of the terms "serious," "moderately serious," and "minor," see text. All data are for California, 1977.

¹⁸ One should bear in mind that the data on fatalities and on nonfatal accidents are not strictly comparable. The fatality data consist of white, adult, U.S. deaths. The nonfatal accident data consist of California accidents for all races and age groups combined. In addition, the decedent is the unit of analysis in the U.S. data set, while the *crash* is the unit of analysis in the California data set.

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indicates the amount by which each type of accident increases after soap opera suicide stories. The findings in this table are consistent with our earlier expectations. (1) Single-vehicle accidents do indeed increase more than other types (in fact other types of accident do not increase at all). (2) Within the category of single-vehicle accidents, serious accidents increase most (by 14.13%), moderately serious accidents increase less (by 7.52%), and minor accidents decrease slightly (by —.97%).

Of the six accident categories in table 3, only two produce statistically significant increases: serious single-vehicle accidents and moderately serious single-vehicle accidents. These accident categories are examined in more detail in tables 4 and 5. Following earlier practice, the information in these tables is restricted to the late experimental and control periods.

Table 4 shows the fluctuation of serious single-vehicle accidents around each soap opera suicide story. The format of table 4 is identical with that of table 3. The difference between the observed number of accidents after suicide stories (col. 1) and the estimated expected number (col. 4) is statistically significant ($t = 2.064$, $P = .039$; 7 df, one-tailed t -test for matched pairs). When the randomization test is applied to these data, $P = .04$.

In contrast to these results for the late experimental period, the early experimental period does not show a significant increase in serious single-vehicle accidents ($t = -.365$, $P = .363$; 7 df, one-tailed t -test for matched

TABLE 4
SEVERE NONFATAL SINGLE-VEHICLE CRASHES BEFORE AND AFTER
SOAP OPERA (SO) SUICIDE STORIES

Person Attempting or Committing Suicide in SO	Crashes in Late Experimental Period (N) (1)	Crashes in Late Control Period (N) (2)	Adjustment Required to Correct for Trend (3)	Adjusted Crashes in Late Control Period (N) (4)	Rise in Crashes after SO Suicide Story (Col. 1— Col. 4) (5)
M. Jeeter.....	65	45	.641	45.641	19.359
H. Grant.....	69				
D. Ryan.....	71	54	.240	54.240	15.760
S. Duval.....	74	57	.160	57.160	16.840
Dr. Namath.....	65				
P. Summers.....	69	68	.240	68.240	—1.240
A. Martin.....	80	60	.160	60.160	19.840
N. Vernon.....	69	60	.481	60.481	8.519
R. Becker.....	68				
E. Conrad.....	63	62	.321	62.321	2.679
"Meredith".....	64				
L. Dallas.....	64	77	.160	77.160	—13.160

NOTE.—All figures are for California, 1977. For dates of late experimental and control periods, see footnote to table 1.

pairs). This evidence suggests that the results in table 4 are not an artifact of the method of statistical analysis.

These findings for serious accidents are similar to findings for moderately serious accidents, which are described in table 5. The results in this table are statistically significant ($t = 2.097$, $P = .037$; 7 df, one-tailed t -test for matched pairs). When the randomization test is applied, $P = .05$. In contrast to these results for the late experimental period, the early experimental period does not produce a statistically significant increase ($t = .308$, $P = .384$).

Summary of Findings in Tables 1 through 5

Tables 1 through 5 show that suicides, motor vehicle deaths, and nonfatal motor vehicle accidents all increase significantly just after soap opera suicide stories. The best available explanation of the findings in tables 1 and 2 is that soap opera suicide stories trigger some overt suicides (table 1) and some covert suicides disguised as motor vehicle deaths (table 2). The best available explanation of the findings in tables 3 through 5 is that soap opera suicide stories trigger some covert suicide *attempts*, which are disguised as serious or moderately serious nonfatal single-vehicle accidents (tables 4 and 5).

TABLE 5
MODERATELY SEVERE NONFATAL SINGLE-VEHICLE CRASHES BEFORE AND
AFTER SOAP OPERA (SO) SUICIDE STORIES

Person Attempting or Committing Suicide in SO	Crashes in Late Experimental Period (N) (1)	Crashes in Late Control Period (N) (2)	Adjustment Required to Correct for Trend (3)	Adjusted Crashes in Late Control Period (N) (4)	Rise in Crashes after SO Suicide Story (Col. 1- Col. 4) (5)
M. Jeeter.....	331	258	5.294	263.294	67.706
H. Grant.....	345				
D. Ryan.....	325	340	1.985	341.985	-6.985
S. Duval.....	346	320	1.324	321.324	24.676
Dr. Namath.....	350				
P. Summers.....	401	317	1.985	318.985	56.515
A. Martin.....	377	385	1.324	386.324	-9.324
N. Vernon.....	462	388	3.971	391.971	70.029
R. Becker.....	362				
E. Conrad.....	338	353	2.647	355.647	12.020
"Meredith".....	403				
L. Dallas.....	342	350	1.324	351.324	-9.324

NOTE.—All figures are for California, 1977. For dates of late experimental and control periods, see footnote to table 1.

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Comparison of Rural-Urban and Male-Female Responses to Soap Opera Suicide Stories

It would be interesting to learn which type of person is most affected by soap opera suicide stories: urban or rural, young or old, female or male, married or divorced. A full examination of these questions is beyond the scope of this paper. Here I will confine myself to an examination of suicide statistics, showing that soap opera suicide stories seem to affect urban residents much more than nonurban ones, and females much more than males.¹⁴

Column 1 of table 6 gives the amount by which white urban suicides increase after soap opera suicide stories. Column 2 gives the equivalent information for white nonurban suicides. The *t*-statistics at the bottom of each column indicate that only urban suicides increased significantly after soap opera suicide stories. In columns 3 and 4 the white urban suicides are subdivided further into males and females. The *t*-statistics at the bottom of these columns indicate that urban female suicides rose significantly after suicide stories, while urban male suicides did not.

The fact that females respond more strongly to soap operas is of course to be expected, because females watch soap operas more than males do. The fact that urban residents respond more strongly than nonurban residents was not predicted, however. One can easily generate intriguing speculations, but no compelling explanation for this finding is at present available. Any

TABLE 6
INCREASES IN U.S. WHITE SUICIDES AFTER SOAP OPERA (SO)
SUICIDE STORIES, BY RESIDENCE AND SEX

Character in SO Suicide Story	Urban Suicides (1)	Rural Suicides (2)	Urban Male Suicides (3)	Urban Female Suicides (4)
M. Jeeter.....	24.639	20.189	19.458	5.181
H. Grant and D. Ryan.....	19.740	11.946	14.672	5.068
S. Duval.....	12.160	-2.703	6.114	6.045
Dr. Namath and P. Summers....	-5.260	8.946	-8.828	3.568
A. Martin.....	20.160	2.297	13.114	7.045
M. Dante.....	-1.840	-10.703	-12.886	11.045
N. Vernon.....	15.479	3.892	15.343	.136
R. Becker, E. Conrad, & "Meredith".....	-3.013	9.927	-2.771	-.242
L. Dallas.....	.160	1.297	4.114	-3.955
<i>t</i> -value for each column.....	2.359	1.671	1.408	2.514
Significance level.....	.023	.067	.098	.018

¹⁴ The term "urban resident" is defined here as a person residing in an area with 100,000 or more inhabitants. Such persons are coded 0-3 in col. 21 in the National Center for Health Statistics Detail Mortality Tape. Like other definitions of "urban," the one used here is plausible, but arbitrary.

such explanation must take into account the different viewing patterns of urban and nonurban residents. Information on these viewing patterns is available to commercial sponsors of television programs but does not seem to be freely available to academic researchers. In the absence of such information, it seems premature to speculate on why soap opera suicide stories affect urban residents more than nonurban ones.

DISCUSSION

There is always danger in speculating too far ahead of the facts, but speculation, though dangerous, can also be fruitful. It seems appropriate now to contemplate briefly some promising lines of research opened up by the findings of this paper. Three lines of investigation will be described: studies aimed at replicating, generalizing, and elaborating the findings of this paper.

Studies Aimed at Replicating the Findings

The ability to replicate a finding is one test of its reliability. Thus, it is important to determine whether the findings of this paper recur in new sets of data. Such data sets include 1978 United States mortality data, which will become available shortly, and daily nonfatal single-vehicle accident statistics for states other than California.

If these additional data sources support the earlier findings, the television networks may need to consider the possibility of reducing the amount of publicity they devote to suicide stories.

Studies Aimed at Generalizing the Findings

At present, we do not know whether the findings of this paper constitute an isolated phenomenon or are instead part of a more general class of imitative processes. Future research should seek to determine whether the results can be generalized. At least three different types of generalization are possible.

Generalizing the topic of the story.—This paper has focused on only one type of violence in the mass media: violence directed against the self. Traditionally, violence against others has received more attention in the literature. Future research should seek to determine whether other-directed violence in soap operas also induces imitative behavior. Such research may not be easy to conduct because accurate daily or weekly statistics on rape, arson, etc., may not exist for large geographic areas and some types of story (e.g., murder) occur so frequently that it is difficult to detect the effects of any given story.

Generalizing the format of the story.—So far I have focused on soap operas only, rather than other types of television format such as movies and

crime dramas. Do other programming formats also trigger imitation, and, if so, which type of format has the greatest effect?

Generalizing the impact of the story to other types of mortality.—So far I have examined only suicides and motor vehicle accidents. Do other types of accidents also increase after soap opera suicide stories? The relative amount by which each type of accident increases may help to suggest the relative size of its suicidal component. Accidents which increase a great deal presumably have a larger suicidal component than accidents which increase very little.

A study of the increases in various types of accidents might help to resolve the current controversy on the degree to which suicide is misclassified as other causes of death. Evidence of substantial misclassification might encourage reexamination of current theories of suicide, which are based on suicide data not corrected for misclassification.

Finally, evidence of substantial misclassification would suggest that suicide is an even greater public health problem than is currently supposed.

Studies Aimed at Elaborating the Findings

The detailed study of a phenomenon often helps to illuminate the processes that produced it. This paper has provided only a preliminary analysis; many specific questions remain to be answered. For example, it would be useful to determine whether one type of soap opera suicide story induces more imitation than another. Which type of character is imitated more often: a hero or a villain, a major or a minor character, a newly introduced soap opera character or one who has been "on the air" for many years? It would also be valuable to learn whether the fictional suicide victim is similar to the actual suicide victims who die just after the suicide story. When a young, single, female soap opera character takes poison, does this lead to an increase in young, single, female poisoning suicides only, or is the effect of the story more diffuse and general? The answers to these questions would help to illuminate the imitative processes linking soap opera characters and their audiences.

SUMMARY

For a generation researchers have hypothesized that fictional television stories lead to fatal imitative violence in the population as a whole. Until now the evidence bearing on this hypothesis has been suggestive but not compelling, in part because most of the evidence comes from studies of artificial behavior in the laboratory instead of natural behavior in the real world. In summarizing the literature on this topic, Andison noted, "What we do not know is whether . . . there are deaths and violence occurring in

society today because of what is being shown on the TV screen" (1980, p. 564). This paper has presented what seems to be the first systematic, quantitative evidence that television stories do indeed trigger deaths as well as near-fatal accidents in society today. Future research should aim at replicating, generalizing, and elaborating these findings. If replication is possible, the policy implications of the findings appear to be very serious.

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Determinants of Extended Household Structure: Cultural Pattern or Economic Need?¹

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This research examines the relationship between household composition and sources of household income among Hispanics, blacks, and non-Hispanic whites. Specifically, we investigate the extent to which extended living arrangements help buffer the effects of labor market disadvantages faced by minority household heads. Results of logit and regression analyses indicate that differences in the prevalence of extended family households reflect primarily group-specific differences in the propensity to extend, but that this demographic mechanism may also serve as a compensatory strategy for supplementing the temporarily or chronically low earnings of minority household heads. In black and Hispanic households, nonnuclear members contribute significantly to total household income, although their relative contributions are approximately similar for poor and nonpoor households. Nonnuclear members in non-Hispanic white households appear not to participate significantly in the generation of household income.

While there is little disagreement that members of minority groups are disadvantaged in terms of occupational attainment and earnings, controversy continues unabated as to whether inequality has decreased over time (Farley 1977; Blauner 1979; Wilson 1978). One key source of disagreement stems from a failure to acknowledge the impact of household composition on household income differentials between groups. This rather serious omission distorts the measurement of income inequality, since the pooling of resources among family members can alter household income significantly and affect the economic well-being of individual household members. In the light of the remarkable shifts in the living arrangements of the American

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population during the past 2 decades, the influence of household composition on traditional measures of economic inequality (e.g., gini coefficients) has become increasingly problematic. Treas and Walther (1978) explain the paradoxical stability of family income distribution in the face of substantial changes in household composition in terms of countervailing forces of greater and lesser inequality among different household types. However, they leave open the question whether the "income patterning of assortive mating, kin coresidence, and marital dissolution changed in response to turns in taste or altered economic constraints" (1978, p. 878). The present analysis represents an attempt to address this question.

Investigations by several researchers suggest that the measurement of inequality in terms of family income misrepresents the character and understates the extent of economic differentials among groups (Miller 1966; Mincer 1960; Sweet 1973; Taussig 1976). In particular, the presence of multiple-earner households reduces the family income differential that would exist if inequality were measured only in terms of the earnings of the head of household (Sweet 1973; Mincer 1960). Little is known, however, about whether placing additional members of the family in the labor force represents a short-term response to economic hardship or reflects group differences in the reliance on secondary earners. The extent to which racial and ethnic minorities differ in this regard is an additional area of uncertainty. Group differences in the level of labor force participation of household members other than the head are likely to influence to an unknown degree inequality measured in terms of family or household income. Analyses of the Current Population Surveys show that in recent years the sizable increase in the proportion of families with working wives has tended to bias downward to some extent relative income differentials among families (Miller 1966; Taussig 1976; Thurow 1975; Danziger 1980). It is conceivable that the earnings contributions of household members other than the wife further contribute to the decrease in family income differentials, though there is little systematic documentation of this phenomenon.

To the extent that multiple earners are more prevalent among minority households, observed reductions in family income inequality may largely represent changes in the income-generation strategies of domestic units rather than improvements in the earnings opportunities of primary or secondary workers. This possibility implies that family organization and labor supply patterns may be confounded with the extent and structure of income inequality. By compensating for the inadequate earnings of the primary earner, the income contributions of other family members may attenuate economic hardship and lead to decreased economic inequality at the household level (Burawoy 1976). The family system, then, may be important in alleviating some of the harsher aspects of poverty for low-income households.

These issues raise a general question which has motivated the research reported here: How is household composition related to the income-generation strategies of minority and nonminority groups? More specifically, does the prevalence of multiple-earner households differ among black, Hispanic, and non-Hispanic white households, and if so, to what extent does placing additional household members in the labor force help buffer the effects of labor market disadvantages faced by minority heads of households? The present analysis focuses on the contributions to total household income of individuals we term "non-nuclear" household members. These are individuals other than the head of household, the wife of the head, and children of the head. The prevalence of households containing nonnuclear members is fairly low in the United States, and few such individuals are available to contribute to household income. Nonetheless, if racial and ethnic groups differ in their propensity to include nonnuclear members, it is important to determine whether the difference affects a group's ability to generate income or merely reflects different tastes for extended living arrangements.

To ascertain whether nonnuclear household members contribute to household income, we disaggregate household income into its components. The analysis focuses on the relationship between living arrangements and the earnings of nuclear and nonnuclear members for non-Hispanic white, black, Mexican, Puerto Rican, Central/South American,² and other Spanish origin family households. Special emphasis is placed on the differential prevalence of extended household composition and female-headed households among these groups. Subsequently, the economic significance of extension is examined by partitioning household income into the relative contributions of nuclear and nonnuclear household members, and determining the factors which influence the propensity of a household to extend. Finally, a regression analysis is performed to determine whether, and to what extent, the earnings of (1) the wife and children of the head, and (2) other co-residing individuals may alleviate economic hardships experienced by minority households.

HOUSEHOLD COMPOSITION AND INCOME SOURCES: SUPPORTING EVIDENCE AND RELATED ISSUES

The importance of exploring the relationship between household composition and sources of income stems from the fact that the labor force participation of family members responds potentially to both temporarily low

² We have combined Cubans with Central and South Americans for the present analysis. We consider this appropriate for both practical and substantive reasons: (1) we can overcome sample size constraints that we encounter with both groups; and (2) Cubans and Central and South Americans are similar in a number of important socioeconomic and demographic characteristics such as immigrant composition, educational attainment, and occupational status.

and permanently low income of the head, so that the difference between customary or desired family income levels and actual levels is partly compensated for by the earnings of other family members. In support of this theory, Mincer (1960) showed that family income levels are supplemented by both the contributions of working wives and the income of other family members. Moreover, his results showed that the contributions of these individuals increase when the head's income is below its customary level.

The most prevalent alternative source of income available to families in which the household head's income is low is the earnings of the wife. In some instances, however, families rely on the earnings of other household members, including nonnuclear members. A major objective of the present analysis is to determine to what degree extended living arrangements may represent a compensatory strategy for dealing with either temporarily or chronically low earnings.

Using 1960 census data, Sweet (1973) found not only that nonwhite wives contributed a notably higher share of total family income than white wives (17% vs. 12%), but also that other relatives too contributed a larger share of family income in nonwhite than in white households (12% vs. 7%). In general, the contributions of other family members, including both grown children and nonnuclear relatives, constituted a higher share of total family income for blacks than for whites. Sweet suggests that this pattern results because the lower earnings of nonwhite husbands require that other family members supplement income to achieve a desired standard of living or, in some instances, simply to meet basic needs.

Less is known about the economic functions of nonnuclear members among Hispanic groups in the United States, though there is a tendency to characterize Spanish origin families as more "familistic" than the general population. For example, the Mexican American family has been depicted as having clearly established patterns of support and mutual aid among family members and as being so cohesive that collective needs take precedence over individual needs (Mirandé 1977; Keefe 1980). Among immigrants, the need to rely on relatives may be particularly great because of the ability of those who have migrated earlier to cushion the potentially disorganizing consequences of international migration for new arrivals. Prospects for obtaining social and material support (e.g., temporary lodging, job information, loans, and legal protection) from friends and relatives at the point of destination play a role in shaping the immigrant adjustment process (Keefe 1980; Tienda 1980; Mirandé 1977).

DATA AND APPROACH

Our empirical analysis is based on the 1976 Survey of Income and Education (SIE), a large public-use microdata file consisting of over 150,000

households. This survey was originally designed to provide reliable estimates of the number of school-aged children living in poverty in each state. However, because of its unusually large size and the deliberate oversampling of Hispanics and blacks, the SIE has become an important intercensal survey for study of the socioeconomic characteristics of the noninstitutionalized population in the United States. When properly weighted, the sample is representative of the general population.

Rather than employ the conventional Census Bureau definition of family—that is, any household containing a head and at least one relative of the head—we employ a stricter definition of family household. The present analysis is based on a subsample of households consisting of all husband/wife and female-headed units in which the head is at least 18 years of age and which contain at least one child under 18. Using the conventional census definition, the majority of female-headed households must contain at least one child in order to be considered a family, whereas a husband/wife household need not contain a child in order to qualify as a family. In our opinion, the economic burdens of households with children are greater than those of households without children.⁸ Households headed by a male whose spouse is absent are excluded from the analysis because there are too few in our sample. The inclusion of households headed by single women is justified not only because of their increasing prevalence, but also because the absence of a male breadwinner greatly increases the likelihood that such families will have incomes below the poverty level (Ross and Sawhill 1975) and that they will be extended (Tienda and Angel, *in press*; Tienda and Ortega, *in press*). In this analysis households containing only a head, spouse, and one or more own children are classified as nonextended, while those containing at least one other relative of the head, secondary family member, or secondary individual are classified as extended.

Our analysis begins with a decomposition of total household income into the following categories: (1) earnings of the head; (2) earnings of the spouse and adult children; (3) earnings of nonnuclear members; (4) welfare income; and (5) other nonwork income. Our first objective is to document differences in the income contributions of nonnuclear members among the racial and ethnic groups. A second objective, less amenable to analysis in this paper, is to determine whether the formation of extended family households is related to the alleviation of poverty or whether other cultural

⁸ Our decision to define "family household" as we do proved important because preliminary analyses revealed that results based on comparisons which included childless couples exaggerated differences between husband/wife and female-headed households. This was due to the fact that the presence of children influences not only the likelihood that a household will extend (i.e., incorporate one or more nonnuclear relatives), but also the amount of labor supplied by other family members.

and demographic factors are more important explanatory variables.⁴ If the formation of extended family households reflects, at least in part, an economic response to poverty, the earnings of nonnuclear members should constitute a significant proportion of total household income.

HOUSEHOLD COMPOSITION AND INCOME SOURCES

As may be observed in table 1, there is great diversity in the demographic characteristics of non-Hispanic white, black, and Hispanic-origin households. Most family households are headed by a married couple, but a large proportion of families are headed by women, especially among minority groups. The proportion of black and Puerto Rican households headed by women is especially high, averaging 44% and 39%, respectively. Between one-fifth and one-fourth of Mexican, Central/South American, and other Spanish origin households are headed by a woman.

Non-Hispanic white and Central/South American households contain the fewest members, averaging 4.3 persons in husband/wife households and 3.0 persons in female-headed households. Mexicans and blacks have the largest families, averaging between four and five persons per household. For these groups, the larger family size is due, in large part, to the persistence of higher fertility and to norms which favor large families (Edington and Hays 1978; Bradshaw and Bean 1972). Puerto Rican and other Spanish households are slightly smaller than Mexican households, a difference which is due at least partly to the greater prevalence of single-parent households among these groups, but also to their lower fertility.

For all groups, extended family structure is more prevalent among households headed by single women than among units where both spouses are present. The proportion of extended female-headed households ranges from 12% for Puerto Ricans to 28% for blacks. Among husband/wife households, on the other hand, those headed by blacks and Central/South Americans are most likely to contain one or more nonnuclear members, whereas non-Hispanic white husband/wife households are least likely to be extended. Note that while the proportion of female-headed households is similar for blacks and Puerto Ricans, Puerto Rican households headed by women are less than half as likely to contain one or more nonnuclear members as are black households.

If the formation of extended households is motivated strictly by economic considerations, units headed by single women should be more inclined to extend, owing to the economic deprivation such households experi-

⁴ Our data do not allow us to determine whether the formation of extended family households is related to the alleviation of poverty or whether extended families are merely coincidental with poverty. This requires panel data which are not presently available. However, our methodology is highly suggestive along these lines.

TABLE 1

SELECTED SOCIODEMOGRAPHIC CHARACTERISTICS OF FAMILY HOUSEHOLDS BY RACE, HISPANIC
NATIONAL ORIGIN, AND TYPE OF HEADSHIP: UNITED STATES, 1976
(Means or Percents)

RACE OR HISPANIC NATIONAL ORIGIN						
CHARACTERISTICS	Non-Hispanic White	Black	Mexican	Puerto Rican	Central/South Americans	Other Spanish
Household characteristics:						
Type of headship:						
Husband/wife (unweighted <i>N</i>).....	85.9 (10,464)	55.7 (3,546)	80.1 (1,362)	61.4 (266)	80.7 (242)	75.3 (375)
Female head (unweighted <i>N</i>).....	14.1 (1,686)	44.3 (2,798)	19.9 (331)	38.6 (163)	19.3 (51)	24.7 (104)
Proportion extended households:						
Husband/wife.....	7.0	19.6	11.1	11.3	19.9	9.4
Female head.....	16.5	28.2	20.0	12.3	21.3	27.5
Mean household size:						
Husband/wife.....	4.3	4.8	4.9	4.5	4.2	4.4
Female head.....	3.0	3.8	3.9	3.5	3.0	3.5
Characteristics of head:						
Mean age of head:						
Husband/wife.....	41.4	41.8	38.8	39.3	41.5	41.6
Female head.....	45.6	39.3	40.5	32.9	37.9	42.0
Proportion foreign born:						
Husband/wife.....	4.7	2.1	31.0	85.0 ^b	97.5	28.2
Female head.....	5.9	1.4	27.5	85.5 ^b	100.0	29.9
Economic characteristics:						
Proportion below poverty:						
Husband/wife.....	4.1	14.2	16.4	18.4	11.2	8.6
Female head.....	22.5	49.2	48.9	59.5	48.1	35.7
Mean household earnings:						
Husband/wife.....	\$17,751	\$13,219	\$11,339	\$11,027	\$15,465	\$14,313
Female head.....	6,910	4,274	3,863	1,671	4,924	5,018
Number of workers per household:						
Husband/wife.....	1.8	1.6	1.6	1.2	1.7	1.5
Female head.....	1.1	.8	.8	.3	.9	.8

SOURCE.—1976 survey of income and education.

^a Includes Cubans.

^b Born on the island.

ence (Cooney 1979a; Ross and Sawhill 1975; Tienda and Angel 1981). However, economic disincentives to the formation and maintenance of extended family households may outweigh potential benefits because the incorporation of additional relatives may decrease the economic well-being of the household by increasing the pressure on already limited resources. The inclusion of a nonnuclear member, then, depends on whether the additional member is needed and is able to share the market and domestic workload, or on the extent to which the nuclear family can afford to take on a new member.

A large proportion of low-income households rely on some form of direct public assistance or other, nonmonetary, transfers in order to compensate for low earnings. This is a particularly important option for female-headed households, since such transfers allow the female head to devote more time to domestic functions but assure her of some minimal support. Certain groups, such as Puerto Rican women, face severe labor market disadvantages and earn very little even when they are able to find work (Cooney 1979b). Note that the average number of workers among female-headed minority households is slightly less than one, but for Puerto Ricans the figure is not even one-third.

Economic differences among racial and ethnic groups are documented in the bottom panel of table 1. As expected, the prevalence of poverty is consistently lower among husband/wife households than among female-headed households. Among husband/wife households, between 4% (non-Hispanic white) and 18% (Puerto Rican) of all units had incomes below the poverty level in 1975.⁵ The prevalence of poverty among female-headed units is three to four times higher than for husband/wife households. Almost half of black, Mexican, and Central/South American households headed by women had incomes below the 1975 poverty threshold. The average incomes of Mexican, black, and Central/South American female-headed households were \$3,863, \$4,274, and \$4,924, respectively. Puerto Rican female heads fared considerably worse, with an average household income of approximately \$1,671 and a poverty rate of 60%. The last row of table 1 indicates that the number of workers per household differs greatly among the racial, ethnic, and headship subgroups.

These findings raise an important question: Do the observed differences in extended family structure among minority group households actually result in increases in household income either through the direct contributions of the nonnuclear members or through the increased market activity

⁵ These percentages are based on the nonfarm poverty cutoffs in 1975 as reported in table A-2 of *Current Population Reports* (U.S. Department of Commerce 1977). Although poverty cutoffs were coded in the Survey of Income and Education, these were based on the Census Bureau definition of family income and thus do not always take into account the earnings or unearned income of nonnuclear members as we define them. Therefore these measures were not adequate for our purposes.

of other family members, or does the presence of nonnuclear members merely reflect cultural preferences for extended living arrangements? This issue is indirectly addressed in table 2, which presents a summary breakdown of the sources of household income for each race and ethnicity group according to type of headship.

Overall, these findings are consistent with evidence concerning the relatively greater share of income contributed by secondary workers among black households (Sweet 1973). A previously unrevealed finding, however, is the importance of secondary nuclear earners (wife and children) among families of Spanish origin. It has been suggested that intrafamilial, non-monetary contributions are particularly important among Spanish origin households (Mirandé 1977; Montiel 1970). This suggestion, however, may reflect a social science bias rather than scientific evidence demonstrating the greater prevalence of familistic norms among Hispanics (see Staples and Mirandé [1980] for recent discussion of Mexican American families). Although Hispanic wives and other nuclear members contribute a lower share of total household income than do black wives, the earnings contributions of Hispanic secondary earners represent a share of household income comparable to or slightly greater than that of non-Hispanic white secondary earners. Earnings constitute the largest portion of total household income for most families, although nonwork income, which includes such things as interest, dividends, rent income, pensions, and a variety of nonwelfare payments, such as child support and alimony, accounts for approximately 9% of average total household income among husband/wife families and between 10% and 34% of total household income among female-headed units. Not surprisingly, the share of total household income obtained from welfare is greater for female-headed households than for husband/wife households. This is especially true for minority groups. Puerto Rican households headed by single women obtain half of their total income from welfare, whereas the comparable share for non-Hispanic white households headed by a woman is approximately 4%.

Among husband/wife families, the income contribution of nonnuclear household members is trivial in both absolute and relative terms. The head's earnings account for approximately two-thirds of total household income for all groups except blacks, for whom the respective share of total household income is 58%. Earnings of other nuclear members, primarily the spouse, represent between 20% and 30% of total household income. It would appear that, within the context of what we might call normative headship structure (two-spouse households), nonnuclear members are probably helped more by joining households headed by relatives rather than the reverse. However, this statement must be qualified by acknowledging that the presence of nonnuclear members increases the flexibility of households in coping with economic hardship by facilitating alternative work arrange-

TABLE 2

COMPONENTS OF 1975 FAMILY HOUSEHOLD INCOME BY TYPE OF HEADSHIP, SOURCE OF
INCOME, AND RACE OR HISPANIC NATIONAL ORIGIN: UNITED STATES
(Mean Income in 1975 Dollars)

Race or National Origin	Type of Headship and Source of Income									
	Husband/Wife Families					Female-headed Families				
	Head's Work Earnings	Other Nuclear Earnings	Non- nuclear Earnings	Welfare	Other Nonwork Income	Total	Head's Work Earnings	Other Nuclear Earnings	Non- nuclear Earnings	Total
Non-Hispanic white:										
Mean.....	\$13,904	\$3,747	\$101	\$45	\$1,697	\$19,494	\$3,951	\$2,252	\$707	\$414
%.....	71.3	19.2	.5	.2	8.7	99.9	39.1	22.3	7.0	4.1
Black:										
Mean.....	\$8,540	\$4,530	\$149	\$219	\$1,217	\$14,655	\$2,913	\$952	\$410	\$1,280
%.....	58.3	30.9	1.0	1.5	8.3	100.0	43.5	14.2	6.1	19.1
Mexican:										
Mean.....	\$8,478	\$2,714	\$147	\$173	\$995	\$12,507	\$2,309	\$1,009	\$544	\$1,098
%.....	67.8	21.7	1.2	1.4	8.0	100.1	36.4	15.9	8.6	17.3
Puerto Rican:										
Mean.....	\$8,398	\$2,488	\$141	\$427	\$1,107	\$12,561	\$1,021	\$334	\$316	\$2,694
%.....	66.9	19.8	1.1	3.4	8.8	100.0	19.1	6.2	5.9	50.3
Central/South American*										
Mean.....	\$11,147	\$4,017	\$300	\$92	\$1,308	\$16,864	\$3,278	\$697	\$949	\$885
%.....	66.1	23.8	1.8	.5	7.8	100.0	50.8	10.8	14.7	13.7
Other Spanish:										
Mean.....	\$11,123	\$3,157	\$33	\$131	\$1,422	\$15,866	\$3,027	\$1,184	\$807	\$992
%.....	70.1	19.9	.2	.8	9.0	100.0	33.3	13.0	8.9	10.9

Source.—1976 survey of income and education.

* Includes Cubans.

ments among nuclear members. In other words, to the extent that the non-nuclear members assume a greater share of the domestic work roles, it is possible for nuclear members to reallocate time to work. This internal allocation of household and market work roles cannot be examined with these or similar census data, but is, no doubt, an important dimension of the interface between work and familial roles that warrants further attention when more suitable data become available.

The income contributions of nonnuclear members are more substantial among female-headed households than among households where both spouses are present. Nevertheless, even among female-headed households the earnings of nonnuclear members form a smaller share of household income than do the earnings of secondary nuclear earners, with the notable exception of households headed by females of Central/South American origin. It is important to note, however, that the propensity of a female-headed household to include one or more nonnuclear members is itself governed by policies determining eligibility for public assistance. The presence of adult males, whether employed or not, may render a household ineligible for public assistance. Thus, while differences in living arrangements may be motivated partly by economic considerations, they are also influenced by institutional factors. Unfortunately, therefore, the relationship among poverty status, household composition, and sources of income remains largely indeterminate (MacDonald and Sawhill 1978).

There are two possible explanations for the differing income-generation strategies of Central/South Americans and those of other Hispanic or non-Hispanic groups. One has to do with the immigrant status of most Central/South Americans. It is likely that the formation of extended households has more to do with attempts to reunite families that fled Cuba, Chile, Brazil, and other Central and South American countries for political reasons than with economic need per se. Unlike Mexican and Puerto Rican immigrants, most legal immigrants from Central and South America are not poor. Thus, the greater prevalence of extended households in this group is to a large extent due to political factors, and only rarely due to economic hardship associated with minority group status. A second reason for the relatively greater income contributions of nonnuclear members among Hispanic households is the more popular cultural argument mentioned earlier, which maintains that Hispanic origin groups have different traditions governing living arrangements and mutual aid among relatives.

While not conclusive, these results are suggestive concerning the role of extension in a family's strategy for earning a living. For female-headed households, the absence of a male breadwinner increases the importance of the income contributions of secondary earners. It also increases the household's reliance upon transfer payments, as women workers suffer the disadvantages of both fewer job opportunities and lower wages for their work.

Although extension does not appear to be a generally effective means for combating poverty, this demographic mechanism may, to some degree, alleviate the harsher aspects of poverty. Our data do not allow us to determine what proportion of nonpoor families were poor prior to the inclusion of nonnuclear members. However, it is possible to ascertain whether the income contributions of nonnuclear members in poor households actually serve to lessen the economic hardship which would exist in their absence. This issue is addressed in the multivariate analysis which follows.

MULTIVARIATE ANALYSIS

Our evaluation of the extent to which the formation of extended family households is related to an income-generation strategy focuses on the income contributions of nuclear and nonnuclear members as components of total household income. One basic goal is to determine whether the formation of extended family households is related to the alleviation of poverty, or whether other sociocultural factors are more important explanatory variables. If the formation of extended family households represents, at least in part, an economic response to poverty, nonnuclear members should contribute significantly to total household income. Although this finding would be insufficient to justify conclusions about the role of extension as a purposive strategy for dealing with poverty, results along these lines would be suggestive.

The analysis consists of two parts. First, we examine the propensity of the various ethnic and racial groups to form extended households. For this we employ a logistic regression.⁶ Second, we employ a regression analysis to determine the extent to which the earnings of nonnuclear members contribute to total household income.

Table 3 presents the logit coefficients for the analysis of differences in the prevalence of extended households among the various ethnic and racial groups. In this table, the dependent variable is a dichotomy, extended versus nonextended. The logarithm of the probability of extension is expressed as a linear function of a constant term, ethnicity, female headship, the ratio of household income to poverty, the educational level, nativity, and full-time employment status of the household head. The operationalization of these variables is summarized in Appendix table A. The coefficients in table 3 are additive parameters which express the incremental impact of the independent variable on the logarithm of the probability of extension.

The first-order partial derivative of each independent variable is also

⁶ This analysis is based on a program entitled *MLOGIT*, version 2.0, written by Bronwyn H. Hall, 204 Junipero Serra Boulevard, Stanford, California 94305.

presented. This is computed as: $b_i P^*(1 - P^*)$ where b_i is the coefficient of the relevant independent variable and P^* is taken to be the proportion of the sample that consists of extended households (Hanushek and Jackson 1977). This derivative is interpretable as the increment to the actual probability of extension associated with a one-unit increase in an independent continuous variable, or with membership in the relevant category of a dichotomous variable. The sample proportion is chosen as a realistic representation of the actual overall probability of extension. The derivatives would be different computed at other points along the logistic curve.

Table 3 presents two logit models. Model 1 tests for the effects of race and Hispanic origin on the probability of extension. In this model, non-Hispanic whites serve as the reference category. Model 1 indicates that, in

TABLE 3
LOGIT COEFFICIENTS DESCRIBING THE EFFECTS OF VARIOUS CHARACTERISTICS
OF THE HEAD OF HOUSEHOLD ON THE LOG ODDS
OF FORMING AN EXTENDED HOUSEHOLD
(Standard Errors in Parentheses)

	MODEL 1		MODEL 2	
	Logit Coefficient	First Derivative*	Logit Coefficient	First Derivative*
Constant.....	-2.446** (.034)	-.280	-1.070** (.091)	-.123
Mexican.....	.452** (.082)	.052	.036 (.091)	.004
Puerto Rican.....	.436** (.154)	.050	.031 (.183)	.004
Cuban/Central South American....	1.047** (.150)	.120	1.130** (.184)	.130
Other Spanish.....	.466** (.144)	.053	.190 (.149)	.022
Black.....	1.266** (.045)	.145	.873** (.050)	.100
Female head.....644** (.051)	.074
Ratio of household income to poverty.....099** (.017)	.011
Education of head.....	-.143** (.007)	-.016
Foreign birth of head.....	-.241* (.106)	-.028
Full-time employment.....	-.180** (.051)	-.021
Proportion of sample extended.....132	...
Degrees of freedom.....	21,316	...	21,311	...
$-2 \times \log$ likelihood χ^2	15,824	...	15,098	...
Degrees of freedom change.....	5	...
χ^2 change.....	726	...

* $b_i P^*(1 - P^*)$, at $P^* = .132$.

* Significant at .05.

** Significant at .01.

every case, nonwhite or Hispanic origin increases the likelihood of extension. The impact of nonwhite and Central/South American origin is particularly pronounced. Black households are 14% more likely than non-Hispanic white ones to be extended. Mexican, Puerto Rican, and other Spanish households are approximately 5% more likely to extend than non-Hispanic white ones.

Model 2 introduces the five control variables which are hypothesized to affect the probability of extending. The decrease in the log-likelihood chi-squared value indicates that these variables add significantly to the explanation of extension. With these controls included in the equation, the coefficients for Mexican, Puerto Rican, and other Spanish households become insignificant, indicating that the increased prevalence of extended households among these groups is due to the control variables. Female headship is associated with a large increase in the likelihood of extension. To the extent that the prevalence of female headship differs among groups, the overall prevalence of extended households will be affected.

The ratio of household income to poverty has a significant but small positive impact on the likelihood of extension. For each 100% increment in the ratio of household income to poverty, the odds of extension increase by 1.1%. The education of the head has an impressively strong negative impact on the odds of extension. For each year of school completed by the head, the likelihood of extension decreases 1.6%. Again, to the extent that groups differ significantly in the amount of education they receive, they will differ in the prevalence of extended household composition. Contrary to what might have been expected, foreign birth of the head of household has a negative impact on extension, suggesting that households with foreign-born heads may be in a process of adjusting to a new society and thus are less able to incorporate nonnuclear members than are households with native-born heads.

Not surprisingly, full-time employment of the household head is associated with a decrease in the odds of extension. This indicates that households headed by individuals who are employed full time do not face the economic constraints of households with unemployed heads. Consequently, the former are less inclined to include nonnuclear members. However, the fact that households headed by persons who are not employed full time are more apt to extend does not establish whether the nonnuclear members play an important role in alleviating economic pressures associated with inadequate employment of the head. Some insight along these lines is afforded below.

In order to investigate the relative income contribution of various household members, we employ a regression analysis in which total annual household income is regressed on the earnings of the wife, nuclear children, and

nonnuclear members.⁷ The regression coefficients for the other household members, then, indicate whether their earnings contribute significantly to total household income. Unlogged dollars are used because we are interested in the pattern of relationships among the components of household income rather than in the rates of return to specific individual-level characteristics. In order to make the distribution of household income acceptably normal, households with negative total incomes are dropped from the analysis and household incomes above \$50,000 recoded to \$50,000.

In addition, in order to assess the relative monetary contributions of non-nuclear members residing in households with incomes above and below the poverty threshold, interaction terms are introduced to capture the relationship between the poverty level of the household and the income contributions of the three categories of secondary earners. This required first creating a dummy variable to represent the status of families with respect to the poverty threshold. All families with a combined household income equal to or greater than 150% of the 1975 poverty line were scored 1.⁸ Subsequently, the dummy variable representing poverty status was multiplied by the income contributions of the wife, own children, and other nonnuclear members (Rao and Miller 1971). This procedure divides the average contributions of the wife, own children, and nonnuclear individuals into two components. For households with total income below 150% of the poverty line the contributions of various household members are represented by the relevant earnings coefficients. In households above 150% of poverty the contributions of the wife, own children, and nonnuclear members are represented by the relevant earnings coefficient plus the interaction term of that component with 150% of poverty.

⁷ The analysis, in controlling for the contributions of wives, children, and nonnuclear members, allows only the excluded income components (e.g., husband's earnings, property income, intrafamilial transfers) to vary in the dependent variable, total household income. Controlling for these various components, therefore, tests for the significance of the income contributions of wives, children, and nonnuclear members at different levels of the excluded household income component. In this way, we test for the relative significance of the contributions of nonnuclear members, as well as of wives and children, at various levels of need measured in terms of the components omitted from the analysis.

⁸ The poverty cutoffs are those identified in note 5, above. Although 150% of the official poverty line may appear to be an arbitrarily high cutoff for distinguishing between the poor and nonpoor, we wish to emphasize that the official poverty line, as currently constructed, is an unrealistically low subsistence minimum. The official poverty cutoff was originally designed to represent a short-term or emergency minimum subsistence standard. Over time, it has become rigidified as an absolute and acceptable minimum standard. We experimented with alternative breakpoints for creating the poverty dummy variable, including a test of the more conventional measure 125% of the poverty threshold to designate the poor family households. However, we found the 150% cutoff discriminated as well as alternative cutoff points, and we opted to use it in order to generate more conservative estimates and portray poverty thresholds more realistically.

Extended Household Structure

Our interest is in whether these contributions differ significantly from zero. For example, if the earnings coefficient of wives is significant, our substantive interpretation of this finding is that their monetary contribution is significant at the respective poverty level. Table 4 shows that black wives in households below 150% of poverty contribute significantly to total household income. The significant interaction term for wives' earnings and poverty threshold indicates that black wives in households with incomes above 150% of poverty contribute a significant *additional* amount to total family income. Because of the structure of this multivariate analysis, the magnitude of the regression coefficients is not readily interpretable.

TABLE 4
POOLED REGRESSION OF TOTAL HOUSEHOLD INCOME (Dollar Amounts) ON
SELECTED COMPONENTS OF HOUSEHOLD INCOME FOR NON-HISPANIC
WHITE, BLACK, AND HISPANIC ORIGIN HOUSEHOLDS
(Standard Errors in Parentheses)

	Non-Hispanic White	Black	Hispanic
Earnings:			
Wife.....	.11 (.52)	.45** (.15)	.23 (.59)
Own children.....	.41 (.33)	.85** (.08)	.90* (.35)
Nonnuclear.....	.67 (.38)	.83** (.13)	.88** (.34)
Poverty status (above 150% = 1).....	6,726.18** (761.11)	4,748.25** (281.74)	6,746.71** (897.48)
Interaction with:			
150% poverty—wife.....	.58 (.52)	.44** (.15)	.64 (.60)
150% poverty—children.....	.56 (.34)	.02 (.09)	-.29 (.36)
150% poverty—nonnuclear....	.05 (.39)	-.06 (.14)	-.30 (.35)
Headship (female = 1).....	-5,408.59** (528.31)	-2,443.51** (232.30)	-2,647.24** (724.73)
Nativity (foreign = 1).....	-1,287.82 (1,189.46)	-504.92 (785.98)	-7.07 (625.21)
SEI of head.....	117.41** (8.71)	76.56** (5.89)	98.08** (14.59)
Age of head.....	37.52* (17.26)	12.35 (7.82)	26.28 (23.33)
Number of children <18.....	302.40* (150.54)	394.95** (52.19)	535.21** (174.56)
Welfare income (presence = 1)...	661.64 (659.14)	160.79 (224.72)	-385.12 (733.13)
Average wage rate.....	2,069.12** (292.49)	1,132.99** (131.52)	-36.91 (473.52)
Constant.....	-6,037.89	-1,411.42	3,062.54
Adjusted R ²66	.81	.62
N.....	967	1,485	368

* Significant at $P \leq .05$.

** Significant at $P \leq .01$.

An insignificant coefficient is, as usual, essentially zero, meaning there is no contribution, whereas a significant coefficient is essentially one, indicating a significant contribution to total household income.

Several controls are introduced to deal with the influence of variables which affect earnings systematically. These fall into three general categories: (1) characteristics of the head which affect earnings opportunities, including nativity, age, and occupational status; (2) household characteristics which are related to the propensity to add one or more nonnuclear members or which alter the economic status of the household, including type of headship, number of children, and the presence of welfare income; and (3) the average prevailing wage rate in a designated labor market which gauges the income opportunities for all family earners. A summary of the means and standard deviations of the variables included in the regression analysis is presented in Appendix B.

The sample for this part of the analysis is the subset of family households containing one or more nonnuclear members. Our basic question is, Given that families are extended, do the income contributions of nonnuclear members represent a significant share of total household income? For this analysis, the Hispanic population is analyzed as a single group since the sample sizes of the extended subset of families preclude further disaggregation.

Results of the pooled regression analyses are presented in table 4. Since the equation constitutes a partial identity, R^2 is quite high for all models, but especially for blacks. This is because the income components omitted from the function form a lower share of total household income for blacks than for the other two groups. Of greater substantive interest are the metric regression coefficients which test for the significance of the monetary contributions of secondary earners by level of income adequacy.

Among non-Hispanic white households with incomes below 150% of poverty, the earnings of the wife, children, and nonnuclear members do not contribute significantly to total household income. This is not the case for black and Hispanic households, however. In minority households with incomes below 150% of the poverty threshold, the earnings of children and nonnuclear members are positively related to total household income, but the earnings contributions of wives are significant only for blacks.

While families with incomes below 150% of poverty may attempt to increase their overall well-being by placing more of their members in the labor force, the limited employment opportunities faced by these individuals render their supplemental income inadequate to lift the household out of poverty. In extended Hispanic households, the earnings contribution of nonnuclear members is significant whereas the earnings of wives are insignificant. Nevertheless, this finding does not justify conclusions about the viability of extension as an economic response for coping with low income

levels because the relative contributions of nonnuclear members are not significant above 150% of poverty. The fact that the total income of some minority households remains below poverty in spite of the positive contributions of secondary earners, including nonnuclear members, implies that the extent of income inequality between minority and nonminority groups would be even greater than it is in the absence of alternative strategies to compensate for the inadequate earnings of primary workers.

Although the income-generation strategies of Hispanic households with incomes above and below 150% of the poverty level appear to be roughly similar (the interaction components are insignificant), this is not the pattern in black households with incomes above the poverty level, in which wives contribute a significant additional amount to total household income in units with incomes below the poverty threshold. Inferences about whether the earnings of secondary earners in nonpoor households actually play a role in achieving income inadequacy remain speculative since such inferences would require information about the family income deficit in the absence of the earnings of wives, adult children, and nonnuclear members. Our data do not allow us to examine this question in greater detail. What seems less debatable is that nonnuclear members do not appear to play important economic roles in the income-generation strategies of non-Hispanic white households.

Headship emerged as a uniformly strong predictor of total household income in all three income equations. The magnitude of the coefficient is as impressive as its consistency of direction for all groups. Among non-Hispanic whites, households headed by single women have incomes approximately \$5,409 lower than do otherwise comparable husband/wife households. A somewhat smaller depressing effect of female headship is observed for black and Hispanic households, where the absence of a male breadwinner decreases total annual household income by \$2,444 and \$2,647, respectively. Because the absence of a male breadwinner implies a substantial loss of annual household income among all groups, it is possible that spouse-absent units will be motivated to seek alternative living and working arrangements to compensate for the absence of a male breadwinner. Moreover, the fact that women generally earn less than comparable male workers should increase further the incentives of female heads of household to seek additional income and social support from other relatives and nonnuclear members (Tienda and Ortega, *in press*).

To investigate this possibility further, we compute separate functions for husband/wife and female-headed family households. The results are presented in table 5. Although the controls employed in the pooled equations are introduced into these equations, only the coefficients for the components of family income in which we are interested are shown. With few exceptions, results for husband/wife households are similar to those obtained from the

pooled models. Notable exceptions include the failure of nonnuclear members to contribute significantly to the total income of Hispanic households. Another difference is that black wives do not make significantly greater income contributions in households above 150% of poverty than in those below 150%.

Compared with households in which both spouses are present, female-headed units have one less adult to participate in the income-generation process. This accounts in large measure for their lower average incomes. However, the results from the regression analysis show that the income-

TABLE 5
REGRESSION OF TOTAL HOUSEHOLD INCOME (Dollar Amounts) ON SELECTED
COMPONENTS OF HOUSEHOLD INCOME FOR NON-HISPANIC WHITE, AND
HISPANIC NATIONAL ORIGIN HOUSEHOLDS BY TYPE OF HEADSHIP
(Standard Errors in Parentheses)

	Non-Hispanic White	Black	Hispanic
Husband/wife families:			
150% poverty.....	8,800.51** (1,046.27)	5,425.21** (449.73)	7,757.77** (1,228.23)
Earnings:			
Wife.....	.74 (.58)	.64** (.17)	.46 (.67)
Own children.....	.33 (.45)	.90** (.20)	1.08* (.49)
Nonnuclear.....	.61 (.56)	1.05** (.39)	.75 (.94)
Interact with:			
150% poverty—wife.....	-.10 (.58)	.18 (.17)	.38 (.68)
150% poverty—children.....	.69 (.45)	.03 (.21)	-.69 (.50)
150% poverty—nonnuclear..	.10 (.57)	-.18 (.39)	-.11 (.95)
Constant.....	-6,406.76	-2,247.29	3,733.04
R ²61	.81	.54
N.....	690	699	254
Female-headed families:			
150% poverty.....	4,438.23** (942.00)	4,675.17** (369.01)	3,661.82** (1,319.81)
Earnings:			
Own children.....	.47 (.40)	.81** (.08)	.88* (.46)
Nonnuclear.....	.67 (.41)	.73** (.13)	.82** (.28)
Interact with:			
150% poverty—children.....	.41 (.40)	-.05 (.09)	.21 (.46)
150% poverty—nonnuclear..	.13 (.41)	.00 (.13)	-.04 (.32)
Constant.....	-9,806.61	-2,435.35	-2,366.05
R ²76	.76	.69
N.....	277	786	114

* Significant at $P \leq .05$.

** Significant at $P \leq .01$.

Extended Household Structure

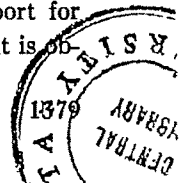
generation strategies of female-headed households do not differ greatly from those of husband/wife family households. If anything, differences among the racial and ethnic groups appear to be more salient.

Variations in the importance of the income contributions of nonnuclear members among racial and ethnic groups reflect differences in the labor market success of the groups as well as differences in the motivation to form and maintain extended family households. As hypothesized, non-nuclear individuals living in female-headed households make net positive contributions to total household income, but only for minority groups. The relative importance of the income contributions of nonnuclear members, however, is virtually identical among poor and nonpoor households. Thus, it appears that although the practice of incorporating one or more nonnuclear members may be linked to the desire to compensate for the earnings of the absent spouse, the extension mechanism does not lead to income adequacy.

SUMMARY AND DISCUSSION

Our results are suggestive concerning the role of extension in a family's strategy for earning a living. There is sufficient evidence to indicate that the extension mechanism may help alleviate poverty, or at least provide households with greater flexibility in allocating market and domestic roles among members. Our analysis, though, reveals that extended family structure is more prevalent among minority households and those headed by single women than among nonminority and husband/wife households, and that the economic roles of nonnuclear members differ according to the race, ethnicity, and sex of the head. Variations in the relative income contributions of nonnuclear members reflect differences in the labor market success of the racial and ethnic groups as well as group differences in the motivation to form and maintain extended family households. Among husband/wife households, the income contributions of nonnuclear members are trivial in both relative and absolute terms. It is possible that in two-spouse households nonnuclear members may be helped more than they help, but we cannot determine with our data whether this is true. However, the presence of nonnuclear members may increase the flexibility of units to accommodate changing economic circumstances by facilitating alternative market and nonmarket activities among nuclear members.

Our results lend some support to the claim that extension is related to the desire to alleviate temporarily or chronically low earnings of the primary earner. The fact that the household head's full-time employment status decreases significantly the odds of extension provides support for the economic argument. Further support for the economic argument is ob-



tained from minority households in which the earnings of nonnuclear members are found to be significantly and positively related to total household income. In contrast to minority households, the presence of nonnuclear members in majority group households has little to do with a household's income generation strategy. Why this should be true is not immediately apparent, but explanations must move beyond the realm of economics to consider nonpecuniary aspects of extended living arrangements.

In extended minority households, the earnings of nonnuclear members exert a stronger positive effect on total household income than do earnings of wives. The fact that some minority households have incomes below the poverty level in spite of the supplemental income of nonnuclear members indicates that the economic contributions of nonnuclear members do not offset the labor market disadvantages faced by minority heads. We cannot, however, determine what proportion of nonpoor families in which nonnuclear members contribute to family income were poor prior to the inclusion of those individuals, but this would appear to be another fertile area for further research. An equally promising avenue for subsequent study is the investigation of the internal reallocation of household and market work roles which is made possible by the presence of nonnuclear household members. It is important to clarify how the economic roles of secondary nuclear workers are themselves conditioned by the presence of nonnuclear members.

Our analysis is based on the *extended family household* and not the *extended family*. In common usage, the latter refers to interaction patterns among related individuals (family of interaction), whereas the former refers more specifically to the actual living arrangements of relatives (family of residence). While both types are pertinent to the investigation of economic interactions among relatives, an analysis of the monetary exchanges based on co-residence patterns tells only part of the story. Informal exchange activities among households are not outmoded forms of social organization in the process of extinction. This study has provided an empirical basis for the claim that families rely on immediate relatives or nonnuclear members within the household for support when social and economic demands are great. But assistance need not be economic to render economic benefit. Troll (1971, p. 265) has identified four aspects of kinship exchange: (1) residential propinquity; (2) type and frequency of interaction; (3) economic interdependence or mutual aid; and (4) varied qualitative measures of familism, value transmission, and affectional bonds. Each of these has potential for clarifying how formal and informal exchanges take place within and between households. For example, assessment of residential patterns can help determine how many kin are avail-

able to help, even though it does not ensure that interaction will in fact occur.

We have argued that the incorporation of nonnuclear members into the nuclear family can foster a reallocation of work responsibilities within the household. By releasing certain nuclear members from domestic duties, this permits an increase in the total amount of labor supplied to the market, provided there is need to increase income and work opportunities are available. Alternatively, additional members may contribute directly to the process of income generation by participating in the labor force and pooling resources with members of the nuclear family. The study of the nonmonetary aspects of intra- and interhousehold exchange can further deepen our understanding of the ways families make efficient and flexible use of limited resources. This is because small-scale transfers of goods and services that cannot be described as cash flows widen the immediate resource base for families. Yet nonmonetary exchanges tend to be underemphasized in research because they are not easily identified or readily subjected to quantitative analysis. Further research should include an attempt to assess the importance of such nonmonetary transfers.

APPENDIX A

CATEGORIES OF VARIABLES IN THE LOGIT ANALYSIS OF EXTENDED HOUSEHOLD STRUCTURE

Variables	Operationalization
Dependent:	
Extended, Nonextended structure (extended = 1)...	Dichotomy
Independent:	
Race/ethnicity.....	Dummies for each of the groups: Mexican, Puerto Rican, Central/ South American, other Spanish, black, non-Hispanic white
Ratio of household income to poverty.....	1 = poverty and below 2 = above 100%–200% of poverty 3 = above 200%–300% of poverty 4 = above 300%–400% of poverty 5 = above 400%–500% of poverty 6 = above 500% of poverty
Education of head.....	Continuous years of education; 0–6 years of college or more
Nativity (foreign = 1).....	Dichotomy
Full/part-time employment status (full time = 1)...	Dichotomy

APPENDIX B

MEANS AND STANDARD DEVIATIONS FOR VARIABLES
INCLUDED IN MULTIVARIATE ANALYSIS

Variables:	Non-Hispanic White		Black		Hispanic	
	X	SD	X	SD	X	SD
Total household income...	\$19,562	\$11,117	\$11,950	\$8,545	\$14,037	\$8,832
Earnings:						
Wife.....	\$1,916	\$3,815	\$1,247	\$2,971	\$1,294	\$2,877
Own children.....	\$2,259	\$4,143	\$1,923	\$4,038	\$1,562	\$3,085
Nonnuclear.....	\$2,402	\$4,872	\$1,277	\$3,343	\$1,726	\$3,373
Proportion above 150% of poverty.....	.83	.37	.51	.50	.63	.48
Interaction, above Poverty:						
Wife's income.....	\$1,862	\$3,818	\$1,117	\$2,935	\$1,192	\$2,873
Children's income.....	\$2,157	\$4,141	\$1,557	\$3,998	\$1,319	\$3,062
Other kin's income.....	\$2,299	\$4,885	\$1,142	\$3,307	\$1,556	\$3,336
Proportion female headed..	.29	.45	.53	.50	.31	.46
Proportion foreign born....	.03	.18	.02	.12	.44	.50
SEI of head.....	32.1	26.2	15.8	18.8	22.0	22.1
Children under 18.....	1.9	1.5	2.7	2.1	2.5	1.8
% receiving welfare income.....	.13	.34	.40	.49	.24	.43
Average wage.....	4.94	.74	4.94	.80	4.90	.66
Age of head.....	47.7	14.2	49.1	14.1	44.8	13.9

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Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

ON "THE ANALYSIS OF ATTITUDE ITEMS" BY PIAZZA

In "The Analysis of Attitude Items" (*AJS* 86 [November 1980]: 584-603), Piazza describes several methods of refining item clusters on attitude surveys. The central thrust of the paper is to provide methods for deleting extraneous items by examining the correlations between test items and other ("background") variables. However, I do not believe that the proposed methodology can accomplish this, nor do I believe that most of the techniques can be made to work.

I have five specific criticisms of the paper.

1. It is well known that the Pearson product-moment correlation coefficient is appropriate only for data containing interval information. It is also well known that the Spearman r_s is an appropriate statistic for some types of ordinal data. It is less well known that the Kendall τ is appropriate for ordinal data with a large respondent/scale-value ratio. That Piazza proceeds in seeming ignorance of these facts is most disturbing since these oversights highly prejudice his conclusions.

2. The above may seem only a carping criticism; after all, many people use inappropriate statistics. However, when Piazza illustrates the use of his techniques, we find him advocating the use of the Pearson r on 0-1 data (item 2, and sex)! This is statistical illiteracy.

3. Piazza recommends the comparison of attitude items with "back-

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ground" variables such as age and education; he suggests that graphical comparison of the correlational patterns may guide one to cluster some questions together. So it may, but the example Piazza uses falls very short. He recommends the rejection of two items on the basis of graphs which give no indication of confidence intervals or significance levels, graphs prepared on the basis of wholly inappropriate statistical procedures. Further, these graphs are different from each other only to a very friendly eye.

4. Also recommended are three other techniques: a function of the r 's, factor analysis, and canonical correlation. We have seen that ordinal (let alone 0-1) data cannot support an analysis based on the Pearson r ; similarly, such data will not support the multivariate techniques.

5. Of course, one might still ask, "Does it work?" Unfortunately, it does not. The analyses mentioned above lead to the clustering of items 1, 2, and 3 away from items 4 and 5. "The fourth and fifth items, on the other hand, have correlation profiles that differ not only from those of the first three items but also from one another" (p. 589). Note items 3 and 4, quoted here verbatim:

Item 3: How much do you think it is the fault of white people living today that the average black person is less well off than the average white person?

—Mostly the fault of white people living today/partly/not at all
—Scored 2/1/0

Item 4: How much do you think it is the fault of black people that they don't do as well as whites?

—Mostly black people's fault/partly/not at all
—Scored 0/1/2

To conclude that these two items are measuring different things seems to me to be an abnegation of common sense.

In conclusion, I must take serious issue with the paper as a whole. It does not appear to be correctable, inasmuch as nonparametric statistics do not admit to interpretations which allow their use in factor analysis. As for canonical correlation, I observe that although some formulations do not require multivariate normality all require well-defined second central moments. The latter depend on interval information (which 0, 1, 2 data hardly approximate). The graphical method might be made to work with appropriate statistics and tests of significance. Perhaps Piazza could address this issue.

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MORE ON "THE ANALYSIS OF ATTITUDE ITEMS"

Peace's objections to the methods outlined in my article fall into two categories. First, he raises questions about the appropriateness of using Pearson product-moment correlation coefficients. Second, he disputes the substantive conclusions reached by applying the methods. Let me offer a few comments on each of these points.

Using Pearson's r

It is true that the use of Pearson product-moment correlation coefficients is, strictly speaking, appropriate only for interval-scaled measures. I am quite aware of this fact and, indeed, I stated in my article that the procedures described there require that it be reasonable to summarize the relationships between the variables with such correlation coefficients (p. 591). I thereby sought to sidestep this issue of the appropriateness of using Pearson's r for typical social science variables so that I could focus directly on the analytic methods themselves. If an analyst is not satisfied that Pearson's r is a reasonable summary of the relationships among a specific set of variables, of course he or she should not use that statistic or use methods which depend on it. In such cases it would be possible to develop a method which uses a statistic like Kendall's τ , sets confidence intervals around each statistic, and perhaps counts the number of times that the confidence intervals of a pair of items do not overlap across a set of criterion variables. Unfortunately, there is no room here to pursue that possibility.

Perhaps what I should do, however, is to present at least some justification for using Pearson's r on the data presented in my article. In order to do so, let me mention what happens if we use another measure of association for the variables given in my example. I generated Kendall's τ for the relationships between the five attitude items and the five criterion variables, corresponding to table 2 in the original article, where Pearson's r is used. Most of the τ 's are somewhat smaller than the corresponding Pearson correlations, but the mean absolute difference is only about .02. The largest difference (.06) is for the correlation of item 1 with education (.26 vs. .32) and for that of item 5 with income (— .05 instead of — .11); in the latter case the nominal significance level of the coefficient goes from the .02 level to the .13 level. All in all, nevertheless, the two measures give the same general picture of the relationships between these two sets of variables. If we were to graph the correlation profiles of the items, as in figure 1 of the original article (p. 590), we would reach the same conclusions: the profiles of items 1, 2, and 3 are different from those of items

4 and 5. I might add that using Spearman's r_s instead of Kendall's τ results in a set of coefficients even more similar to those yielded by Pearson's r .

I do not wish to imply that Pearson's r is always a good summary statistic for ordinal social science variables. The only point I wish to make is that frequently it serves well enough for the type of exploratory item analysis that I am discussing. Matrices of Pearson correlation coefficients are much cheaper to generate than Kendall or Spearman coefficients, and the resulting matrices are always nonnegative definite (except for some special cases) and therefore amenable to further multivariate analysis. Even Pearson correlations between dichotomous variables, called ϕ coefficients, are useful summaries, provided that analysts bear in mind that a factor analysis of a matrix full of ϕ coefficients is not likely to yield substantively meaningful results, since the maximum value of a ϕ coefficient is a function of the marginal distributions of the two component items. Fortunately for us, however, Pearson correlations among a set of ordinal variables and of a few dichotomous variables can often reveal the same basic information we would get from other, theoretically more appropriate, sources.

Value of Substantive Results

One of the conclusions I drew from my analysis of the data set used in the example was that items 3 and 4 should not be considered to measure the same underlying attitude within the theoretical domain defined by the five background variables. In other words, I concluded that blaming whites for the fact that blacks are generally less well off than whites is not the reverse of blaming blacks themselves. For some reason my critic seems to think that this conclusion defies common sense. But the questionnaire allowed respondents to blame both whites and blacks mostly, partly, or not at all, and there are eight possible combinations of answers (excluding the mostly-mostly combination which was in fact empty). There is no necessity for one item to be a good predictor of the other, and as reported in table 1 of my article, the Pearson r between the two items is only .24 (Kendall's τ - β is .23).

Furthermore, the whole point of my article was to go beyond the inter-item correlations and to examine the relationships with other theoretically relevant variables. Whether we look at Pearson's r 's or Kendall's τ 's, the contrast between items 3 and 4 seems very clear. Those who say that whites are mostly to blame tend to be younger and more highly educated than those who say that whites are only partly or not at all to blame. On the other hand, age and education do not seem to be important correlates

of blaming blacks. The least likely to blame blacks are women and lower-income persons, with or without controls for age and education. Since items 3 and 4 are related quite differently to the theoretically relevant criterion variables, I concluded that it would be a serious error to ignore this fact and simply to lump the items together in some scale because they are correlated. It seemed to me that analysts would find it very useful to have some methods that could help them arrive at such a conclusion.

In summary, I do not mean to downgrade the perennial problem of analyzing ordinal social science data with statistical tools designed for interval data. This is definitely a problem that cannot be ignored. Nevertheless, even somewhat inappropriate tools can often help us reach substantively meaningful conclusions, particularly when they help us deal with large numbers of variables. Analysts generally need not reject the help of such tools out of hand but rather should seek to verify the conclusions by some other, more appropriate, means when possible. If this exchange serves to remind us of the desirability of such verification, well and good. Without some of our imperfect tools, however, we might not have many conclusions to verify.

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Review Essay: The Growth of the World System

The Modern World-System II: Mercantilism and the Consolidation of the European World Economy, 1600-1750. By Immanuel Wallerstein. New York: Academic Press, 1980. Pp. xi+370. \$22.00.

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I do not think this book is very good. But it does not explain what it is trying to do, and so does not generate standards internally according to which I can judge it to have fallen short. I will therefore consider it successively as a sociological monograph urging a theoretical argument, as a history of the 17th and early 18th century, and as a source book and guide to the literature on economic history. I will argue that it is not very good by the standards generated by any of these three visions of what the book is, though of the three it probably comes closest to the last; it is a pretty good annotated bibliography in 17th-century economic history.

A book with a theoretical argument ought to discipline the presentation of the facts so that they bear on the argument. The ratio of discussion of the relevance of the facts to descriptions should be high, so that at each point one is aware what the theoretical issue is, why something about (say) Dutch versus English tax rates is relevant to it, and what it would demonstrate if the facts were one way (say Dutch rates higher) rather than the other. For example, Jeffery Paige's *Agrarian Revolution* (New York: Free Press, 1975), on the relation between the world system and class relations in poorer countries, has long introductions to the tables to specify the theoretical meaning of highly condensed facts and is often attacked by more prolix writers for "not including" a lot of stuff relevant for explaining other sets of facts (particularly the crucial one of whether the revolution wins, which Paige does not try to predict). They will probably like Immanuel Wallerstein, who surely includes everything. But the first requirement of a theoretically motivated book is that it tell what the theory is, so as to specify something for the facts to be relevant to.

Since no theoretical argument is urged in *The Modern World-System II* I will have to make one up. Much of the book could be conceived of as providing evidence relevant to the following arguments: (1) Capitalism *grew* by its own inner logic, conquering new areas and transforming societies, rather than *being caused* by social conditions in those societies. (2) The central things that grew and that transformed societies were productive activities, in manufacturing, agriculture, mining, and transportation, serving markets in other societies, or in other words what grew was a world

system of production. (3) The growth and maintenance of this world system involved extensive activities by growing national states: encouraging manufacturers, protecting markets, fighting wars to maintain an international "order" favorable to capitalist growth—or at least favorable to their own economic advantage—conquering colonies, issuing letters of marque legitimating piracy, and so on, and the requirements for such activities encouraged the growth of state power and of the state apparatus. (4) The amount and type of constraint from the world system on both the political possibilities and the economic health of the constituent states (and of their export-oriented industries) varied with a sort of overall rank in the system, with (a) hegemony being the position of a state always winning by each of its constituent elements doing what was immediately profitable (cf. the excellent discussion on p. 39), (b) "core" states being those which dominated the system more than they were constrained, (c) semiperipheral states being those which have at least some ways of winning advantages in the system, (d) peripheral states being completely constrained by political domination and by the fewness of their economic alternatives, and (e) external states being those which did not need capitalist products or produce things capitalists needed and which consequently had neither possibilities nor constraints relevant to the world system (they would however get the constraints before the possibilities in later years).

This is a generally sensible theory. It is certainly preferable to the vague notion that industrial capitalism started among some puritan spinners in Manchester around 1800. Part of the difficulty with the Wallerstein book is that it does not state this theory and consequently does not argue for it. This in turn means that the mechanisms that make it go remain vague and ad hoc. Sometimes international influence works by English slave trade (p. 161), sometimes by English piracy (pp. 159–61), sometimes by the Dutch producing ships that can be run by smaller crews (p. 55). Sometimes worldwide depression causes the peripheral Poles, Neapolitans, Mexicans, or Prussians to increase local industrial production and production for local markets in rural areas (pp. 137–38, 147, 156, 235), sometimes instead it causes cereals production to be turned to subsistence because it cannot be sold (p. 137 on Poland), sometimes it seems to cause Mediterranean areas to turn from producing wine, which can easily be sold, to cereals which are in surplus—surely suspicious behavior in a growing capitalist system (pp. 84–85, 262, but see p. 180 for a contrary assertion). Because the theory does not have determinate mechanisms, it reduces to a general imperative for the scholar to look for world system influences, perhaps wise advice but not very specific.

Further, since the concepts are never defined autonomously from their effects, tautologous reasoning is hidden in the argument, rather than eliminated by theoretical thought. The tautologous formulation of point 4 above, that if you rank societies by the degree to which they dominate or are constrained by the world system, the "core" ones do better with mercantilist policies than the "peripheral" ones, is my own tautology. I

believe, however, it fairly represents how Wallerstein thinks about the subject, and he certainly never defines "coreness" by criteria other than national success. This means that coreness cannot be used to explain national success without going around in circles.

So if the book has the theoretical purpose I have imposed on it, it does a bad job of arguing it and builds logical circles into the middle.

But perhaps the aspiration of the book is instead simply historical, to tell the story of the growth of the world system and some of its main national components. The careful avoidance of theoretical statement and argument may be an antitheoretical statement of faith. And certainly the way Wallerstein has adopted the worst vices of historians' useless debates about "periodization" (really about when to start a book and when to stop rather than about any serious question) suggests that he has adopted historians' purposes with their vices. But the story he writes is fuzzy and incomplete.

A central criterion of a "well made history" is that when it induces in us an interest in a given cause, person, or event, it gives us a satisfactory account of how it all came out. In Emmanuel Le Roy Ladurie's wonderful *Montaillou: The Promised Land of Error* (New York: Random House, 1979, trans. and abridged by Barbara Bray from the French version [Paris: Gallimard, 1975]), we are induced to become interested in Pierre Clerge, a priest and member of a leading village family who betrays his enemies to the Inquisition. If we were not told how he died at the hands of the Inquisition himself, if he were allowed to disappear from the story with no further word, we would be left hanging. Narrative integrity, so that we know what happened to the central elements of the story, is a requirement of a good history as a "story of the growth of capitalism."

If the Dutch held an advantage because they built better ships more cheaply (p. 55; the margin seems to have been very substantial), how did England and the New England colonies come to dominate the shipbuilding industry later (pp. 91, 241, 257)? If Dutch commerce lived off its dominance in the Baltic grain trade (pp. 52-53), then when increased productivity in the low countries, Northern France, and Southeast England drove Baltic grain from the market (p. 133), what happened to the economies of scale of Dutch commerce? If the Dutch could produce some goods competitively when their wages were about a sixth higher than England's (and presumably than France's) (p. 92), what happened (if anything) to give the competitive edge in the long run to England? That is, causes of national advantage seem to operate only when Wallerstein has a quotation but to disappear without a trace in the rest of the story. We never learn why the Dutch lost out (actually, it is not clear they ever did lose out except that they were not militarily equal to England and France, so it is not clear that there is anything to explain—but that too is leaving off the end of the story). Finishing the story would require a continuous analysis of what happened to Dutch advantages. This is only one case of an incomplete story from a historian's point of view.

For another example, it is clear that a lot of Wallerstein's story has to do with very expensive governmental policies—not only wars but, for example, a bounty on wheat exports from England, whose value Wallerstein gives as about a sixth of the value of the exports themselves (p. 261), or the rescue of the Bank of England from speculative losses while the system of John Law in France was allowed to go under (pp. 282–84). We learn very late in the book that the Dutch had a much higher tax rate than the English, who had a much higher rate than the French (p. 287), who in turn taxed at a rate five times as high as the Austro-Hungarian empire (p. 277). If anyone were interested in how states became strong enough to follow mercantilist policies and win, differential legitimacy of taxes and differential effectiveness of tax collection should obviously be central. Yet we never get the story of taxation powers. In the next volume Louis XVI will have to fall while trying to raise taxes to compete with the English and the Dutch, and we will not know how his problem originated in the 17th century. The Hapsburg government of the Austro-Hungarian empire lasted a lot longer than the French one (on p. 232 Wallerstein seems to write off the Austro-Hungarians pretty early) but presumably not because of its lower taxes.

The theoretical impoverishment of the book contributes to its inability to get the whole story out. Since mechanisms are merely interpretations for particular developments, not theories of how capitalism grew, they appear more or less at random. For example, somewhere in the book nearly every kind of domestic and international condition is urged as a cause of concentration of property or business in a few hands (simply "in the 17th century in Eastern Europe," p. 134; "hand in hand with expanded . . . corvée labor," p. 138; the Long War in the English islands, p. 164; boom conditions in tobacco, p. 166; greater resilience of large producers in the face of acute world competition, p. 168; expropriation of the lands of large landholders in Sweden [!], p. 216; greater usefulness of new fodder crops to livestock owners, p. 259; low price of cereals, p. 259; cereals monoculture, p. 262). Because Wallerstein has no theory of concentration, we do not get the story of why a few sugar planters got rich while many went under, or why Dutch shipbuilders were bigger (if they were; p. 43 suggests they had economies of scale without saying they were bigger), or whether in the long run the great Swedish landed magnates were favored or undermined by Sweden's growth to a centralized "great power."

If one of the features of the growing world system is bigger businesses, we never learn that story, because there are a dozen or more stories whose connections are never discussed. And this in turn is because we have no theory of development of the size distribution of producing farms or firms.

If the book has no theory and is not coherent history, it certainly has a big bibliography, and this is extensively quoted and discussed in footnotes. The footnotes are in general the best reading in the book. We can evaluate Wallerstein as an abstracting service, telling us what to read if

we want to understand the economic history (with politics where necessary) of the 17th century.

Unfortunately I am not a very good reviewer of this aspect of Wallerstein's book because the central criterion for historiographic work is how well it summarizes the central contributions to knowledge of others' papers or books. To judge that, I would have to have read a large share of the bibliography. I have not. I will try to judge this aspect of Wallerstein's book from the internal evidence, but I would urge interested readers to consult reviews in the historical journals.

The first problem with Wallerstein's abstracts is that they are reported different ways in different places. For example, on page 132 he says that there is disagreement on whether the productivity of Polish agriculture went down when the exports were down, with some saying productivity stagnated, some that it decreased. On page 133 the decrease in exports is taken as evidence of declining productivity (see also p. 146, which says that "per capita grain production was falling"), though obviously a switch to subsistence production and local markets is an alternative explanation. On page 137 a switch to subsistence production and to local markets is asserted, without reconciling this with earlier assertions of lowered productivity. So after the abstracts we are left not knowing what the sources have actually demonstrated.

Similarly there are repeated assertions that in Eastern Europe the exploitation of legally coerced serf labor increased as a response to lowered grain prices (pp. 138-39), though the increased subsistence production mentioned above makes this assertion suspicious, and the increases in profits from selling drink to the peasants suggests that one could not simply coerce the surplus value out of them (p. 141). But it turns out that in the areas most influenced by export markets legally coerced labor was not used (p. 140), that serfs worked for wages as well (p. 235), that the *corvée* was reduced in Bohemia in 1680 (p. 232) after the increase around 1621 (p. 139), and the only specific fact on the number of days of obligatory labor (p. 226) refers to Prussia and gives the *corvée* as six days a week, leaving no time for subsistence production or for wage labor (p. 235).

There are other examples of unresolved discrepancies, where the nature of the evidence which has led the authors cited to their divergent conclusions is not described. Sometimes the sources are answered with rhetorical questions (e.g., p. 136, n. 39), which may lead the careless reader to believe the source has been refuted when no evidence has been offered. So as an abstracting service the book has its flaws.

However, the bibliography is copious (pp. 291-349), and in the footnotes (on the 10 pages 214-23, 39% of the space occupied by print is in footnotes, a typical ratio) one can get an idea in English of whether it will be worth struggling through the French, German, Spanish, and Italian sources. The book will render the historical materials more accessible for people who read the appropriate languages.

Perhaps the deeper question for a review of this book is whether anyone should have attempted to write either the theoretical treatise on the origin of the capitalist world system, or the story of the world system in the 17th century. To say that Wallerstein does not have the power of intellect to do these mammoth jobs sort of implies that someone else does. While I could name people I think are more likely candidates than Wallerstein (e.g., Hobsbawm or Le Roy Ladurie), I am not sure I would urge them to try. It is not clear to me that there is a coherent and determinate pattern of growth toward a final world system with defined characteristics, so that one *could* have a theory of the growth of world capitalism. And it is not clear to me that the international influences which Wallerstein documents piecemeal are of such a character as to make economic and political developments in the 17th century around the Atlantic sufficiently unified to be part of a common story. Are Sweden's and Prussia's and Venice's histories in the 17th century subject to sufficiently similar influences so that they belong in a chapter on the semiperiphery? Are Polish exporting landowners and Mexican silver mines really tied together closely enough to constitute a history of the periphery? And are the interdependencies of such a character as to add up, even in the hands of a master, to a theory of the development of the world capitalist system?

The basic notion of a system is one of aggregate dynamics. It is not sufficient for the purpose that Spain ruin Mexican silver mines because taxes are higher on Peruvian silver, unless we can show that the Spanish colonial apparatus is responding to a fluctuation of the world economy or polity as a whole and that other subsystems situated like Spain are also subjecting economic development to a political squeeze. Further, for a science of world systems to be *new*, it has to say something more than that an increase in productivity in grain growing in Northwestern Europe will drive down the prices of grain exports from Poland. We already had the economics of international trade.

Presumably the way social systems generally affect their subparts in a common way is by posing them with comparable problems, which then have a limited number of reasonable solutions. If the subparts are a hegemonic state, core states, semiperipheral states, and peripheral politicoeconomic systems (often not satisfying the definition of a state), then the world system has to pose each category with comparable problems. The solutions should resemble each other (in crucial respects) within the categories, so there is some use to knowing that the problems were comparable.

Clearly Wallerstein's emphasis shows that he thinks the problems have something to do with the profitability and developmental potential of a given niche in a world productive system and with the political and military capacity to defend or conquer that niche. The basic proposition is perhaps that profitability, political capacity, and military power tend to covary in the 17th century, each being partly caused by the others, so that they can be summarized by a single ranking from hegemonic power to periphery. While Wallerstein has not demonstrated this, it is hardly dif-

ferent from what Samuel P. Huntington and, earlier, Kingsley Davis have argued. In fact it is rather like those tedious statistical studies that show that when you exclude enough ex-colonies (usually we have to exclude oil exporters, Oceania and Canada, Israel, communist countries, Hawaii, Puerto Rico, and Alaska, in order to make dependency theories work, for example), ex-colonies are having a hard time getting as rich as Western Europe. The reason one has to write a big book instead of a short statistical paper in a political science journal is that the United Nations did not collect the statistics in the 17th century.

When Marx alleged that capitalism was a system, he argued that the aggregate dynamics of a falling rate of profit, for example, posed a series of problems to capitalists that made them behave in one of a few common ways (substituting capital for labor, increasing the rate of exploitation). The other parts of the system (landowners, proletarians) are also faced with problems distinctive to their positions, resulting in a common pattern of attempted solutions. The clash of attempted solutions in the class struggle constitutes an evolutionary push on the system as a whole, so that in succeeding epochs the problems posed and solutions sought are different. *That* is what a theory of a system looks like, *not* naming poor weak countries "peripheral," rich strong ones "core."

I am very doubtful whether the world system posed similar enough problems to Northern Italy, Prussia, and Sweden to produce similar tendencies in the "semi-periphery." And it seems that New England was semiperipheral only because it was later to become an industrial power. Rich peripheral Jamaican planters lording it over London society seem to me in a different situation than peripheral Polish nobles faced with declining grain prices. The hegemonic Dutch East India Company losing money for a century or so (making it up in the Baltic) does not immediately suggest its own analogue in 19th-century hegemonic Britain. If the world was a system at all in the 17th century, and I suppose it was, it was a ramshackle affair with a lot of random motion and ill-fitting parts going in their own orbits, banging into each other occasionally to produce sporadic systemic behavior. According to Wallerstein's argument, the only demonstrated similarity in the solutions is that whatever problem an economy is confronted with, concentration of ownership is the solution.

The fact that Wallerstein has failed to carry out either the sociological or the historical tasks I have posed for him in this review does not constitute proof that no one could do it. But I am doubtful whether the world in those days was sufficiently tied together causally so that it can be treated either in a single theory of social change or as a single chronological narrative. But at any rate, if it is possible it will take more intellectual self-discipline and systematic thought than Wallerstein has applied.

Book Reviews

The Centralist Tradition of Latin America. By Claudio Véliz. Princeton, N.J.: Princeton University Press, 1980. Pp. xii+355. \$22.50 (cloth); \$9.75 (paper).

Enrique A. Baloyra

University of North Carolina at Chapel Hill

Claudio Véliz identifies four major elements present in Northwest Europe and lacking in Latin America: (1) checks on monarchical rule that evolved from medieval precedents (p. 23); (2) a nexus between religious nonconformity and libertarian movements (p. 190); (3) a national revolution led by peripheral actors resulting in a model markedly different from the *ancien régime* (pp. 150–54); and (4) an industrial bourgeoisie unwilling to become aristocratic (p. 242).

The specificity of these differences between regions must be gleaned from the text since Véliz, somewhat apologetically, presents his argument stressing the absence of these elements from the sociology of national development of Latin America (pp. 3–5). More important, Véliz does not clarify whether he is making an argument about subject or predicate, and he refuses to define centralism (p. 7), the key concept in his analysis. This refusal leads to two basic flaws in *The Centralist Tradition of Latin America*.

First, centralism becomes subject, predicate, and too many things altogether: “style of political behavior” (p. 3); “secular disposition” (p. 3); “tradition” (p. 6); “rational mode of domination” (p. 7); “bureaucratic tradition” (p. 7); “mood . . . supported and strengthened precisely by a political tradition that has always been centralist” (p. 9); “efficient form of centralized government” (p. 10); “structure of government” (p. 150); “temper” (p. 236); and “pragmatic tradition devoid of ideology, a style of ordering society perceptible in practice, not in theory” (p. 301).

To be sure, a centralized form of government has become paradigmatic in Latin America, but instead of linking together the different elements of the *Castilian* tradition of government (pp. 16–17, 31–32, 34–45, 52–57, 64–67, 72–73, 83–87, 126–27, 228–32), he immerses them in a “vast centralist social conglomerate” (p. 12). In this he assumes an almost perfect congruence between norms and institutions, and among institutions, that is rarely found in historical reality.

The second flaw results from this and cannot but obscure the “inverse relationship” between centralism and the four absent factors that constitutes the backbone of his analysis. Véliz argues a good case, but the lack of a causal scheme leads him astray. Dealing with center-periphery rela-

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tions, he suggests that feudalism has never existed in Spanish America because the bureaucratized Castilian monarchy would not have transplanted to America something that had ceased to have political relevance in the Peninsula (pp. 26, 33–38, 43–47); that Spanish regalism, assisted by a Church under firm Crown control (pp. 37–42), prevented *conquistador* and *encomendero* elements from institutionalizing feudalism (pp. 47–68); that Bourbon (pp. 85–88) and Pombaline (pp. 103–14) recentralizations neutralized peripheral challenges; and that the liberalization of trade responded to central, not peripheral, impulses (pp. 126–40).

Véliz remains convincing in arguing that postindependence anticlericalism was essentially nonreligious (pp. 192–94); that neither positivism and the Religion of Humanity (pp. 196–201) nor religious nativism (pp. 201–6) represented religious dissidence; and that although, until independence, the Church followed a policy of pragmatic “latitudinarianism,” this did not result in a proliferation of dissidence (pp. 207–15).

Concerning the national and industrial revolutions, Véliz repeats some familiar assertions: independence resulted from a collapse of the center (pp. 140–43) and preceded nationalism (pp. 163–64); nationalism came closer to enlightened despotism than to revolutionary ideology (pp. 153–54); economic liberalism delayed the emergence of an industrial bourgeoisie (pp. 182–84); Latin American urban culture is pre-industrial (pp. 223–36); in Latin America, industrialization was a response to external circumstances (pp. 251–56); a *bourgeoisie d'affaires* has always played a key strategic role in this scheme (pp. 264–76).

But once he has established these differences, Véliz does not know what to make of them except in relation to centralism: “The absence of [religious] nonconformity is an additional manifestation of the centralist temper of society” (p. 191); “the cities of Latin America . . . are yet another faithful reflection of her centralist temper” (p. 236); and “the persistent centralist bias *in all other aspects* of Latin American life also influenced economic arrangements” (p. 237, my italics).

This is gratuitous since, in his concluding chapter, Véliz is still unable to indicate which aspect of the vast social conglomerate is most relevant. In addition, he continues to perceive centralism as “as much a consequence of accidental external circumstances as of deliberate choice” (p. 301). This is frustrating since the author maintains that, until now, “the centralist tradition has been latitudinarian, legalistic, and emphatically civilian” (p. 302), although it has now emerged “under undisguised authoritarian control” (p. 305).

In summary, *The Centralist Tradition* is disappointing not because it misses as grand theory but because having covered so much ground, having exposed so many misconceptions, and having clarified so many important points it fails to make a positive contribution. If the book is addressed to those “intellectual and political leaders [who] have not resigned themselves to being what they are” (p. 12), it is difficult to see how they would benefit from what is basically a disorganized, confused scheme. Broad and vague

notions may indeed exert a powerful influence on actual behavior, but they are not very helpful if the intention is to clarify why Latin American government has remained centralized and how this style of government could be reconciled with democratic institutions. The inventory of reasons provided by Véliz includes some very powerful and intriguing ones, but he has given himself too much latitude. In addition, he appears to have fallen for the role of hedgehog in relating everything to a single central vision.

Dependent Development: The Alliance of Multinational, State, and Local Capital in Brazil. By Peter Evans. Princeton, N.J.: Princeton University Press, 1979. Pp. xviii+362. \$20.00 (cloth); \$5.95 (paper).

Joseph A. Kahl
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In the past decade, many scholars attempting an integrated view of the process of industrialization in Latin America have used the paradigm of dependency. The central thrust of this perspective, most clearly enunciated in the writings of Brazilian scholar Fernando Henrique Cardoso, is that the larger countries of the region have moved from a state of "classic dependency," which received its impetus to growth from the export of agricultural and mineral products, to a new stage of "dependent development" based on the internal expansion of industrial production. The new stage began as a drive for local manufacture of consumer goods previously imported, and is expanding and diversifying in many directions. Originally it was hoped that the countries would gain in autonomy—not autarchy or separation from world markets, but autonomy in the sense that the dynamic for economic growth would be internally generated and that social and political independence would gain strength. That hope evaporated as it became ever more clear that the multinational corporations were expanding their role in local production, while simultaneously international finance was increasing its control over local decisions.

The new economic diversification produced a new structure of national power: a mix of local entrepreneurs, the managers of multinational subsidiaries, and government bureaucrats who attempt to manage the economy and to participate in it through publicly owned enterprises. This triple alliance, an uneasy partnership of groups that share basic interests but compete for relative advantage, is the subject of Peter Evans's book, *Dependent Development*.

The paradigm that Evans adopts is not new, but the data he presents on Brazil are innovative and important. Instead of merely presenting economic history on a national level, like most other writers on the subject, he goes beyond it with interview material from many executives that locates their decision making in the concrete realities of the national and international situation. Economic rationality is not the same for leaders in the three seg-

ments of the alliance. Each must follow the bounded rationality (in Herbert Simon's sense) of its own organization. The multinationals seek profit maximization on the world level, which usually leads them to monopolize research and development for product innovation in the home country, a key element in the continued technological dependence of Brazilian industry. Local firms, unable to develop their own technology, can concentrate on commercial skills or political contacts to maximize advantage in certain areas (such as textiles or beer or exploitation of natural resources), or they can seek multinational partners to aid them or even absorb them, as happened to most of the firms in the pharmaceutical industry, which Evans studied in particular detail. And the government moves in to supply entrepreneurship and capital for huge long-term projects that are crucial for other industrial growth, such as energy or steel or petrochemicals. Recently, it has occasionally done so by forming companies that are jointly owned by local capital, foreign firms, and the government itself.

Evans discusses a number of separate industries, giving both the overall pattern and the case histories of particular firms and clusters of firms controlled by the same financial group. He often traces them from birth to expansion to multinational partnership, and sometimes to extinction. He has a fine hand at combining case material with theoretical relevance. In general, he demonstrates that the multinationals are strongest in the areas of fastest growth based on new technology and large investment, and that the locals must learn to survive either as junior partners or as leaders in smaller and more traditional industries. And the government finds itself becoming the major source of local investment, despite a commitment to the ideology of private enterprise. Only the increasing power of government allows a form of balance among the three partners that keeps national interests from being totally submerged.

The political and social implications of this style of development are clearly stated, although they are not the central focus of Evans's work: the new, imported technology is capital intensive, and the industrialization it produces is unable to provide enough good jobs for the many people who move to the rapidly expanding cities. The goods produced by the new factories are aimed at affluent customers who can buy refrigerators and automobiles, so a concentration of income and credit that increases inequality is good for business. The government gains its legitimacy from growth, and thus shares with local and international capitalists an intense desire for rapid accumulation and investment. The increasing need for foreign finance (both for industrial investment and to cover trade and government deficits) requires orthodox policies to buttress external confidence. The masses are marginalized from both production and consumption, therefore "to allow them political participation would be disruptive. Social and cultural exclusion follow from political and economic exclusion" (p. 29).

Evans has written the best book I know describing the new style of dependent development in a single country. And he links the story of Brazil with the general theories of imperialism and dependency, as well as with

short but useful comparisons with Mexico and Nigeria. He analyzes the structure and functions of the triple alliance in Brazil, and outlines problems for the future that emerge from certain contradictions among the elite partners, and from difficulties in world trade. Some writers use the concept of dependency as a guide to statistical comparisons among many countries; here Evans demonstrates its power as an organizing perspective that integrates the empirical study of the firm with an analysis of the transformation of a national economy in the context of international capitalism.

The Patrimonial Foundations of the Brazilian Bureaucratic State. By Fernando Uricoechea. Berkeley and Los Angeles: University of California Press, 1980. Pp. xvii+233. \$16.50.

David Parker
University of Washington

In the portion of *The Patrimonial Foundations of the Brazilian Bureaucratic State* that is based most directly on evidence, Fernando Uricoechea uses Max Weber's terminology to discuss the relatively short-lived Brazilian national guard (1831-73). He arrives at the tenable conclusion that the organization was administered and functioned according to patrimonial norms: that national guardsmen were rewarded with honorary distinctions rather than money, that they held their offices as prebends, and that there was a clear emphasis on liturgical services. The author's purpose, however, goes far beyond the testing of Weberian theories or the analysis of Brazilian bureaucracy. The broad scope of the work explains much about the relatively little space given to the analysis of the national guard and the need to rely on assertion rather than evidence for much of the support of the thesis.

The book's argument—in abstract terms—is both clear and plausible. Uricoechea contends that the law that (with certain exceptions) made all voters, some 200,000 men, liable for unremunerated national guard duty constituted a major step in the militarization of Brazilian society. He thus views the study of the national guard as synonymous with the study of Brazilian society. He then goes on to suggest that by making these men liable for military duty, the government was extending its control over parts of the society that had, until then, been beyond its control. By 1873, he argues, local authority had been successfully incorporated into what was becoming a modern, bureaucratic state. The persuasiveness of this argument depends on a number of critical factors: a functional and testable definition of "militarization" and evidence that militarization in that sense increased after 1831; a testable definition of "centralization" and "rationalized" (the terms in which the author discusses "bureaucratic state") and evidence that Brazil was in those senses more bureaucratic in 1873 than it was in 1831 (the outside dates for which he has systematic evidence); finally, it re-

quires a working definition of "society" that makes explicit the sense in which discussion of the national guard and the way it was administered can be understood as a discussion of what the author terms "nation building."

Uricoechea's evidence of militarization is generally circumstantial. Because colonial fazendas had to defend themselves from Indian raids, Uricoechea concludes that the latifundia were militarized (p. 18). Because over half the budgets of four states was devoted to defense in 1822–23 (the unacknowledged high point in Brazil's struggle for independence from Portugal), he concludes that militarization had increased. None of this, however, projects the degree of militarization that is manifested after 1831, according to the author, by the fact that some 70% of those who signed "popular" representations from even the remotest points in the empire identified themselves by military rank. His argument for change, at least in theory, could have been strengthened by comparisons: of petitions signed in 1831 or before with some signed in 1873 or after, for example. Unfortunately, his portrait of the national guard is that of a static and generally passive institution that comes into reluctant existence and disappears without a whimper—all at the stroke of a pen. Nor does he give evidence of any anger on the part of the co-opted local power structures. They too are passive creatures.

The work's problems regarding evidence go beyond the understandable difficulties involved in proving every point in an argument. Quite often, the author's evidence seems to be making a point opposite to the one intended. Having described the landlords as powerful opponents of the central authority, he quotes a national guard officer's reference to them as "landlords little experienced in military service, living in more or less outlying areas, busy with their agriculture and business" (p. 100). Having argued that Brazilian society was militarized, he does not explain the implications of the continuing and often successful efforts of many to evade military service—including the willingness of some to endure imprisonment rather than serve (pp. 202–3, nn. 4 and 5). He quotes a government official as saying that "as soon as the Government has available enough force, it will stop the onus," and then proceeds directly to assert that "it is clear that these liturgies [military duties] were not simply contingent on the availability of governmental forces" (p. 67). A consistent theme in this study is the implication that 19th-century Brazilians either did not know what they were doing or did not mean what they were saying. Failure to take these Brazilians seriously leads the author to interpret the events in which they acted in terms directly opposite to the terms in which they themselves saw matters. Nineteenth-century Brazilians saw the creation of an unpaid, locally controlled national guard in place of the regular, centralized, institutionalized military as a triumph of liberalism, of decentralization, and of democracy as they understood it.

But Uricoechea goes beyond not taking 19th-century Brazilians at their word. He views them with contempt: "Monstrosity," he says, "may be defined as a mode of being, the essential changes of which are contingent and

accidental." He then continues: "If we accept this definition we can readily apply it to patrimonial bureaucracy" (p. 43). It is one thing to suggest that the administration of a bureaucracy followed no detectable pattern or even an erratic pattern as far as the author is concerned. It is quite another to find it useful to label such a phenomenon a "monstrosity." This work seems to belong less in the category of Weberian analysis or in that of 19th-century Brazilian social history than in the category of works that seek to explain the emergence of Brazilian "militarism" and its "monstrosities" since the coup of 1964. I suspect it is a work of the heart.

On History. By Fernand Braudel. Translated by Sarah Matthews. Chicago: University of Chicago Press, 1980. Pp. ix+226. \$15.00.

Guenther Roth
University of Washington

Over the past decade numerous translations of major authors of the *Annales* school have been published in English, writings by Marc Bloch, Georges Duby, Lucien Febvre, Jacques Le Goff, and Emmanuel Le Roy Ladurie. A large part of Fernand Braudel's research is also available: the two revised volumes of *The Mediterranean and the Mediterranean World in the Age of Philip II* and the first volume of *Capitalism and Material Life*, together with his own English summary of this three-volume work, *Afterthoughts on Material Civilization and Capitalism*. In addition, several volumes of translations from *Annales* have appeared. One of them, *Economy and Society*, edited by Peter Burke (New York: Harper, 1972), contained Braudel's most succinct statement of his general perspective: "Histoire et sciences sociales: La longue durée" (1958).

The dozen mostly occasional pieces in Braudel's *On History* do not add much substance to this one essay (included therein), but the University of Chicago Press seems to have been in a hurry to put out another *Annales* volume. The book was published in 1980 together with Duby's *The Three Orders: Feudal Society Imagined*. The latter contains an appalling number of misprints; the former, too, is without proper editing. In the text and index one author is split into three, two authors are made one, and a "great textbook" on universal history by an unidentified Weber is mentioned without its author, Georg Weber (1808-88), being distinguished from Alfred or Max Weber. There are erroneous first names and dates, and the surprising claim that Braudel became "president of the Ecole Pratique des Hautes Etudes in 1937" (p. iv). The faults, however, go deeper and lie on both the French and the English side. The volume was originally put together by Polish and Spanish followers, who were presumably eager for an amplification of the master's voice. In 1969 Braudel allowed a French edition to appear, adding a somewhat defensive preface to his "thoughts of

yesterday." In the selections, Braudel's message of the integration of the social sciences under the leadership of history is reiterated in a single-minded and repetitive fashion. This goes to the extreme of printing identical passages (pp. 47-50 and 77-79). The articles address themselves mostly to the French situation in the 1950s. Thus, they have little to say to American "new social historians" and American social scientists in general. For the English language edition it would have been better to make a more rigorous and up-to-date selection and to provide a perspective in an editor's introductory essay.

Apart from the well-known "History and the Social Sciences," the volume contains a two-page extract from *The Mediterranean*, the inaugural lecture of 1950, a brief article entitled "Unity and Diversity in the Human Sciences," the chapter "History and Sociology" from Georges Gurvitch's *Traité de sociologie*, a short article on historical economics, and reviews of Pierre Chaunu, Maximilien Sorre, Otto Brunner, Ernst Wagemann, Alfred Sauvy, Louis Chevalier, and even the young Marvin Harris—social and economic historians, anthropologists, ecologists, and demographers. The book concludes with a lengthy essay, "The History of Civilizations," from the *Encyclopédie française*, in which Braudel treats Guizot, Burckhardt, Spengler, Toynbee, and Alfred Weber.

In these articles Braudel frequently comes across more as a "social science czar" than as a working historian. He writes as a self-conscious spokesman of a school and from a position of extraordinary institutional power as simultaneous head of several institutions. It is questionable whether today anybody would want to read most of these pieces if their author had not written *The Mediterranean*, edited *Annales*, presided over the Sixième Section of the Ecole Pratique des Hautes Etudes, directed the Centre de Recherches Historiques, and organized the Maison des Sciences de l'Homme. Apart from their datedness and politics, these essays are likely to present American social scientists with other difficulties. Too often rhetoric and declaration take the place of methodological discussion and historical analysis. There is too much illustration and too much metaphorical imagery. More names are mentioned than there are pages in the volume. Braudel likes to make points by dropping names. Many of those whose names he drops must be unfamiliar to American readers; some of them he himself quotes secondhand.

He makes much of the need for a rapprochement between history and sociology, the only two "global" sciences. But sociology is for him represented by the legacy of Durkheim and by Georges Gurvitch, his late colleague in the Ecole. That, too, is likely to puzzle American readers. In rejecting American social science, Gurvitch advocated a "global," "dialectical," and holistic sociology of "total social phenomena" that has not exerted any influence in the United States. Braudel argues in a friendly and intimate way with Gurvitch; in fact, the duplicated pages in the volume reiterate verbatim his critique of Gurvitch. The points of contention, however, do not emerge clearly. Matters are not helped if the questioning reader turns to

Gurvitch's rejoinder, reprinted in Georges Balandier's memoir *Gurvitch* (New York: Harper, 1974], pp. 81 ff.).

When Braudel wrote the essays, the dominant American and British development and modernization literature of the 1950s and 1960s was still beyond his ken. There are only passing critical references to the beginning of postwar area studies. The reaction against this literature (with its functionalist orientation) in the United States and abroad in the sixties and seventies was closely connected with the resurgence of Marxism and the turn to the "new social history." Braudel's name lent itself as a war cry even for Marxists—witness Immanuel Wallerstein's Braudel Center at SUNY, Binghamton. But today these very developments may make Braudel appear as a forerunner left far behind.

Is there, then, nothing positive that can be said about the reprinting and translation of these essays? There is. They are useful for readers interested in the workings of Braudel's mind and in the French academic scene in the wake of the Second World War. Also, I for one appreciate very much Braudel's literacy in several languages, and his voracious appetite not only for the facts of material life but also for all of European historiography from the 18th to the 20th century. For me Braudel is not so much a forerunner of a new dispensation as a genuine successor of the great historians who preceded him, many of whom were also energetic organizers and intrepid partisans—much weaker on theory than on practice, and properly so.

Entrepreneurship: A Comparative and Historical Study. By Paul Wilken. Norwood, N.J.: Ablex Publishing Corp., 1979. Pp. xiv+306. \$22.50.

James E. Schraeger
University of Chicago

Little is published about entrepreneurs, and Paul Wilken's new book is a welcome addition to a sparse literature. *Entrepreneurship* consists of an excellent synthesis of the literature, a conceptualization of the role of entrepreneurship in economic growth, a well-documented historical review of the economic development in six countries, and an ambitious attempt to construct a model to determine the causal significance of entrepreneurship in the economic development of the countries selected for study.

In his literature synthesis, Wilken divides the literature into economic, noneconomic (sociocultural), and psychological factors affecting the emergence of entrepreneurship. His review of noneconomic factors is especially complete and includes such topics as the legitimacy of entrepreneurship, social mobility, social marginality, social integration, security, and ideology. In his section on psychological factors, he discusses both the merits and weaknesses of David McClelland's need-achievement theories. Also mentioned is Everett Hagen's work, based on the withdrawal of status respect as the motivating factor for entrepreneurs.

Wilken conceptualizes entrepreneurship as "a role which involves combining factors of production to initiate changes in the production of goods" (p. 75). He divides the role into three major characteristics: the identity of the entrepreneur, the type of entrepreneurship, and the degree of change. Through this definitional typology, he applies his model to the historical literature.

The model is built around the idea of studying societies in which economic opportunity conditions have varied, and comparing the resultant growth rates in these countries to determine the causal significance of entrepreneurship. The conditions under which a negative causal chain would be discovered are stated as follows: "A low degree of economic growth and development in a situation in which the economic opportunity conditions are relatively favorable exemplifies entrepreneurship's negative causal influence and the existence of noneconomic factors hindering entrepreneurial emergence" (p. 54).

Wilken then develops a carefully footnoted historical narrative of six countries: Great Britain, France, Prussia-Germany, Japan, the United States, and Russia. The years under study range from 1750 to 1927 with certain periods within those years carefully selected for each country.

From his six case studies, Wilken concludes that entrepreneurship "was of little causal significance in the industrial transition in these societies" (p. 253). This seems to mean that the act of bringing together the factors of production to initiate changes in the production of goods was an insignificant part of the economic growth in the country under study. But Wilken is telling us that his comparisons across six countries, looking at the favorableness of opportunity conditions versus the economic growth rates achieved, show that the effects of the entrepreneurs who took advantage of the favorable climates or who stayed away from the unfavorable ones were insignificant.

Wilken presents two other findings. First, the extent of favorable economic conditions revealed a high degree of multicollinearity with the extent of favorable noneconomic conditions. Both types had to be favorable before entrepreneurship appeared. His other finding is that the actions or inactions of governments are crucial to the fate of entrepreneurship in a society.

Readers may disagree about the adequacy of Wilken's model, depending on their preferences for quantitative operationalization versus reasoning by analogy. For those who are looking for a quantitative interpretation of this elusive concept, entrepreneurship, the book charts a significant analytical path. For those who think that aggregation mixes too many apples and oranges, or that the topic itself requires more of a social psychological cast, the book may appear misdirected.

Although anyone in the subject area will benefit from picking up Wilken's book, I would take issue with a number of methodological and conceptual aspects of the model presented. On technical grounds, the basis on which Wilken calculates the proportionality of the economic opportunity conditions versus the achieved economic growth rate is not clear enough. I have

a related criticism: it is difficult to draw lines concerning the interaction of entrepreneurs and their economic environment. With regard to the definitional issue, Wilken does not apply his carefully elaborated typology to indicate who is in and out of his model.

A counterintuitive finding in applying this model is that entrepreneurship had little causal effect on economic growth in the six countries discussed. In line with my reservations about the ambiguities in the analysis of economic opportunities versus economic growth, and about the question of interaction, an alternative explanation for his finding could be that it is an artifact of the measures employed.

On balance, this study of entrepreneurship is a welcome addition to the literature. Regardless of his measurement ambiguities, Wilken's book provides an excellent foundation on which to build further studies.

Armies and Societies in Europe, 1494-1789. By André Corvisier. Translated by Abigail T. Siddall. Bloomington: Indiana University Press, 1979. Pp. ix+209. \$15.00.

M. D. Feld
Harvard University

In the 1930s the Dublin Zoo was celebrated for its success in breeding captive lions. Word got to Berlin, and a delegation of German curators of the major cats was sent to Ireland to investigate the situation at first hand, and to determine the causes and instruments of this achievement. "Basically, it's very simple," said the Dublin lion keeper. "It's a matter of grasping the philosophical principle of the lion."

"And what is that?"

"Every lion is different."

André Corvisier's book, *Armies and Societies in Europe, 1494-1789*, is motivated by the principle that all armies are different. This fact is sufficient for the composition of a fair-sized book. In the period Corvisier covers, Europe was made up of a dozen or so national systems, every one of which was unique in its military problems and the policies and attitudes they engendered.

On the other hand, Corvisier seems to proceed under the assumption that the function of politics is to overcome this diversity, to impose a rational superstructure on the messiness of everyday life. Nation-states begin, in modern times, as agencies of convenience, administrative systems designed to impose discipline and fiscal responsibility on the predatory and disorganized tradition of socially sanctioned violence, otherwise known as feudalism. Military systems are, in effect, the resolution of this tug-of-war between political order and social violence. In essence, armies differ in kind because no two societies are quite alike. They do, however, acquire a generic

or family resemblance through the fact that the problems of their political management and control are pretty nearly universal.

The special significance of the period covered by this book is that it comprehends the fateful course whereby the state evolved from a narrowly functional superstructure into the vitalizing core of society itself. In the process, the relics of the feudal aristocracy who made up the professional officer class lost a great deal of their prestige and authority. Service in the ranks, on the other hand, which had been the most onerous of social impositions, was, with the politicization of society, progressively accepted as an honorable social obligation. In short, as the status of the original military caste declined, the social acceptance of military service itself was sharply enhanced.

Corvisier does not so much explain this process as chart its progress with the aid of selected historical accounts and random statistical gleanings. His data are, of necessity, limited by the role and weight military glory and tradition have acquired in the historical consciousness of the nations in question. The data are plentiful for France and Germany (Prussia), but rather disappointing for the rest. Spain, England, Denmark, the Netherlands, Sweden, Austria, Poland, Russia, and the Ottoman Empire appear in only the most limited of supporting roles. Such selectivity tends to distort the picture. For the first half of the period covered (i.e., 1494–1650), Spain, the Netherlands, and Sweden possessed the most successful and widely imitated of the European armed forces. Its Franco-Prussian tilt makes this book most useful for the period between the Treaty of Westphalia (1648) and the Fall of the Bastille (1789).

A more serious criticism is that Corvisier is dominated by a bias that favors the study of collectivities. The prosopographic approach is utterly neglected. Individual military leaders and organizers are rarely mentioned in the text, and their biographies are conspicuously absent from the appended bibliography. This is to be regretted. There has been some splendid historical research along these lines in recent times, some of it by authors whose more impersonal publications have been used by Corvisier. But he makes no mention of the major works of these writers. I have in mind Alexandre van der Essen's biography of Parma, Golo Mann's of Wallenstein, Michael Roberts's of Gustavus Adolphus, and Thomas Barker's of Montecucoli. These archetypal robber barons were pioneers in tactical and logistic reforms that determined the size, composition, and use of armies for succeeding generations. All these books are rich in the kind of statistical data Corvisier obviously cherishes. Their omission must be a matter of principle.

The book suffers thereby. As its conclusion makes clear, armies were essentially political instruments. If we are to confine ourselves to the study of its empirics, politics becomes an account of who gets what, where, when, and how. This is precisely the area the book does not investigate. Corvisier actually skirts some of the issues. If the prestige of the military was declining by the end of the 18th century, why were members of the bourgeoisie so anxious to get into it? And why were special measures needed

to keep them out? Why was it easier to gain noble status through a military career in the 17th century than in the 18th? Without the use of case histories, such questions cannot be answered.

We should also be investigating the manner in which military careers were tied to political objectives. To what extent were many of the new states (e.g., the United Provinces, Sweden, Prussia, and Austria) the creations as well as the creators of their military systems? Their military prowess at least does seem to antedate their political prominence. These questions are central to the subject of this book, and they can be answered only if we assume that the leaders of the military systems in question were self-conscious and purposeful individuals. To do this we have to know who and what they were.

Then, too, there is evidence of careless translation and editing. The Chevalier Bayard (1473–1524) could hardly (p. 185) have been one of the knights of Charlemagne (ca. 742–814). Le Pays-Bas, though literally translatable as the Netherlands (p. 203), is actually Belgium. Only specialists will recognize the United Provinces (p. 204) as what we now call the Netherlands.

Poverty and Inequality in Common Market Countries. Edited by Vic George and Roger Lawson. London and Boston: Routledge & Kegan Paul, 1980. Pp. xii+253. \$27.50 (cloth); \$15.00 (paper).

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This is one of a growing number of books on comparative social issues and social policies, and as such is a welcome addition to an important but problematic field. *Poverty and Inequality in Common Market Countries* presents informed reviews of available data for six nations (United Kingdom, Vic George; Belgium, Jos Berghman; France, Adrian Sinfield; Ireland, Séamus Ó Cinnéide; Italy, David Moss and Ernesta Rogers; West Germany, Roger Lawson) on a series of specific issues: inequality of income and wealth; definitions of poverty; incidence of poverty according to age; disability, unemployment, sex, family status, residence, and occupation; redistribution effects of taxes and social benefits, especially housing, medical care, and education; and take-up rates for various programs.

Since new data on these issues were not developed specifically for this volume, direct comparisons among the six nations are possible only to a limited extent. It is in fact quite difficult to get a firm grasp on the similarities and differences among the nations from this volume, not only because the data are not directly comparable, but also because Vic George and Roger Lawson allocated a chapter (or about 30 pp. of text) to each nation and gave each author a relatively free hand in organizing the material. For-

unately, the result of this organization of the material is also a relatively holistic and broad account for each country, with generally careful attention to strengths and weaknesses in the data, for which the authors and editors should be commended. This is a major strength of the volume and distinguishes it from other comparative efforts. On the other hand, like most other comparative studies, the accounts are necessarily incomplete and at times superficial because of space limitations. In particular, it is quite difficult to get a good overview of the various social programs in each country. I found it most useful to refer frequently to the U.S. Department of Health and Human Services, *Social Security Programs throughout the World, 1979*, Research Report no. 54 (May 1980), to help keep the discussion in perspective. It would have been very helpful if the editors had attempted to do something similar for the various social policies considered.

George and Lawson recognize most of these problems and attempt to summarize the major conclusions in a short, final chapter. Briefly, they conclude that inequality in most of the countries is greater than is usually recognized, and that economic growth and the extension of social benefits in recent years have not had much impact on the degree of inequality (with the apparent exception of Ireland), although absolute poverty has been reduced considerably. They also emphasize that the bulk of social programs in most of the nations benefit those above the poverty line(s) more than those below. These arguments are complicated by the serious problem of obtaining a satisfactory definition of poverty in any of the nations, let alone a definition that is comparable across national boundaries. Thus examinations of the percentage of citizens living in poverty are of problematic validity. The book also points to persistent problems of poverty among female-headed families and rural populations, whereas the fates of the aged, disabled, unemployed, and large families differ somewhat among the nations.

The major conclusion is that these trends underscore a structural interpretation of poverty, and the relevance of a conflict theory of social stratification, rather than a functional interpretation focusing on individual and cultural factors. As such, they are squarely in the mainstream of most recent sociological writings on these issues. While the specifics of the data presented in the volume occasionally make those conclusions seem a bit too glib, the bulk of the material seems consistent with such an interpretation.

Unfortunately, the editors do not pay similar attention to other important issues raised by the contributors, perhaps because these observations do not so easily link directly to conflict theory, the major theoretical perspective emphasized. Nevertheless, the review of the six nations indicates clearly that unless there is a strong central government which can define social issues, formulate social policies, and allocate responsibilities, concern with poverty will be minimal and late and policies uncoordinated and ambiguous. This is seen most clearly when contrasting the United Kingdom and West Germany, where the central governments have played strong roles, with Belgium, where social policy responsibility has been left to local authorities and the "social partners" of employers and organized labor,

perhaps in response to regional conflicts. Italy is another example; there the dominance of the Catholic church and the intellectual preoccupations of the Communist party have resulted in lack of attention to poverty as a structural phenomenon. In Italy, more than in any of the other countries reviewed, the emphasis on religious and local institutions as desirable providers of welfare services has resulted in a welfare system so uncoordinated, fragmented, and inefficient that the American system, often described with similar adjectives, seems a model of coordination and efficiency in comparison.

Where the central government has not taken on a strong role, the result has been a remarkable absence of national data on social and economic conditions until very recently. In fact, one is struck by the dearth of such data in all of these nations, with the possible exception of the United Kingdom. One is left with a genuine appreciation for the Current Population Survey and other major data sources in the United States and for the social significance of data as a milieu for generating social policies. Without data, little attention is paid to poverty and inequality, except as a diffusion from those countries where these issues have received central concern, or as various "disinherited" groups have made claims on the state. Both of these phenomena have been crucial in attracting public attention to poverty and inequality and thus generating social policies in response.

Finally, the iron law of incrementalism is clearly demonstrated in these accounts: benefits tend to expand and in the process fill some of the existing gaps, while creating new ones.

In short, in spite of some important, and perhaps inevitable, shortcomings, this book is a valuable contribution to the field. Some of the shortcomings, such as the lack of direct comparability and standardization of presentation, also work to the advantage of the book, in that a fuller understanding of the uniqueness of the six nations becomes possible. Although the editors attempt to provide an overarching theoretical framework for the various accounts, their discussion seems unnecessarily limited to their emphasis on structural inequality. Finally, it is possibly of some concern that some of the chapters (those on the United Kingdom, Belgium, and West Germany) are written by nationals, or at least residents of these countries, whereas the remainder are not. It is also unfortunate that the editors had to abandon attempts to include accounts of the remaining Common Market nations (Denmark, Holland, Luxembourg). More important, the divergence in attention to poverty and inequality, in trends in the incidence of poverty, and most especially, the differences in the structure and impact of social policies among the six nations point to the serious difficulties the Common Market faces in forming a genuine European economic and social community.

Social Darwinism and English Thought: The Interaction between Biological and Social Theory. By Greta Jones. Atlantic Highlands, N.J.: Humanities Press, 1980. Pp. xiv+234. \$30.00.

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The title of this fine book, *Social Darwinism and English Thought*, should not convey an impression that its author's concern is to do for the history of English thought what Richard Hofstadter's work on Darwinism did for American thought. Greta Jones is aware, of course, that social Darwinism contained a defense of laissez-faire in Britain as in America, and that, in a broader sense, it provided a framework for analysis of production as intense individual competition for given resources. But much more than this was involved in social Darwinism, she reminds us, and if we are to appreciate the long-range significance of even these outer manifestations we are obliged to examine the philosophical disputes and the controversies in biological thought as the intellectual climate in which Darwinism took shape, as well as the social and political milieu that conditioned the process. There is an identifiable body of thought called social Darwinism, Jones insists, and Charles Darwin made a definite contribution to it. Yet we must look at all this in context if we are to avoid the serious mistake of supposing that social Darwinism is simply a translation of vulgar conservatism into Darwinian terminology.

The context explored by Jones is a broad spectrum of ideas, assumptions, perspectives, and heuristic devices with which Darwin and his interpreters worked. These include (1) the idea of progress; (2) the belief that the human mind has developed over time; (3) a conviction that human society and social behavior, as well as culture, are products of individual human "faculties"; and (4) the correlative notion that social and cultural changes result from intellectual changes. The basic outlook framed by these ideas has led, in Jones's interpretation, to beliefs that cultural differences through time and space are reflections of biological differences in the carrying populations, and that group or class (or sex) differences within a society are similarly explicable. Methodologically, this general perspective has oriented inquiry to seek explications of social phenomena in terms of individual (psychological) phenomena. Finally, it has produced a conviction that a naturalistic ethics is both possible and necessary.

Jones shows painstakingly and convincingly that Darwin was enthralled by these beliefs. She demonstrates that a careful reading of *The Descent of Man*, and of some of Darwin's notebooks, can teach us a great deal, not just about Darwin and social Darwinism, but about basic intellectual orientations that characterized 19th-century social theory and continue to shape contemporary thought about society. The marked contrast between *The Origin of Species* as a work of science and *The Descent of Man* as largely an expression of current opinions is nicely portrayed. Jones firmly, but

gently, makes her point that Darwin identified the content and direction of human social evolution by assuming that it was a movement that produced the characteristics of individuals in his time, his country, and his class. His conviction that all human phenomena could be accounted for by natural selection led ultimately to a belief in a natural morality. His belief that there is an organic basis to all social and cultural phenomena, combined with his acceptance of the idea that an array of cultural differences in the present could be arranged to represent a succession of evolutionary stages, produced an image of a world of unequally endowed human populations whose fates were determined biologically, and some of whom were destined to be destroyed by their superiors.

One of Jones's main objectives is to persuade sociobiologists today to acquaint themselves with the striking similarities between their guiding ideas and the ideas of those who have previously undertaken to pursue the objectives of Darwin in *The Descent*. She hopes that sociobiologists will become fully aware of both the intellectual and social implications of such ideas, and she is also quietly eager that others become aware of the implications of sociobiology itself.

Her concern is broader than this, however; the book is not a specific response to sociobiology. The "legacy" of social Darwinism is more general. Jones sees it in the general character of functionalism, in the attractiveness of the comparative method, in the chilling history of the eugenics movement, and in the persistent concept of race and the prevalence of ethnocentrism. And a recurring theme in the book is that social Darwinist beliefs in the reducibility of social to individual phenomena and in the intellectual or facultative basis of human social life have distracted attention from the material forces that shape our lives and the reality of the social structures within which such forces operate.

To show how a variety of complex ideas were absorbed from a given intellectual environment by Darwin and used in *The Descent of Man* is a formidable task in itself. To try to isolate Darwin's own contribution adds to the job. And finally to trace the complicated paths by which ideas reiterated by Darwin are taken up once more, with new terminology in different social circumstances, amid new developments in the social sciences, is the most difficult part. Jones's degree of success in such an undertaking is remarkable. Because it is such an ambitious intellectual venture within modest physical dimensions, signs of strain will no doubt be noted.

For example, to call the range of related ideas dealt with here "social Darwinism" might be at odds with common usage; and this is not just a matter of terminology. As her subtitle suggests, Jones is concerned with relations between social and biological theory that go far beyond Darwinism. She acknowledges that Darwin took substantially all the social theory of *The Descent* from his contemporaries in the human sciences; what is not made equally clear is that the basic organicism of that social theory had deep roots in European intellectual history. Jones is obviously aware of that; it is only that as she takes us through details in the work of Herbert

Spencer, Leslie Stephen, A. R. Wallace, Walter Bagehot, Francis Galton, et al., an impression is left on occasion that we are looking at a 19th-century English phenomenon that can be understood in its own terms—that is, in terms of social Darwinism.

Because the book attempts so much in so little space, there is also an irritating pattern of undeveloped allusions to important points. Jones is concerned about a pattern of assumptions in social thought that obscure our vision of real social processes of a material nature. The implication of an alternative theoretical outlook is left at little more than that, with the result that readers might be given the impression that a mere statement of the alternative should be convincing.

Criticisms of this sort, however, are not in keeping with the observation that Jones's agenda was already burdensome.

Social Darwinism and English Thought is an excellent work. It deserves careful reading at a time when there are strong signs that social science is turning once again to biology for guidance.

New Directions in Urban-Rural Migration: The Population Turnaround in Rural America. Edited by David L. Brown and John M. Wardwell. New York: Academic Press, 1980. Pp. xvii+412. \$29.50.

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Until the early 1970s, migration toward large urbanized areas had been accepted as the dominant trend in both industrialized and developing countries. The discovery in the past decade of a reversal of this pattern in the United States provoked wide speculation on dramatic social changes producing such a break with previous behavior patterns. Recent research has documented the breadth and character of the reversal, cataloging a list of contributing factors, with emphasis on the role of technical and economic development as underlying causal agents. *New Directions in Urban-Rural Migration* is a collection of 16 papers which, together, provide a wide review of recent research as well as several contributions on topics related to the migration reversal.

The first chapter, by the editors, David L. Brown and John M. Wardwell, documents the trend and provides a fairly comprehensive summary of its discovery and the literature attempting to describe and explain it. During the fifties and sixties, counties outside the nation's metropolitan areas suffered high levels of net out-migration. Sometime around 1970, such counties began to experience aggregate net in-migration. The population gain due to migration was greatest for counties adjacent to metropolitan areas, suggesting a continuation of the long-standing trend toward urban deconcentration and spread. More surprising, and indicative of a major break with previous trends, was the growth of in-migration for counties removed

from population centers. The greatest change in net migration occurred for counties of lowest density, those largely rural in character. The change is most striking because it occurred across all major regions in the United States, and is reflected in the migration patterns of all population subgroups.

Several contributions in this volume explicitly test or elucidate various explanations of the reversal in migration patterns. Wardwell, in the most comprehensive treatment, reports similar migration reversals over the past 20 years in a dozen developed countries. He provides an excellent critical review of proposed explanations for the trend in the United States, analyzing them in terms of applicability across countries experiencing the reversal. Wardwell's own synthesis attributes the reversal to improvement in communications and transportation technology, and to increased personal affluence. While other explanations focus on changes in valuations of rural and urban living, he does not believe such changes play an important part in the migration reversal. He observes correctly that the migration choice may continue to entail economic considerations, but that a convergence of rural and urban employment opportunities may permit choices to be made on the basis of preferences for residential environment. In an attempt to test the implications of the theory, Wardwell examines changes in consumption patterns and per capita income in the 12 countries. Unfortunately, no attempt is made to analyze these changes with reference to the magnitude or timing of migration trends for the particular countries, nor are any data provided for countries not experiencing the migration reversal. For this reason, the data as presented provide at best weak evidence in support of the theory.

A dominant theme in the popular press attributes the renewed rural growth to a reawakening of preferences for rural living and a greater concern for "quality of life," combined with an unprecedented willingness to forgo income to achieve these goals. Three of the chapters follow from the research on the role of preferences in the migration reversal. The chapter by Joe B. Stevens estimates pecuniary sacrifices made by migrants to obtain perceived environmental amenities, and the study by James D. Williams and David Byron McMillen examines stated reasons for movement to nonmetropolitan areas. Although results in both studies support the notion that nonmonetary interests are important for movers to nonmetropolitan areas, neither study addresses the issue of whether such preferences are new, and whether they are playing a new role in recent migration patterns. An economic analysis of differences in wages by city size (e.g., I. Hoch, "City Size Effects, Trends, and Policies," *Science* 193 [1976]: 856-63) indicates that individuals have long been willing to forgo earnings in order to live in less urbanized areas. It is possible that migrants to rural areas in the fifties and sixties had preferences similar to those of the more recent migrants. Nonetheless, the study on stated reasons for moving does make a contribution to the research on migration decision making. In contrast to the method used by the census bureau, which asks respondents simply to indicate reasons for moving, the authors distinguish between reasons for leaving and reasons for choice of destination. Although it is important not to take such stated reasons at face

value, this distinction does highlight the important difference between the event that precipitates a move and the criteria used to select a destination.

James J. Zuiches discusses the relationship between mobility expectations, actual mobility, and migration characteristics, a relationship that is of value in making migration inferences from preference data. Unfortunately, longitudinal preference data are not available, so it is not possible to use the findings of this comprehensive literature review to look at the role of preferences in the migration reversal. While the paper provides an empirical summary, I found that it had too little theoretical focus to be of much general interest.

Several chapters focus on the employment structures in rural and non-metropolitan areas. For those who hold the popular stereotype of rural employment as farming, the chapter by Calvin L. Beale provides useful corrective background. Employment in most nonmetropolitan areas is no longer dominated by agriculture, but by 1970 had grown to share many of the characteristics of employment in metropolitan areas. The chapter by Lloyd D. Bender provides a model of the impact of trends in economic structure—dispersal of basic industry and growth of services—on population and income growth in rural areas. The paper by Zuiches and Michael L. Price examines changes in the industrial composition of nonmetropolitan employment. Though limited to Michigan, this paper provides some intriguing data on movement of jobs and migrants between metropolitan and nonmetropolitan areas before and after 1970. Those who see the recent migration trend as less dependent on employment shifts than previous movements will find some support in these analyses, although the authors note that rather strong caveats apply, given the nature of their data.

The chapter by Edwin H. Carpenter provides analysis suggesting that the proportion of employed migrants who remain in nonmetropolitan counties for more than three years increased slightly between 1965 and 1972. Although this finding is an interesting one, I found the focus of the analysis unnecessarily narrow. No data are presented on changes in the size of observed employed migrant streams, nor are comparable figures on migrant retention shown for metropolitan areas.

The contribution by George H. Dailey, Jr., and Rex. R. Campbell provides a good detailed description of the migration reversal in the Ozark-Quachita uplands, a historically depressed area that began to experience moderate growth through migration in the middle sixties, foreshadowing a greater influx in the seventies. The area is typical of those for which recreational amenities are presumed to draw migrants. Their analysis shows areas with lakes and other environmental attractions to be growing fastest, providing a local replication of earlier research based on all U.S. counties.

Three studies examine the impact of new migrants on small towns, but in radically different contexts. William F. Stinner and Michael B. Toney analyze the way that individuals' characteristics and migration histories influence stated community satisfaction in eight nonmetropolitan Utah communities. Although of little relevance to the migration reversal, the findings do

provide an interesting contribution to the study of migrant satisfaction. The chapter by Steve H. Murdock, F. Larry Leistritz, and Eldon C. Schriener focuses on five western towns experiencing high in-migration due to increased development of coal resources. Comparison of the attitudes and characteristics of recent migrants and of longer-term residents indicates the social impact of this kind of rapid economic growth. Most of the nonmetropolitan growth observed in the seventies did not result from this kind of resource exploitation, however, and Louis A. Ploch's study of four moderately growing Maine towns may provide more insight into the impact of the migration reversal. The study examines the social and political interaction of newcomers with native residents. Ploch's observations on the impact of increased in-migration suggests that changes of a cultural and social nature in these newly growing communities may be as important as the economic and demographic ones.

Included are a chapter that discusses the public policy impact of the migration reversal, and a description of a software package for analysis of county data. The chapter on use of administrative records for statistical analysis is a useful introduction and will be of particular interest for those considering using employment location data collected by the Social Security Administration.

While the range of issues addressed relating to the migration reversal is wide, this volume is not a text, and so covers relevant literature only as it relates to the particular issues addressed by each author. While almost all important research on the trend reversal is referenced—much of it was written by the authors of chapters in this volume—it is not presented in detail. The reader who knows nothing of the recent migration research will get an introduction to the basic findings and issues but will find additional reading valuable.

Although summaries for each of the main sections attempt to provide an overall structure for the book, chapters differ so greatly in perspective and methods that the book must be seen as a collection of loosely related studies. Nevertheless, several make useful contributions to the understanding of the migration reversal, synthesizing past research in helpful ways and presenting data that are often suggestive, though seldom definitive. Those studies that are only tangentially related to the migration reversal often provide data and analysis that help to place the trend in context, although the reader who is not interested in the particular issues addressed may find the return for reading them low.

Overall, *New Directions in Urban-Rural Migration* provides enough recent data and analysis that the serious student of the migration reversal will find much of it necessary reading. Others are likely to find valuable comprehensive reviews in chapters by Wardwell and Brown, and Wardwell, and perhaps the local descriptions by Dailey and Campbell, and Ploch, but will wish to read other chapters only as they correspond to particular research interests.

Demographic Patterns in Developed Societies. Edited by R. W. Hiorns. London: Taylor & Francis, 1980. Pp. viii+208. £10.00.

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This brief volume, edited by R. W. Hiorns, is a collection of three papers on fertility in Britain, the United States, and Western Europe, one on trends in marriage and divorce, and two on mortality in Europe and Britain. It concludes with an essay on the fear of population decline in Western Europe. *Demographic Patterns in Developed Societies* was produced as a symposium by the Society for the Study of Human Biology, co-sponsored by the British Society for Population Studies. M. G. Kendall and E. Grebenik played active roles in chairing the sessions.

After a straightforward description of socioeconomic differentials in fertility in England and Wales, one is immersed in a difficult but rewarding analysis of trends in American fertility by Norman B. Ryder. His focus is on the purely demographic level of explanation and on the decomposition of movements of period fertility into those that can be attributed to movements of cohort quantum parameters and those that respond to movements of cohort tempo. The translation of period and cohort modes is a challenge that has occupied Ryder's attention and talents for several decades. One of the principal conclusions of this analysis is that most of the baby boom would have occurred without any change in the numbers of births per woman—that is, it was mainly a matter of changes in cohort tempo. This conclusion applies equally to much of the decline in fertility since the baby boom. Much of this changing tempo has been due to changes in the age at first birth. Ryder goes on to point out that tempo changes can have quantum consequences "and that the two are to some degree alternative manifestations of the same underlying behavior" (p. 44).

Van de Kaa's description of fertility trends in Western Europe is more substantive in treatment than Ryder's essentially methodological analysis. One of the more interesting observations that he offers is that the radical changes observed in Sweden and Denmark will probably not be repeated with the same strength in other Western European countries, a belief that is shared by D. A. Coleman in connection with nuptiality. Coleman's conclusion is that unfavorable economic conditions outside Scandinavia, not any radical changes in the institution of marriage, are responsible for the recent fall in marriage rates.

On the subject of mortality, Guillaume Wunsch observes that male death rates in some European countries have become higher than female death rates at all ages. This has resulted mainly from more rapid decline in female mortality. Wunsch isolates the particular diseases associated with the disadvantages of males. A more detailed treatment of the same topic for England and Wales is contributed by A. M. Adelstein and J. S. A. Ashley.

One of the most interesting essays in the book is by J. M. Winter, an

English historian who here traces the concerns about population decline in Western Europe from 1870 to the eve of World War II. The trail takes us through military anxiety, class fears, racism, nationalism, and especially the long-standing French concern about the specter of "more coffins than cradles." The not always enlightened population ideas of R. A. Fisher, Leonard Darwin, C. P. Blacker, Leonard Huxley, and others are reviewed. The more naked biological, eugenic theories—frequently focused on group differences in reproduction—gave way gradually to sociological concerns about the conservative implications of aging populations and the socialist thinking of writers such as R. H. Tawney, who saw declining fertility as a rejection of the competitive ethos of capitalism.

Although the postwar baby boom dispelled concerns about low fertility, the prewar demographic trends have returned, and once again we see European governments and observers pay increasing attention to the problems created by low fertility. The rhetoric is now greatly softened and cast in social terms, but it will undoubtedly escalate as low birthrates continue.

Facing Zero Population Growth: Reactions and Interpretations, Past and Present. By Joseph J. Spengler. Durham, N.C.: Duke University Press, 1978. Pp. xiv+288. \$18.75.

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As Joseph Spengler notes in the short preface to *Facing Zero Population Growth*, he has been interested in its subject since the 1930s. Indeed, this work can be seen as a culmination of the long and very fruitful career of one of this country's leading economist-demographers. Judged by that high standard, the book is, in spite of its several significant virtues, something of a disappointment.

It is, first of all, less a unified work than a series of essays related to three major themes—a history of economic doctrines pertaining to population growth, the relation between a stable population and its age structure, and the economic effect of a cessation of population growth. Even within each of these almost discrete parts, the argument is loosely constructed. Many of its points are taken up several times in only slightly different contexts, and the frequent repetition hardly adds to readability. Instead of the forceful statement that one would expect from a scholar of Spengler's authoritative status, the reader is given an unnecessarily complex mish-mash. The author develops his ideas only circuitously, half-hidden in a jungle of quotations from item after item in the 37-page bibliography.

A shorter list of truly significant references would have brought to the surface dramatic contrasts in the cycle of opinion on these issues. In the 1920s a near consensus in Western nations expressed concern about "standing room only," in the 1930s about an imminent "depopulation." J. M. Keynes, by a wide margin the most influential economist of the previous

generation, helped set both climates; and Spengler quotes from both sets of writings, but in passages so widely separated that the sharp reversal is blurred. In the 1930s the low birthrates were seen almost universally as disastrous, and their recurrence in the 1970s has stimulated—in Western and Eastern Europe—a corresponding pronatalist wave. In the United States, however, the frenetic campaign for zero population growth (ZPG) went from student groups to such quasi-official bodies as the Rockefeller Commission, whose numerous reports were brought forth while American fertility was falling to the lowest point in the nation's history. This contrast, too, is absent from Spengler's account.

Spengler illustrates the transition of the American population to a stable state by using a number of extrapolations denoting the age structure that would ensue with each of several possible futures. These projections are illustrative, but should one take them as realistic? At one session of the last meeting of the Population Association of America, several distinguished demographers reviewed forecasts made in the 1950s and 1960s—all of which were lamentably and even laughably off the mark. For instance, in 1949 Warren Thompson, a specialist on the Far East, held that Japan's situation was truly desperate: "A real catastrophe involving millions of persons may be in the making" ("Future Adjustments of Population to Resources in Japan," in Milbank Memorial Fund, *Modernization Programs in Relation to Human Resources and Population Problems* [New York, 1950], p. 153). This makes strange reading to accompany today's news of how the U.S. government has been prodding Japan to help revive the ailing American automobile industry. If we have learned anything from the past, it is that demographers are not privy to the future, and in particular that the recurrent predictions of disaster have all been less an indication of coming events than a symptom of current malaise.

Spengler holds that the advantages of ZPG outweigh the disadvantages, but he does not argue the position vigorously. He focuses on how the economy would react to a stable population, not to a transition to that state—which at best will take most of our lifetimes. Moreover, he assumes that the "skill and flexibility of private enterprise" will function without impediments from either the state or such agencies as trade unions and oligopolistic or monopolistic organizations (p. 155). "Since the advent of a stationary population should not prove difficult, given a *flexible* economy, the net advantages realizable should prove considerable" (p. 171). Other writers have held, perhaps more plausibly, that since capitalism is uniquely suited to economic growth, a stable economy would stimulate the rise of socialism. In any case, to hold that the power of the government, trade unions, and large corporations will wither away smoothly is to load that overworked phrase, "ceteris paribus," with every political struggle.

A stationary population would lead to a static social structure, one in which upward mobility would be a good deal more difficult. Spengler discusses intergenerational succession, quoting Nathan Keyfitz's ingenious calculation that moving from an increase of 2% annually to an increase of

zero would entail a delay of 4.5 years in the average time it takes to reach a middle position in factory or office. The key issue related to social mobility, however, Spengler passes over. Even in a growing economy it has been exceedingly difficult to effect the American credo of ethnic equality, for some upper-level whites have resented the rise of blacks, for example, to comparable positions. In a static economy, in which blacks could rise only by supplanting the whites holding high positions, opposition would be far stronger and the possibility of upward movement far less.

Within the deliberate bounds of a demographic-economic analysis, *Facing Zero Population Growth* has some of the virtues one expects from Joseph Spengler. His account of other men's views is always fair even when he patently disagrees with them. The complexity of the problem is never converted into a facile simplification, such as has become all too familiar during the past decade or so. Indeed, the cumbrous and tentative argumentation, though less elegant than one would like, reflects the author's full mastery of the issues he is analyzing, his acquaintance with the long debates about them and the variegated conclusions that one could reasonably reach.

Human Fertility in Russia since the Nineteenth Century. By Ansley J. Coale, Barbara A. Anderson, and Erna Härm. Princeton, N.J.: Princeton University Press, 1979. Pp. xxv+289. \$16.50.

Dennis P. Hogan
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This book on geographic variations in nuptiality and marital fertility in Russia from 1897 to 1970 is the fifth volume in the series of monographs resulting from the European Fertility Project. As such, *Human Fertility in Russia since the Nineteenth Century* must be evaluated in the context of that project.

The European Fertility Project was initiated in 1963 by Ansley J. Coale of the Office of Population Research, Princeton University. It represents an ambitious undertaking to utilize census, vital registration, and other manuscript data to document for small geographic areas the declines in mortality and fertility that constitute the demographic transition. The project devotes special attention to the effects of marriage patterns on fertility. Although direct evidence on the causes of declines in marital fertility is unavailable, the studies have made ingenious use of mathematical models of the age pattern of natural fertility (developed by Coale and various collaborators) to identify age-specific patterns of fertility characteristic of married populations practicing family limitation.

The turbulent history of Russia from 1897 to 1970 has produced irregularities in long-term trends in vital rates, as well as troublesome variations in data sources, accuracy, and detail. The data permit observations of marital fertility in the provinces of European Russia for the periods around

1897, 1926, 1940, 1959, and 1970. For the non-European republics, data are presented for the reference periods of 1897, 1926, 1959, and 1970. Separate estimates of marital fertility were made for rural and urban areas of the provinces and republics. The available data are subject to various errors, as the authors carefully document. Ansley J. Coale, Barbara A. Anderson, and Erna Härm corrected biases wherever possible, and they take great care in describing the adjustments made. The data production aspects of their study are impressive. Few demographic studies are as distinguished by scholarly attention to detail as those done as part of the European Fertility Project; this study of Russian fertility is no exception.

The authors report the indices of marital fertility, total fertility, and nuptiality for the geographic areas in every reference year. The geographic patterning of fertility levels and trends is communicated to the reader through elaborate computer-generated figures, as well as through conventional maps. Some rudimentary correlations and regressions are calculated in an attempt to identify social structural determinants of geographic variations in nuptiality and fertility at single points in time.

Although this study provides many new facts about fertility in Russia since 1897, it provides a limited basis for generalization about the determinants of geographic variations and trends in nuptiality and fertility. In part this results from a tendency to confuse two sources of variability in the rates: interarea variations at a single point in time and intertemporal variations within areas. For example, a regression analysis of singulate mean age at marriage of males and females in urban and rural areas of the 50 provinces of European Russia in 1897 indicates that a higher proportion literate among rural females aged 20–29 corresponds to a later age at marriage among all groups. However, since age at marriage was nearly constant (decreasing slightly from 1871 to 1908) at a time of increasing literacy, the authors conclude that literacy did not account for variations in nuptiality. (Imagine a similar analysis concluding that higher education is not associated with a later age at marriage among American women because the post-World War II declines in marriage age corresponded to a time of intercohort increases in education.) This analytic problem can be resolved through the skilled application of pooled cross-section techniques, as demonstrated by Toni Richards in her reanalysis of the German European Fertility Study data (Toni Richards, "Fertility Decline in Germany: An Econometric Appraisal," *Population Studies* 31 [November 1977]: 537–53). Probably the abilities of the authors to draw conclusions from this study would have been enhanced by the use of these more refined analytic techniques.

This book is recommended reading for demographers interested in Russian nuptiality and fertility. While it may disappoint social scientists seeking an explanation of the demographic transition in Russia, it contributes to our storehouse of information about the demographic transition. There can be little doubt that the European Fertility Project ultimately will provide the basis for a comprehensive explanation of the demographic transition in Europe.

John Milton and the English Revolution: A Study in the Sociology of Literature. By Andrew Milner. Totowa, N.J.: Barnes & Noble, 1981. Pp. viii+248. \$29.50.

Wendy Griswold
University of Chicago

Of the navigators in the sociology of literature, the late Lucien Goldmann was the most successful in avoiding the Scylla of methodological heedlessness and the Charybdis of disregard for the text itself. His mode of investigation, genetic structuralism, brought to light homologies between the structure of a literary work and the structure of consciousness of the collective subject from which it emerged. Goldmann believed that a world vision is a social, not individual, phenomenon, the result of a group's specific historical experience. A writer gifted with particular sensitivity to the mental structures of his group recreates these structures in his work. The task of the literary sociologist is, first, comprehension, finding in a text a significant structure that will account for almost all elements within the text and, second, explanation, matching the literary structure to the structure of a group's world vision.

Andrew Milner has made a commendable attempt to apply genetic structuralism to John Milton. In *John Milton and the English Revolution*, he suggests that the Independent party, with Milton as its literary spokesman, shared the mental categories of Protestant nationalism: radical individualism, the advocacy of reason free from all institutional and psychological constraints, and a logical atheism. This Independent world view, a nonpredestinarian Protestant ethic, arose from the needs of the bourgeoisie. Milner explains Milton's prose and greater poetry in terms of this world view. For example, in *Paradise Lost* Adam and Eve are individual personalities capable of moral choice, their sin consists of allowing their passion to overcome their reason, and God allows this to happen because he is not an independent actor in Milton's cosmos. The late poems are a construction of consolation, wherein the apparent triumph of passion over reason at the Restoration is resolved in rationalism. *Paradise Lost* depicts the difficulty of maintaining hope in the face of the defeat of reason, a problem with which Milton was grappling following the Restoration. In *Paradise Regained* the poet settles for quietism, victory at the personal level only. But in *Samson Agonistes*, which Milner assumes to be written shortly before its 1671 publication date when the Restoration was beginning to unravel, the triumph of reason at the political-historical level is again presented as possible. Thus Milner sees the aged Puritan activist returning to his original position.

Milner's effort to elucidate a structure that accounts for the totality of Milton's major works avoids the methodological folly, which he rightly criticizes in Christopher Hill's *Milton and the English Revolution* (New York: Viking, 1978), of ransacking literature for bits of evidence to sup-

port his case. But there are damaging historical and literary problems in Milner's own case. To contend that Milton faltered in and then recovered his commitment to Independent radical activism, Milner must assume that *Paradise Lost* was written shortly after the Restoration, *Paradise Regained* in the late 1660s, and *Samson Agonistes* last of all, shortly before 1671. But recent scholarship suggests that *Samson Agonistes* may have been written in the 1640s, and Milton had outlined a tragedy called *Paradise Lost* in about 1640. Thus the germs of the poems may have preceded the Independents themselves. Furthermore, the Independents were hardly a cohesive social group. They were at the most a Parliamentary party born in the 1640s during the disputes between the moderates (Presbyterians) and extremists (Independents). By the end of the decade the Independents themselves had split along moderate/radical lines, with the radicals supporting Pride's Purge and constituting the Rump Parliament (1649–53). The Rump itself, to Cromwell's disgust and Milton's, turned increasingly conservative and concentrated more on the maintenance of its own power than on radical reforms. Thus the Independents were a short-lived political cluster, one which Milner admits had neither distinctive social origins nor a fixed ideology. If they did not differ from the Presbyterians either socially or ideologically, how does Milner justify his claim that the Independents constitute a collective subject with its own world view? "At each successive state in the class struggles of the 1640s and 1650s, the Independent, rather than the Presbyterian, programme is that which embodies most fully the class interests of, and which expresses at the highest level the class consciousness of, the English bourgeoisie" (p. 82). This is stratification by ideological cream skimming, a most unsatisfactory business.

Even if one were to accept the view that the Independents were a coherent group with time to evolve a distinct world view of Protestant rationalism, the homologies Milner draws between Milton's poetry and this world view are shaky indeed. Milner's key dichotomy is reason versus passion. According to his reading, Milton's three major poems, written in the context of "reason embattled," the seeming defeat of reason with the Restoration, continue to maintain that individual freedom comes only when passion is subordinate to reason. Milner is trying to establish that the reason/passion conflict, with the former necessarily and ultimately triumphant, structures (1) the economic views of the rising bourgeoisie, (2) the world view of the Independents, and (3) Milton's poetry. This is why the Independents had to be an identifiable and persistent collectivity, historical evidence to the contrary notwithstanding: they are the missing link between the world-historical context and the literary product. But even if the ascendancy of reason over passion were part of the ideology of the rising bourgeoisie—and Weber has suggested the irrational roots of such rationality—it will not do as the structural linchpin of Milton's poetry. Submission to God's providence is Milton's central theme. Both understanding and bliss are allowed (Milton broke with tradition in suggesting that Adam and Eve

enjoyed sexual relations before the Fall), but only with the circumscription of God's laws. Lust for fruit or knowledge was not the original sin; breaking the law was. Raphael's warning to Adam was explicit:

Be strong, live happy, and love, but first of all
Him whom to love is to obey, and keep
His great command; take heed lest Passion sway
Thy judgment to do aught, which else free Will
Would not admit; . . .

[*Paradise Lost*, bk. 8, lines 633-37]

Earlier, Raphael had admonished Adam not to ask too many questions about the workings of the universe, for such things are rather to be admired than fully understood. Reason is submission to divine providence, not the acquisition of knowledge leading to calculation. Such submission, wherein lies true freedom, can lead either to tranquillity of mind or the ability to bring the temple down on the Philistines, as *Paradise Regained* and *Samson Agonistes* respectively show. Milton was indeed a Protestant rationalist, but the accent was on Protestant.

Milner's religious unmusicality makes him not only misconstrue the meaning of Milton's reason but misread the optimism expressed. Milton was wrestling, not simply with the historical problem of how the Protestant revolution could have failed, but also with the theological one of theodicy. How God could have let his saints fail in the Good Old Cause was simply one historical manifestation of the old problem, If God is good, why do human beings suffer? Any answer to the theodicy question is bound to be optimistic, for it flies in the face of empirical evidence. Milner concludes that "the mediating agency between present defeat and ultimate victory, clearly embodied in the symbol of Samson's 'great deed,' is, of course, moral and political activism" (p. 194). Nonsense! The mediating agency for Milton as a believing Christian was Christ's intercession with God on behalf of human beings. In the context of Christ's redemption, neither the fall of the Puritans nor the Fall of Adam was absolute. To one who accepted God's providence and Christ's mediation, obedience is optimism. Milton's advice was, do not worry or despair, all will work out without human assistance. "They also serve who only stand and wait."

Milner has not given us a persuasive demonstration of the genetic structuralist method. To draw a homology using the world view of a transitory social group on the one hand, and a reading of a great religious poet that ignores religion on the other, is perverse. John Milton sought "to justify the ways of God to men." Andrew Milner sought to justify the ways of Milton to sociologists. Both efforts are praiseworthy, but Milton's was the more successful.

Yearning for Yesterday: A Sociology of Nostalgia. By Fred Davis. New York: Free Press, 1979. Pp. xii+146. \$10.00.

Tom Panelas
University of Chicago

It is generally agreed that the 1970s were a particularly nostalgic time in American life. But if there was anything special about the nostalgia of the seventies, it was not the number of people that succumbed to its spell so much as the newly collectivized manner of its observance. The commercialization of nostalgia—made possible now by the passage of several decades in which the principal markers of time were the changing genres of the mass media and fashion industries—meant that recollections of the most intimate sort could be evoked simultaneously in millions by the mere regurgitation of the culture industries' file footage. Nostalgia had always been a rather solitary pastime; now it was possible to indulge ourselves in the company of many others.

Fred Davis's *Yearning for Yesterday* is the first full-length treatment of nostalgia by a sociologist, and it is a welcome study indeed. While it might seem mischievous to draw the analogy, a sociology of nostalgia is needed now for much the same reason that a sociology of suicide was needed in Durkheim's day: to explain, in Davis's words, "that nostalgia, despite its private, sometimes intensely felt personal character, is a deeply social emotion as well" (p. vii). Davis makes the argument convincingly, and in its course there emerges a sense of respect and appreciation for an emotional condition commonly stigmatized as ephemeral, self-indulgent, and conservative. Far from being trivial, sociological interest in nostalgia is justified by the latter's central role in the formation and maintenance of the self, the management of status passages throughout the life cycle, and even the entire society's sense of collective identity.

Drawing on interviews he conducted and a miscellany of cultural texts such as news articles, comic strips, films, and music, Davis sketches the lineaments of nostalgic experience, differentiating firmly and precisely between what he believes it is and is not. In doing so, he assembles many of the ideas that might constitute elements of a more general theory of social memory.

Nostalgia itself, however, is not ordinary memory; it is a particular form of recollection, distinguished from others by the "special past" which it possesses, one in which pleasant experiences are elevated to prominence and more painful memories screened out. But although nostalgia draws from the past, it is quite clearly a product of the present. It is always evoked in the context of current fears and anxieties, and looks to alleviate those fears by "using the past in specially reconstructed ways." Nostalgia's influence may be the most positive at those transitional points in life when anxiety is felt about one's performance in a new role, or when one is disappointed, guilty, or shamed. By recalling previous trials in which one displayed com-

petence and integrity, nostalgia "cultivates appreciative stances toward former selves" (p. 35), emphasizes continuity between the past and present, and helps to restore confidence.

In an analogous way, Davis notes that nostalgia may also contribute to the maintenance of solidarity in whole societies undergoing "untoward historic events" such as wars, assassinations, or depressions. The strains produced by these situations typically elicit historical imagery which highlights the collective fortitude exhibited by citizens during previous crises. Nostalgia can thus help to restore confidence to the societal self in ways quite similar to its function for the individual personality. This parallelism between the individual and collective levels, incidentally, resurrects an unresolved debate staged decades ago between Frederick C. Bartlett and Durkheim's protégé Maurice Halbwachs over the nature of collective memory. Stated simply, the controversy centers on whether or not such a thing as group memory—conceived as something other than the aggregate of individual memories—may be said to exist. Ultimately this issue goes to the heart of Durkheimian method and doctrine, and its resolution is crucial for any theory of social memory.

The dignity that Davis wishes to confer on nostalgic experience impels him to probe its nuances and demonstrate its commensurability with other human experiences. Cleverly adapting the work of Alfred Schutz—although at points Mead would have done as well—Davis differentiates three separate "orders" of nostalgic experience—*Simple*, *Reflexive*, and *Interpreted*—distinguished from one another by the degree of reflexivity that each entails. Nostalgia is not always passive recollection, but meets with varying degrees of scrutiny from subjects at different times. Individuals engage in cognitive sparring with nostalgic affect—challenging the latter to prove the veracity of its images and claims about the past—often to the point where the probity of the entire experience is called into question. The delineation of these separate "orders," it should be stressed, is not intended merely to supply convenient analytic abstractions. The value of this typology emerged inductively from the material collected in Davis's interviews and denotes distinct evaluative postures which subjects adopt toward their nostalgic feelings at different times. Each one is amply illustrated with excerpts from interviews and other material.

Davis also insists on precision in using the term nostalgia. Properly invoked, the concept applies only to memories of lived experience; it must therefore be distinguished from what Davis terms "antiquarian feeling," the latter being longings for a prebiographical, legendary past that one knows only through representations in folklore, history books, films, etc. One cannot be truly nostalgic for places one has never seen or events that one did not live through. Fair enough, but left this way the discussion fails to acknowledge the *similarities* between the two kinds of experience. It ignores the extent to which the prebiographical past may be made to feel eminently personal, and also the fact that lived experience may be selectively reconstructed in ways that resemble the selective reconstruction of the distant

past. Cultural practices, rituals, and representations create powerful collective archetypes which put the individual in close emotional contact with her or his cultural history and evoke feelings of attachment to these periods which may be experienced as vividly personal. At the same time, culture can re-present personal experiences in so many different ways that autobiography and history may both come to be seen through a similar retrospective lens, one which refracts, distorts, and magnifies in accordance with current circumstances and the prevailing mode of socially constructing the past. The differences remain important, of course, and Davis is quite correct to distinguish between personal history and that mediated by legends and historical texts. But the sharpness and conviction with which the distinction is drawn here may, I fear, overstate the differences and foreclose discussion of a conceptual problem important for the sociological study of memory.

Yearning for Yesterday belongs to that delightful tradition of sociological writing of which Lewis Coser has recently said, "acuteness of perception, stylistic grace and humane significance are joined in an endeavor to bring at once enlightenment and pleasure" (Lewis Coser, ed., *The Pleasures of Sociology* [New York: New American Library, 1980], p. xi). It is interpretive sociology at its very best. Davis says what he has to say precisely, but without depleting the experiences he describes or the nuances that give them life and make them special. He uses language's supple faculties to their fullest in conveying the irony and ambivalence of feelings, an engaging tack which unfortunately is anathema in sociology today. Readers interested in the sociology of aging, emotion, or memory—or in the application of phenomenological concepts to sociological analysis—will find this a very rewarding book.

Changing Images of the Family. Edited by Virginia Tufte and Barbara Meyerhoff. New Haven, Conn.: Yale University Press, 1979. Pp. ix+403. \$19.50.

John Modell
University of Minnesota

Amid the buffeting suffered of late by the Parsonian account of the rise of the modern Western family, a flourishing specialty in family history has grown up. A multidisciplinary volume, *Changing Images of the Family* would seem to be understood most charitably as a set of proposals, some grounded empirically, others more theoretical, and some simply "inspired," directed to the enrichment of the family-history enterprise.

The notion of "image" serves to bring some unity to a chaotic set of contributions. To editors Virginia Tufte and Barbara Meyerhoff, respectively a literary critic and an anthropologist, "image" denotes the expression of a sentiment that is nonnormative in form but nevertheless is value laden and possess direct emotional impact. "Images are fundamentally and inevitably rhetorical, then, persuading us of the validity of the information

they carry" (p. 9). As rhetoric, images depend on cultural forms that may change slowly. It is equally true, however, that because images also "appeal primarily to our senses, . . . arousing in us our own remembered experiences" (p. 9), they emphasize relatively recent occurrences. Linking, thus, two temporal frames of reference, the notion of the image may help us to construct a nonlinear, adequately ideational history. Although one contributor maintains that "only an artist can imaginatively construct an image in which we *participate*, through our senses, nearly directly" (pp. 161-62), the editors and many other contributors argue that the study of images is itself image making, that the volume adds to the debate over what the family should be.

Such stimulating ideas, it seems, led in 1976 and 1977 to colloquia that inspired the essays in *Changing Images of the Family*. To judge from the rather murky introduction, editorial nondirectiveness was pursued as a matter of policy, permitting many unrelated essays, many poor ones, and little cumulation. Of the 16 essays in the volume, perhaps half use the "image" notion centrally and consistently, and not all of these effectively. Among the essays not much indebted to "image," two deserve mention: Barbara Laslett's sociological speculation on the significance of changing household composition, and Stephen J. Morse's rigidly organized but insightful account of changes in American family law. If we omit these essays and the dross, the organized, coherent "action" in this volume seems to center on the proposition that a sentimentalized image of an isolated nuclear family, widely propagated, came to prevail in bourgeois settings a century and a half ago.

Until the crumbling of the *ancien régime*, Arlene Skolnick argues in her contribution, the family's behavior was under extensive public scrutiny. As society's oversight waned, "images and prescriptions for family life . . . began to be promoted by the new mass media of the nineteenth century [that] were both vague and perfectionist [and which] introduced impossible ideals that did indeed make normal family life seem flawed" (pp. 303-4), a burden recently assumed by sociologists, among others. Skolnick would prefer "lowered expectations of family life" (p. 315).

John Demos's contribution adds a bit of historical specificity to this general design, proposing, in addition, a more clearly emergent third stage. If the passing of the *ancien régime* led to "the family as refuge," the comfort and security of recent welfare capitalism has brought a new family image which emphasizes not succor but novelty, "a bubbling kettle of lively, and mutually enhancing, activity" (p. 58). Often enough, this demanding image contradicts reality, dialectically implying the antithetical image of squashing individual growth. The conflict of image produces our historically unique self-consciousness about family life.

Two satisfying iconographical essays offer some empirical support. Sylvia Manning argues that those who would read Dickens as ethnographer must consider that "in Dickens's stories there are virtually no happy, natural families" (p. 141), but instead vicious families, or temporary aggregations

blasted by urban forces. At the ends of the novels, another kind of family appears, transparently idealized, "always there, passingly glimpsed, beckoning, hardly describable" (p. 152). The sentimentalized image exists *because* it contradicts reality, novelistically described. Only in the 20th century were novelists able to omit the idealized haven at the end. Hogarth's visual assault on late 18th-century family relations also lacks the happy ending. "Hogarth's perception of the child was as an essentially parentless creature in a society which reneged upon parental responsibility within both the private nuclear family, and the public family formed by social institutions" (p. 101). David Kunzle sees Hogarth indicting urbanization.

David Stannard's essay, one of the few to engage directly with the others, is an attack on the pivotal notion of a privatized, emotionally demanding family. Stannard argues that public institutions have steadily eroded family life, robbing it of content while at the same time letting stand (indeed propagating) the image of family as emotionally overburdened. Thus self-deluded, we invite crisis.

As a concept, then, "image" might eventually bear analytic weight, viewed macroscopically from a historical perspective. Despite high points, this collection scarcely begins the task.

The Black Extended Family. By Elmer P. Martin and Joanne Mitchell Martin. Chicago: University of Chicago Press, 1978. Pp. 129. \$10.95 (cloth); \$4.50 (paper).

Walter R. Allen
University of Michigan

This important book presents one of the most extensive analyses of black American family life to appear in the contemporary literature. Motivated by a felt need to examine black families outside the confines of myopic debates over whether these families are "strong" or "weak," "pathological" or "healthy," Elmer P. Martin and Joanne Mitchell Martin have sought instead to portray black families for what they are . . . variations on the broader human theme of family relationships. Using what they call a "human perspective," the authors explored the structure, dynamics, and values of some 30 extended black families (comprising more than a thousand persons) in an attempt to understand better the essential character of black family life in the United States. Data were collected using multiple methods of unstructured interviews, group discussions, observation, and analyses of family records and personal documents. The study was predicated on the assumption that black family life is best exemplified as "a multigenerational, interdependent kinship system which is welded together by a sense of obligation to relatives; is organized around a 'family base' household; is generally guided by a 'dominant family figure'; extends across geo-

graphical boundaries to connect family units to an extended family network; and, has a built-in mutual aid system . . ." (p. 1).

The Black Extended Family represents a valuable addition to the literature on black American families. It is thoughtfully written, encompasses a wide range of issues, and provides many useful insights. Those expecting methodological rigor will be somewhat disappointed, though. For, as Martin and Martin acknowledge, this is an impressionistic study of a nonrandom set of families, many of which included personal associates of the authors. Moreover, no quantitative presentations or analyses of data are found in the book. But make no mistake, the findings are indeed representative of the broader reality of black family life; one comes away from the book with increased understanding of these families' structures and functions. Readers will be gratified by the authors' ability to capture and portray the subtle—yet telling—nuances of black family life as revealed through the value orientations, organizational patterns, and interpersonal dynamics apparent in this sample.

Like any creative work, this book is not without its shortcomings. Of greater consequence than the absence of quantitative analyses and the use of a nonrandom sample is the authors' tendency toward excessive brevity. At numerous points, key issues or dimensions are treated quite superficially. For example, chapter 6, "Sex, Courtship and Marriage," is a collection of synopsis statements about such topics as "dating," "sub-extended family absorption," "manhood," "interracial marriage," and "homosexual unions"—to name but a few. Clearly 11 pages hardly provide sufficient space for the careful, detailed attention these topics demand. Ideally, the book should have been at least a hundred pages longer. The authors would thereby have been freed to go beyond their valuable, yet greatly limited, condensations of major points or findings. Another negative consequence of brevity is the absence of extensive subject quotations, case history materials, and summaries of observations, all of which would have served to reveal the richness of Martin and Martin's data base, as well as the sources of their conclusions.

The discussions of black family history and the treatment of American scholarship (chap. 9 and the appendix) are unfortunate additions to this book. At best these two sections are superfluous; at worst they damage the book's overall credibility. Many of the representations of African societies, African family life, sex-role egalitarianism among blacks, and contemporary black religion are patently incorrect. Similarly, the critique of published work on black families overlooks numerous recently published books and articles which would have helped to make for a better-informed, more accurate commentary. As the footnotes to chapter 9 and the appendix indicate, insufficient attention was paid by the authors to the voluminous writings of the seventies on various aspects of black family life—past, present, and future.

In conclusion, *The Black Extended Family* is essential reading for students of black American—and American—family life. It is an informative,

manageable, interesting work, useful as a research and teaching tool. Of its many contributions to the literature on black families, the following seem most noteworthy. It manages to lay bare to careful scrutiny the aspirations, problems, triumphs, defeats, and daily routines of black family life. The book also introduces several concepts which acknowledge that black family life is organized around alternative, culturally distinct principles (e.g.; absorption," "family base," "sub-extended families," "stabilizer" and "organizer" family roles). Unfortunately, Martin and Martin fail to reference the writings of contemporaries like Sudarkasa, Nobles, Shimkin, McAdoo, and Aschenbrenner, who support their view that the black extended family has distinctive organizing principles and procedures. All things considered, the authors have performed a valuable service for those who seek alternative frameworks which encourage the culturally relative, systematic, and *humane* study of black families. For this they are to be applauded.

Women, Work, and Family. By Louise A. Tilly and Joan W. Scott. New York: Holt, Rinehart & Winston, 1978. Pp. xiv+274. \$9.95 (paper).

Barbara A. Anderson
Brown University

Women, Work, and Family should be read by everyone interested in changing roles of women with industrialization, in female labor force participation, in family power, or in family decision making. It would be a useful supplementary text in undergraduate or graduate courses on women, modernization, social change, work and occupations, or social history. A large and diverse literature is integrated effectively to provide a good introduction to the changing status and roles of women in England and France from the late 17th century to the present.

The book has three parts: (1) "The Family Economy in Early Modern England and France," (2) "Industrialization and the Family Wage Economy," and (3) "Toward the Family Consumer Economy." In the first part, Louise A. Tilly and Joan W. Scott discuss England and France in the late 17th and early 18th centuries. The focus is on the extent to which the work of both married and single women, whether in or out of the household, was a part of a family strategy. The research on this period tends to be based on individual villages, since national data bearing on these issues are generally not available. Thus there is inevitably generalization from scattered studies. The emphasis is on women in rural areas, although there is some discussion of women in urban areas.

In the second part, the changes in women's work with industrialization are discussed. The emphasis is on women in urban areas, although conditions in rural areas for women certainly changed greatly during the Industrial Revolution. The discussion focuses on the implications of the change from the household as a subsistence unit to the household surviving through

accumulation of money, especially through wages. Much of the single woman's wages went directly to her family, making the change to wage work relatively minor. Over time, though, single daughters increasingly gained some autonomy from their families. For married women, wage work was more difficult to coordinate with home responsibilities than home subsistence production work had been.

In the third part, the emphasis is on England and France since about 1910. The authors discuss the factors which led to decreased paid work by women for several decades. After World War II, women's labor force participation again increased.

Women, Work, and Family is not a piece of primary research. Instead, Tilly and Scott summarize and relate a large body of research. The book contains copious footnotes as well as a general bibliography. These allow the interested reader to pursue a variety of topics in more detail.

The book is accessible to undergraduates. It is well written and clear. Literary evidence is integrated with historical studies in an especially pleasing way. The excerpts from 18th- and 19th-century novels made me want to read the novels.

The accessibility of the writing style, however, could lead some readers to misinterpret the authors' meaning. When there is no generally accepted explanation of a phenomenon, Tilly and Scott usually present a plausible, perhaps the most plausible, interpretation of it. However, the reader unfamiliar with the literature in that area might gain the impression of more scholarly consensus about the explanation than actually exists.

For instance, there is a discussion of fertility decline in England and France. The causes of the timing and the differences in timing of the English and French fertility declines have been the subject of much debate. Fertility declined in France first, although England industrialized before France and a higher proportion of the English population lived in urban areas. Additionally, the general timing of the fertility declines in both countries is a question of interest, since marital fertility fell in Hungary at about the same time as in England, although Hungary was then largely illiterate and rural. There is no generally accepted explanation of the causes of these declines.

Tilly and Scott explain the declines as results of a combination of four factors: (1) a desire to maintain an increased standard of living; (2) a decline in the "economic value of children" as it became increasingly difficult to find paid work for children; (3) an increase in the "cost of children" as children stayed home longer and parents had to spend money on schooling and job-training costs; (4) a falling rate of infant and child mortality, which created financial burdens for parents trying to support a large number of surviving children. It is certainly plausible that these changes motivated fertility control, but a scholar would be in error to say that these were definitely the causes of the declines and to cite this book as the source.

Tilly and Scott are well-respected social historians, and their orientation is that of the historian, not the sociologist. The typical sociologist reading

this book will learn much about French and English history, but he or she will not find an integrated theory of the status or work roles of women according to social and economic characteristics of the woman, her family, and the society. There is no stated and seemingly no consistent implicit theoretical orientation. The explanations combine a functionalist and a Marxist approach.

My objections to this book are minor and stem from the trade-offs necessary in writing a clear and accessible book and from long-standing differences in orientation between sociologists and historians. This is definitely a book about history which sociologists should read and use.

Divorce and Separation: Context, Causes, and Consequences. Edited by George Levinger and Oliver C. Moles. New York: Basic Books, 1979. Pp. xx+363. \$15.00.

Robert Chester
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Europeans have been accustomed to regarding the United States as highly divorce prone in comparison with their own societies, but the upsurge of divorce throughout the West since the mid-1960s has indicated to them that mass divorce is a transnational phenomenon and not an American peculiarity. Thus, although this book deals only with divorce in America, it will be of interest and value to readers in many other countries.

Divorce and Separation originated in a collection of articles in the *Journal of Social Issues* (vol. 32, no. 1 [1976]), and consists of nine papers from that source (three extensively revised) plus 10 newly commissioned pieces. George Levinger and Oliver Moles have exercised good editorial control, so that this collection has a coherence often lacking in "readers," and there is a degree of interreference among the papers. The papers are grouped into sections labeled "Perspectives on Marital Dissolution," "Social and Psychological Determinants of Break-up," "Economic Determinants of Break-up," "Consequences for the Ex-Spouses," and "Consequences for Families and Children." These headings do not cover all the possible topics arising from divorce—for instance, there is no consideration of consequences for the kindred or systematic discussion of consequences for the social system or the state—but much ground is covered nevertheless, since the papers in each section deal with a variety of topics.

The papers are of different kinds, including surveys of data, reviews of literature, theoretical essays, reflective pieces, and reports of individual projects. They thus differ in level of generality, but all are of interest outside the United States because the issues they touch on are widely relevant. Part of the book's value lies in its up-to-dateness, because the pace of family change has been such that the current applicability of many earlier research

findings can no longer be assumed. By the same token, some of the more limited material presented here may also become dated somewhat rapidly, whereas the more general and theoretical pieces are likely to have a longer shelf life.

In a brief review of a collection which involves some 30 authors it is not possible to give separate attention to all the materials presented, but it is possible to draw out some themes suggested explicitly or implicitly by the material as a whole. One such theme is that the current high rates of divorce are not a passing experience or temporary aberration but are structured predictably into the social conditions of mature industrial society. The conditions of the postwar world have created new patterns of opportunity and constraint for individuals, and these have spawned new values which have challenged traditional moral orthodoxies. In many ways divorce has become normalized as an experience with which society and numerous individuals within it must learn to live, even though social traditions make this difficult to accept for many people.

A second theme, on which several papers converge, is the part played in this general phenomenon by changes in the situation and consciousness of women. In particular, changed economic opportunities—persisting sexual inequalities notwithstanding—provide women with alternatives to remaining in dissatisfying or destructive marriages. This is not to deny that most women are likely to be economically worse off after divorce, but rather to assert that their prospects are more favorable than in previous times and that the wife's economic resources or potential influence the probability of divorce.

A third theme, and again one on which a number of papers dwell or touch, is the nature of social response in the form of law and welfare policy as these are related to divorce. It is probable that the macrosocial causes of high rates of divorce are less policy-responsive than are the consequences of divorce, and both law and social policy have for some time engaged in rather ad hoc responses to rising divorce levels. To say that divorce has become normal in some Durkheimian sense is not to say that it carries no penalties or costs for those concerned. Several papers demonstrate that relative poverty and individual distress are common consequences of divorce. The question is posed, however, how far such penalties and costs are not so much *entailed* by divorce as brought about by incomplete institutionalization, failure to resolve some of the issues which divorce creates, and social reluctance to grapple fully with the relative fragility of contemporary marriage. A clear example is afforded by the relative poverty which divorce may bring, particularly to custodial mothers, poverty which reflects the continuance alongside reformed conceptions of divorce of only partially reconstructed ideas concerning the liability to maintain, together with continuing differences of opinion about the income-maintenance responsibilities of the state. Similarly, unresolved issues concerning parental rights are reflected in some of the difficulties associated with child custody and the welfare of children.

One final point which may be noted is what might be called the "reconstructionist" perspective hinted at in several papers, an approach which is skeptical of the view that divorce is necessarily traumatic or permanently damaging to the parties involved, and one which requires careful appraisal. It is one thing to show that, for example, children are more resilient than may be thought, that absence of the father has not been shown convincingly to be damaging, that divorce may be more beneficial than residence in a conflicted home, that many people successfully or more than successfully reconstruct their lives after divorce, etc. It is quite another thing to show that divorce is actually *good* for people, and there may be certain dangers in reconstructionist optimism. Sociologists have a propensity for debunking taken-for-granted public axioms and for claiming to reveal socially creative or adaptationally functional aspects of phenomena widely assumed to be problems. They are also somewhat choosy regarding the issues on which they exercise their wits in this way, so that, for instance, demonstrations of the salutary consequences of racism, poverty, or rape are rather rare, as are searchingly critical reviews of literature purporting to show the damaging consequences of such phenomena. There is at least some possibility that researchers who are unsympathetic to traditional notions about the family may be selective in the matters to which they attend and not wholly evenhanded in their critical deliberations. It is not claimed that the present authors have seriously succumbed in this way, but there are at least some hints that the possibility exists.

In summary, this book may be recommended to academic and practitioner audiences as a good representation of recent American research and thinking in several disciplines on the subject of divorce. Inevitably it does not have all the answers, but it gives a good ration of meat for the penny, and is a worthwhile purchase.

Self, Society, and Womankind: The Dialectic of Liberation. By Kathy E. Ferguson. Westport, Conn.: Greenwood Press, 1980. Pp. xii+200. \$22.95.

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Kathy E. Ferguson's recent work, *Self, Society, and Womankind*, is an ambitious and original addition to feminist scholarship. The author seeks to formulate a theory and praxis of feminist liberation by employing a symbolic-interactionist framework. In Ferguson's view, existing strategies for liberation are inadequate because they fail to come to grips with the dialectical relationship between "being" and "becoming." According to Ferguson, there is "an underlying ontological dualism in the structure of human experience. . . . We are tied to what we know . . . and to what we can imagine in the unknown . . ." (p. 11). Ferguson criticizes the concept of selfhood formulated in the work of feminist liberationists because, she

argues, none of these theorists expresses adequately this fundamental dialectical relationship. The author points out that the concept of human nature posited by a given sociopolitical theory serves to circumscribe the type of political association that the theory can envision. Ferguson contends that existing approaches to feminist liberation emphasize either the "being" or the "becoming" aspect of existence; hence, they ignore a dialectical relationship and its implications for liberation.

According to Ferguson, "liberal feminism," thematized by Betty Friedan's *The Feminine Mystique* (New York: Dell, 1963), inherits the drawbacks associated with the liberal-bourgeois tradition of viewing individuals as socially preformed and as ultimately responsible for their own circumstances. The influence of social relationships on individual action and the possibility of radical social change are thus ignored or denied. Instead, limited social reform and individual initiative constitute the means for eliminating social "injustices." In Ferguson's estimation, such solutions imply a tacit acceptance of "being" over "becoming" as a liberationist strategy.

"Marxist feminism" errs in the opposite direction. Ferguson contends that, because of its emphasis on revolutionary change, Marxism embraces "becoming" rather than "being." Marxism is, moreover, essentially a "class theory," and therefore, Ferguson believes, inadequate for an analysis of a sex-gender category: "The Marxist conceptualizes human beings as *producing* agents, not as experiencing actors, and this image will not suffice as a basis for liberation theory because of its partial nature" (p. 135). Ferguson rather misses the point here; production and experience *together* express the fundamental basis of being and consciousness from the Marxist perspective. She is correct, however, in her assertion that socialist feminism has not yet produced an adequate theory of human liberation. Although Ferguson wishes to deny the adequacy of the Marxist paradigm for an analysis of gender, she succeeds only in pointing to the deficiencies exhibited by concrete, socialist-feminist analyses. She is more on the mark in her discussion of Simone de Beauvoir's "existential perspective" (*The Second Sex* [New York: Bantam, 1952]). The author deftly points out the dilemmas that arise from Beauvoir's tendency to equate the central concepts of existential analysis—immanence and transcendence—with femininity and masculinity, respectively. She notes that (1) it is not clear that Beauvoir's generalization constitutes an empirically accurate portrayal of women's and men's lives; and (2) the implication of the analysis is that liberation lies in the direction of transcendence (hence masculinity), thereby negating positive aspects of femininity altogether. Moreover, Ferguson argues, the existential concept of the self as that which is perpetually in opposition to the other, overlooks actual, nonconflictive aspects of human interdependencies.

Having thus rejected the liberal, Marxist, and existential perspectives, Ferguson argues that a more promising approach for formulating a feminist-liberationist theory can be gleaned from the works of George Herbert Mead. From this view, the self is conceived as both social and processual: it has the capacity to view itself from the perspective of others (i.e., it is

social) and is continually in a process of becoming something other than what it is at any given moment, through continued interaction with its environment (i.e., it is processual). Meadian individuals are organically bound to, and dependent on, others for their conception of themselves. At the same time, however, individuals have the capacity for self-definitional autonomy through their ability to synthesize others' perceptions of them and to formulate unique conceptions of themselves. The self emerges against the larger backdrop of the "generalized other"—the organized attitudes shared by the group of which it is a part. The generalized other, too, is internalized by the self and is used as a means of assessing self and structuring interactions with others, but is itself subject to modification through individual inventiveness, initiative, and interaction. Thus, from this viewpoint, individual and society exist in a dialectical relationship.

Ferguson's line of reasoning seems quite promising. She locates a concept of self and of society that allows for an understanding of the being and becoming aspects of existence and thus is consistent with her initial ontological assumption. In her extensive discussions of Mead, she sharpens our understanding of the general bases of gender socialization and gender maintenance. In addition, Ferguson argues that the Meadian self can ground a liberationist strategy consisting in freedom and compassion. Freedom refers to the ability to define one's self and situation without interference (i.e., unimpeded process), while compassion refers to the ability to "appreciate" the perspective of others (i.e., deliberate and reflective sociality). Although the potential for freedom and compassion is implied by the Meadian perspective, Ferguson points out that such conditions do not exist empirically. Concrete individuals do not all have equal abilities to define themselves and their situation; some have "the ability to establish and enforce the definition of the situation in which the other participants must act [and this] radically alters the nature of the self-other interaction . . ." (p. 69). The ability to define the situation, contends Ferguson, constitutes "power" and is an important aspect of interaction overlooked by Mead.

Ferguson argues that the generalized other must be understood as reflecting and expressing power relationships. Social rules for interaction and the meaning attached to objects in the external environment (1) can be more easily defined or modified by some groups than by others, and (2) give greater interactional power to some groups than to others. According to Ferguson, this asymmetry severely limits the possibilities of self-definition for subordinate groups, and lessens the necessity for superordinate groups to assume the perspective of those they dominate. Under such conditions, neither group is liberated since neither group fulfills its human potential for freedom and compassion. By adding the power element to Mead's perspective, the author seeks to provide the tools by which empirical obstacles to human freedom can be analyzed—and removed. Unfortunately, Ferguson does not develop her concept of power to the degree of specificity necessary for attaining this ultimate goal.

In forwarding a concept of power that subsumes all superordinate-sub-

ordinate relationships, Ferguson obscures the specific dynamic of what she seeks to explain—male domination. To say, for instance, that the upper class has definitional power over the poor, without specifying the economic basis of that power, describes a result but does not explain the dynamic of class relationships. While Ferguson asserts that the asymmetry in gender relations is not merely an economic phenomenon, she does not tell us what it is. A related problem is her tendency to conflate or to confuse the power to define (i.e., to invent, name) with the power to impose upon others a preexisting definition of a situation. The ability to define reality for others is something quite distinct from the ability to impose upon others a preexisting and understood definition of a situation. Although the former type of power may lessen the necessity for superordinates to take on the perspective of subordinates, the latter type clearly does not. Contrary to Ferguson's claims, the armed robber (p. 56), for instance, must take the perspective of those upon whom he or she has imposed the definition of the situation "robbery" if he is to maintain control over this situation. It is true that certain groups are able to define and to have accepted as "correct" (i.e., accurate, appropriate) their definitions of "reality" (which is what Ferguson usually seems to mean by "power"). Once again, however, to say this is to raise the question of what gives such groups their legitimacy—that is, what is the source of their power to define?

The shortcomings of Ferguson's concept of power are most noticeable in her application of the modified Meadian perspective to the case of gender. She points out (in a way reminiscent of Beauvoir) that men have power to define women and their situations, that women tend to be more adept at taking the role of the other than are men, and that the generalized other articulates asymmetrical gender relations which are enacted by concrete individuals. Although these claims are well documented and presented in an interesting way, such observations have been made before.

On the issue of women achieving "freedom," Ferguson is vague. "After they attain legal equality (a task not yet accomplished), women should next concentrate on achieving economic independence through total and equal integration into the economy" (p. 172). Though undoubtedly good advice (but not very different from the "liberal-feminist" solution), this suggestion does not derive from Ferguson's earlier description of the nature of sex oppression. She does point out that, in addition, women must seek to define themselves independently of male interference. To do this, she states, women should be exposed to alternatives to traditional feminine-gender models so that they can construct more androgynous concepts of themselves. What she omits to note, once again, is that the widespread availability of such alternative models is largely contingent on power resources other than definitional autonomy and inventiveness.

Ferguson's work merits serious criticism because it attempts an immensely difficult and important task. Gender theory is sadly underdeveloped, and Ferguson has provided a new and original conceptual foothold for understanding the dynamics of gender socialization and gender main-

tenance. This, in itself, is no small accomplishment. Though in some ways it falls short of its goals, *Self, Society, and Womankind* nonetheless demonstrates that progress in gender theory is being made.

Mind in the Heart of Darkness: Value and Self-Identity among the Tswana of Southern Africa. By Hoyt Alverson. New Haven, Conn.: Yale University Press, 1978. Pp. xiii+209. \$16.00.

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The literary allusion implied in the rather enigmatic title of this book is more telling than might first appear. *Mind in the Heart of Darkness* is a study of the important and current issue of the role of consciousness in sociological analysis—in particular, in the analysis of rapidly transforming Third World societies such as that of the Tswana of southern Africa. Hoyt Alverson is justly critical of positivist social science depictions of such colonized peoples, which present them as mindlessly determined by material oppression. However, the terms in which he seeks to correct this bias—those of phenomenological sociology—impose a different set of constraints, possibly more insidious because they are more subtle. For the concepts and concerns of this mode of analysis are firmly rooted in tacit assumptions of our own Western epistemology, and, contrary to claims made for them, their uncritical application to other cultures often imposes our own axiomatic categories on indigenous experience.

In fact, such ethnocentric assumptions are constantly at war with the sensitive but unsystematic insights about Tswana perception offered in this book. The author relies heavily on data elicited from interviews, rather than on the observation of meanings inherent in social action. Indeed, he argues that the anthropological analysis of nonliterate societies has tended to neglect the role of "propositional," conscious knowledge (p. 186). But, while this may well be true, he seems to have erred in the opposite direction, constructing an account of Tswana "mind" from the rather literal interpretation of stated belief, and hardly acknowledging meanings *implicit* in the categories of Tswana thought and everyday life. The concepts around which he organizes most of his discussion—those of "self-identity," "horizons of doing," and "freedom of will"—express the assumptions of our own rational, individualist world view. They imply a theory of knowledge which distinguishes "action" from "cognition," and which confines the role of metaphor to the explicitly poetic domains of culture.

Now, while these might well be viable categories for some analytical purposes, they cannot be held to reflect the texture of Tswana perception without adequate support. And such support is not really provided in Alverson's account. Thus, for example, his data suggest strongly that in Tswana culture the notions of "experience" and "action" merge, as do the concepts of

"individual" and "social" being. Yet he treats Tswana perception largely in terms of disembodied reflections about individual identity. And he limits the discussion of metaphor to the subjects of praise poetry and animal fable, ignoring the resonant images of selfhood his own informants present (notions of the "witch," of "hot spirits," and of the "heart," which for Tswana is the site of both perception and emotion, and hence raises questions about the meaning of "mind" itself in this context). Alverson also fails to consider the role of ritual (both indigenous and Christian) which other ethnographers (Paurw; Setiloane) suggest is integral to the world view of many modern Tswana. Such ritual expresses a scheme of social, spiritual, and natural elements which, although it has undergone much transformation, still entails a relationship of self and context more complex than he allows. It is this scheme which is the essence of the Tswana notions of *thlogelo* ("order") and *malao* ("law"), both of which Alverson introduces but fails to develop in terms of the multifaceted cosmology they contain, and the images of personhood and being which they imply.

In short, the order of meanings and action which constitutes Tswana culture is missing from this book. By this I do not wish to imply that Alverson should have presented us with a neat "structural" account; it is to his credit that he has not. It is rather that his analysis calls for a more systematic integration of Tswana concepts and categories in their own social and historical context. In this respect, his failure to consider the implications of the rich scholarly literature on these peoples is a serious one. The reader is left with the impression that the Tswana case has been used to address certain preoccupations current in social philosophy; as a result, important questions raised by the ethnography itself concerning the relationship of consciousness and history in modern Africa remain unanswered.

This is a pity, for the author has a worthwhile point to make about the creative response of peoples like the Tswana to their historical predicament. But to illuminate this point requires insight into the social and cultural underpinnings of such modes of thought and, in particular, their existence in time. And such a project, arguably, calls for concepts and methods beyond the scope of this sort of phenomenology—even in the hands of a practitioner as sensitive as Alverson.

Identity and Authority: Exploration in the Theory of Society. Edited by Roland Robertson and Burkart Holzner. New York: St. Martin's Press, 1980. Pp. x+318. \$27.50.

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Contemporary social theory, both mainstream and critical, reflects a noticeable shift in problem area from traditional political, economic, and social justice concerns to questions concerning culture (identity) and political

power (authority). Speaking in general terms, these discussions reveal a theoretical insufficiency in two regards. First, the dominant paradigms for analyzing identity are psychological and, as such, tend to conflate identity with "naturalistic" concepts like ego, psychosexual development, or dispositional theories of personality. Second, in the main the analysis of authority has proceeded separately from the conceptualization of identity. This reflects, in part, the tendency to associate authority with a collectivist social structural realm while linking identity to an individual psychological realm. In the introduction to *Identity and Authority*, Burkart Holzner and Roland Robertson propose a far-reaching revision of the prevailing analytical discussions of identity and authority. Essentially they seek to differentiate the more naturalistic concept of personality from identity, which they perceive as a concept mediate between personality and social structure. This strategy allows them to offer a wholly phenomenological and sociological framework. Identity is viewed as an enactment achieved in the interaction between a self that is reflexive and a context which defines and constrains the self. By situating identity in an authoritative sociocultural context, the authors provide an analytical bridge between identity and authority. Holzner and Robertson provide a programmatic essay which is followed by a series of essays, at once sustaining a high analytical level and yet posing the issue of identity and authority in more concrete thematic terms.

In chapter 2 ("Logics of Selfhood and Modes of Order: Civilizational Structures for Individual Identities"), Vytautas Kavolis applies the "civilization-analytic perspective"—an approach codified by Benjamin Nelson—to an analysis of identity and authority or, in his terms, logics of selfhood and modes of order. The civilization perspective (CP), one must understand, is a very welcome reaction against individualistic, ahistorical sociologies and shallow social psychological accounts of identity which begin and end with techniques for sustaining coherent patterns of self-management. The CP seeks to penetrate to the "deep structures" or presuppositions of action and order by uncovering the operative "logics" or "rationales" or "codes." Using the CP, Kavolis discerns four logics of selfhood. One example is the notion of the true self as a unique pattern of internal coherence that is either a self-achievement, as in the popular American idea of the "self-made man," or a pattern given by some version of fate, as in Greek tragedy. This logic of selfhood may be contrasted with the Goffmanesque image of the true self as an other-directed self who is defined as the situation requires and is without a stable inner core. The aim of the CP is to generate civilizational logics that cover the range of historical experience.

Whereas Kavolis sought, it seems, a formal typology, Rainer Baum ("Authority and Identity: The Case for Evolutionary Invariance") offers a substantive analysis of authority and identity within the thematic context of the modernization debates. Baum raises the question of whether modernization necessarily entails convergence at the level of political institutions and legitimations (what Baum, in the dreary language of Parsonian action theory, calls "meaning codes of authority"). Baum believes it does

not and details the divergent "codes" of authority that underlie Western political modernization. His main point is that, once elaborated, these codes—which entail premises regarding the individual-group relationship and the part-whole relationship at the societal level—tend to endure while legal-institutional change may occur. In speaking of evolutionary invariance and continuity, Baum provides a kind of structuralist reconstruction of Parsonian action theory, but one whose theoretical formulation tends to obstruct instead of facilitate historical penetration and sociological insight.

The next three chapters address the problem of identity and, to a lesser extent, authority in contemporary America. Richard Fenn ("Religion, Identity, and Authority in the Secular Society") maintains that, in contrast to those (seemingly dwindling) theorists who project a unified American culture, present-day America has evolved two separate cultural patterns in terms of which individuals derive identity and collectivities legitimate their authority. Fenn's essay is tentative and inconclusive. John Marx ("The Ideological Construction of Post-Modern Identity Models in Contemporary Cultural Movements"), in perhaps the most penetrating essay of the collection, pursues in detail the theme of two cultures. Marx contends that America has entered a postindustrial and postmodern historical period characterized by a stable, inflexible, highly routinized and predictable social structural/institutional sphere upon which has arisen a dynamic culture oriented to the quest for meaning, identity, self-actualization, authenticity. Marx maintains that, despite the differentiation of the two spheres and their differing cultural values and logics, they are mutually reinforcing and subsist without serious tension. Somehow individuals, Marx insists, are able to compartmentalize their routinized reified lives in the institutional sphere so as to pursue self-actualization during leisure time. Departing from the disjunction theories of Fenn and Marx, Guy Swanson ("A Basis of Authority and Identity in Post-Industrial Society") argues that a new principle of societal legitimacy has crystallized: authority is no longer lodged in persons, the office, or procedural correctness but in purposes, goals, and ultimate values. The countercultural critique of the dehumanization of formal rationality and its reassertion of the primacy of value rationality in legitimating social action and order is just another aspect of the new social organizational ideology of "management by objectives" and "human relations." The same renewed concern with humanistic goals is found in the contemporary emphasis in religion on a personal and direct relation to God, religious experimentation, and innovative worship. In all these instances the appeal to legitimate action is to ultimate values and goals and, as Swanson insists, the same value constellation: individual expression, communitarianism, democratic participation. Swanson perceives in postindustrial society nothing less than the beginnings of the overcoming of "alienation" and the anticipation of Kant's "Kingdom of Ends."

Swanson's optimistic portrait of contemporary America highlights a major omission in the entire collection: the absence of a critical perspective. Wherever one turns, individualism, it seems, is flourishing. In view of the

massive assault on civil, political, and human rights mounted by the Reagan administration (not to mention the New Right critique of cultural modernism and the diminishing expectations in a coming Age of Scarcity), I find ironic the assertion of a reinvigorated culture of individualism, of a society where individuals have nothing to do but seek new identities. No attempt is made to assess critically the ideology of self-actualization or authenticity by linking it, for example, to the administrative redefinition of politics and the necessary depoliticization of the populace or connecting the search for identity to the extension of the economy into the production and marketing of personality.

The editors' integration of the analysis of identity and authority is on an abstract and formal plane because concrete issues of this sort fail to receive treatment. Their substantive analyses avoid making the kind of critically inspired linkages between the different societal realms that one finds, for example, in Daniel Bell, *The Cultural Contradictions of Capitalism* (New York: Basic, 1976) or Jürgen Habermas, *Legitimation Crisis* (Boston: Beacon, 1975). My objection, to state it in analytical terms, is that by discarding a substantive notion of human nature (or a naturalistic basis for a theory of identity), the authors, excepting Roland Robertson whose concluding essay I lack space to discuss, relinquish ties to the critical sociological heritage, including of course Weber and Durkheim, associated with the normatively charged concepts of ideology and rationalization.

The Self-Concept. Vol. 2: Theory and Research on Selected Topics. Rev. ed. By Ruth C. Wylie, in collaboration with P. J. Miller, S. S. Cowles, and A. W. Wilson. Lincoln: University of Nebraska Press, 1979. Pp. xiv+825. \$35.00.

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In 1979, *Psychological Abstracts* listed well over one thousand articles and books under the headings "self-concept," "self-esteem," "self-evaluation," and "self-perception." In 1969, in contrast, only about four hundred publications were listed under equivalent headings. If the yearly increase in self-related publications is assumed to be linear, we can calculate that at least seven thousand books and articles pertaining to the concept "self" were published in the past decade. A projection of the same linear trend leads to what one may hope is the facetious conclusion that the next decade will see some seventeen thousand writings on the same subject.

It is fair to ask who the "consumers" of all this scholarly output could possibly be. Just keeping up with the outpouring of self-related publications would seem to leave little time for putting that information to any use. Enter at this point Ruth C. Wylie's volume on self-concept. All by itself, the proliferation of theories and data about the self would justify Wylie's

comprehensive tome on the self. For amid a vast, tangled thicket of writings on the subject, Wylie has provided a sure-footed and knowledgeable guide, a way to put some order into and make some sense out of what otherwise might be all but overwhelming.

In writing *The Self Concept*, Wylie made many sound decisions. One such decision was to concentrate only on selected topics in the self-concept area. Compulsively surveying every topic ever associated with the concept of self would have left little room for the careful and thorough work Wylie did regarding the important topics she did cover. Another welcome decision was to define the concept of self that was relevant for this volume as the "phenomenal" self. Wylie's book designedly pertains only to articles purporting to study how subjects consciously feel about themselves. Inferences about "unconscious self-concept" or concept of self as seen from other persons' points of view are not included.

Even more fortunate was Wylie's choice of format: instead of simply listing relevant works in each area, or drawing general conclusions, she presents the most important studies grouped by self-concept and instrument used and further groups them under major subtopic headings. When presenting such studies, Wylie does not simply summarize their conclusions but evaluates their methodology and discusses their limitations. Less important studies, studies using fewer subjects or idiosyncratic self-concept measures or poorer research design, generally are just listed in case the reader desires to inspect them. As if this vast amount of work were not enough, for each topic covered Wylie also summarizes the theoretical issues that provide the backdrop for the empirical studies and summarizes the state of the art, at this time, regarding self-concept and its relationship to the particular area surveyed.

Wylie's work, in short, is indispensable for a student of self-concept. Unless, of course, that student is prepared to replicate the vast amount of effort Wylie and her collaborators have expended in reading and digesting articles in the self-concept area. With this book on hand, the student or investigator can learn quickly about contemporary work on the relationship between self-concept and such areas as race, gender, age, family, and psychotherapy. By using the excellent indexes Wylie has provided, the reader can find out about the work of any one of hundreds of authors or read about work done using any one of dozens of self-concept measures. An 86-page bibliography gives the reader access to a listing of many of the best works in the self-concept area.

As valuable is the fact that the reader of this volume can, by heeding what Wylie says, implicitly learn the most important dos and don'ts of self-concept (and most other) research. When Wylie criticizes the lack of generalizability of a sample, or the lack of reliability of an instrument, or the failure to control for SES, or the adding together of what might be orthogonal scores, for example, she is in effect warning future researchers not to make the same mistakes. Similarly, by modeling Wylie's approach, the reader can learn how to be a sophisticated consumer of self-concept

research. As a case in point, when Wylie observes that for most self-concept instruments random responding will tend to lower (that is, tend to produce poorer) self-concept scores, she is telling us to be wary of phenomenological interpretations of certain intergroup differences. For with this piece of information in hand, we might well construe what purports to be a group's poor self-concept as that group's having responded less carefully to the self-concept measure employed. That Wylie, however, has been to some extent a lone voice of reason unheeded by the multitude is indicated by her conclusion (p. 700).

From one perspective, the yield from the many examples of research considered in this book is disappointing and ambiguous. The extremely high incidence of avoidable methodological flaws is especially dismaying. This lament is particularly notable because this revised edition comes 18 years after the first one. For those interested, however, the information is here, to be heeded and used.

I part company from Wylie in only two respects. The more important is a theoretical, almost philosophical, issue. As she stated in volume 1 of this work (p. 328), Wylie finds self-concept useful only in the context of a predictive behavioral science.

If self-concept has little influence on individual differences in behavior, this approach to personality should be abandoned.

For Wylie, the self is the phenomenal self, the subjectively experienced self. Yet she seems to imply that it is important only insofar as it leads to behavioral differences. One imagines that for her, unless self-concept measurements enhance the observer-scientist's ability to predict, they have no value. If this is the stand Wylie is taking, I feel she has lost sight of the importance of the human subject. As she herself explains, the reason most "objective" antecedent conditions such as gender or race have failed to correlate with measured self-esteem is that persons select to a marked extent how they react to these conditions. If I am a member of a disvalued group, I may feel good that I am what I am *despite* that fact. Or I may feel I am a particularly good member of that disvalued group. If the proactive subject's constructions of self are critical in determining self-concept, why should not that subject's goals be important in determining the worth of the measurements of the concept itself? If, for example, the subject finds the measurement useful or enlightening, or if the measurement helps a subject to control his own behavior more effectively, why should we not say that the measurement is useful or valid? One goal of science may be to map relationships between objective phenomena over time, to induce lawful relationships among objective phenomena. But phenomenology addresses itself specifically to knowledge of another *person's* state of mind. Therefore it need not be limited to inducing intervening variables used only to facilitate prediction from the observer's point of view. Perhaps communication with another person, listening in a more structured way to how they feel about themselves, is a worthy goal per se, as is enhancing subjects'

ability to choose their own course in defiance of "objective" predictions. Predicting subjects' behavior may be a separate, if equally justifiable, goal.

A far lesser flaw, in my opinion, is Wylie's decision not to include dissertations in her review. This decision is understandable given the vast amount of published material. But one suspects that some of the best work in this field is contained in unpublished dissertations, done laboriously under the tutelage of dissertation committees and graduate school standards. Perhaps, though, this work is for another book and another time. Perhaps Wylie could find the time and energy to do it.

In summary, Wylie's revised volume 2 of *The Self-Concept* is an invaluable addition to inquiry in this area. Students and researchers alike will find it indispensable. It is not a book to be read straight through. It is an invaluable reference work, to be kept on a shelf and repeatedly consulted, a guide through a veritable entanglement of other contributions to the self-concept area.

Human Encounters in the Social World. By Aron Gurwitch. Pittsburgh: Duquesne University Press, 1979. Pp. xxviii+203. \$15.00.

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Written in 1931 but discovered in 1973, this book is Aron Gurwitch's first and is his primary contribution to social psychology and sociology. *Human Encounters in the Social World* is divided into three parts, which may be characterized loosely as addressing the following issues: knowing the mental life of other persons; which types of "object" in the natural world we and other persons are; and the basic forms of being together in the social world. Since the third part is of most direct interest to sociologists I will emphasize it in this brief review.

Gurwitch's book will be of interest to scholars concerned with this period of development of phenomenological social theory, since he discusses and revises works published in the 1920s by Scheler and Schmalenbach. In addition, the author appraises Brentano, Lipps, and Dilthey, and applies to sociological issues the ideas of Heidegger and Husserl. For those who care more about the substantive sociological questions addressed than about the history of theory, I suspect the work will prove a disappointment. The fundamental problems are twofold: Gurwitch's conclusions are commonplace in sociological theory; and the evidence for them, on phenomenological grounds, is unconvincing.

Consider some of Gurwitch's conclusions in part 3: "One *belongs* to the community in the sense that one *has* always *belonged* to it, *has* lived in it at all times, *has grown together* with it, i.e., *has grown into it*" (p. 124); ". . . the being together in the dimension of community is therefore not a being together of individuals as individuals" (p. 130); "Whenever people

are seized by a new 'idea,' a new feeling of life, a god, a hero, etc., and find themselves together as seized by them, then a new social formation arises among them" (p. 137). A difficulty in reading Gurwitch's intricate arguments for such conclusions is that other sociological theorists have established these principles in more useful ways, either because they embed them in full theoretical systems or because they argue in terms of practical sociological problems. When reading Gurwitch's discussion, one thinks of Durkheim, Simmel, and Weber at the very least, but in his text Gurwitch brings up only Weber (and then only with regard to charisma), instead quoting extensively from Scheler and Tönnies.

The more basic problem, *within* phenomenological theory, is that Gurwitch does not accomplish what phenomenological theorizing is uniquely designed to do, namely, the demonstration that features or processes are *essential* (roughly: necessary and immutable). Gurwitch only gives examples, or quotes other authors, to support his conclusions. In a phenomenological inquiry an eidetic, transcendental, or other reduction is displayed, in which one shows that the phenomenon at issue could not be otherwise. Although some might argue that Gurwitch is more in the mold of "lifeworld phenomenologists" such as Schutz, among whom essentialism does not play a role, clearly that is not Gurwitch's own goal. Instead, he writes of his own work, "We take up these phenomena vis-à-vis the attitude of the 'phenomenological epoche'. We exhibit, describe, and analyze them, try to develop their meaning for those involved, inquire into the extent to which these modes of awareness are determined for the manner of existence" (p. 145). But there are no data from others' ways of seeing, nor evidence for the determinateness of the phenomena. Although in the passage just quoted he allies himself (through a footnote) with Husserl's method in *Ideas*, that method is not utilized in the analysis.

Nevertheless, sections within part 3 are insightful (e.g., "The Charisma as the Phenomenon of the Beginning"). Some of the arguments are also fruitful for consideration within other theoretical systems, most notably ethnomethodology. For instance, Gurwitch argues that we understand others' expressions by putting them in a total situational context, and that understanding of another always involves taking account of the particular role of the other in the concrete situation (pp. 111-15). Indeed, his largest argument in part 3—that the three primary modes of social being are partnership, membership, and fusion—deserves broad consideration.

In some ways, however, the first two parts, though far from most sociologists' interests, constitute the most impressive scholarship. In part 1 Gurwitch builds upon the works of Lipps, Scheler, and Cassirer to consider the possibility of knowing the mental processes of other persons, these others viewed as social beings (i.e., a variation on the "problem of other minds"). He attacks those who hold that information about others is limited to physical data such as their movements. Heralding Husserlian phenomenological inquiry as a route to additional data (pp. 8-9), Gurwitch proceeds to show that "only when the fellow human being is already presupposed

as human (and *not* as automaton) does it make any sense at all to believe in or doubt the specific mental processes he is undergoing" (p. 17). Overt behavior is seen as "called into existence" by mental life (pp. 18-26), and if we investigate our own perceptions we find "moments in which the mental life of someone else appears" (p. 32).

Part 2 concerns Husserl's concept of "natural attitude" (which is so important in the independent work of Schutz) and the place of other persons within the natural world. Gurwitsch argues that we view others in concrete social contexts, not as lone objects of nature. His goal is to extend Heidegger's "radicalization" of Husserl's analysis, and to integrate this phenomenological perspective with Gestalt theory (p. 38). Gurwitsch adopts a relativistic and antirealist position, that "perception is not related to objective physical things existing in themselves" (p. 62), and that instead we view anything in the world within its milieu (i.e., its local, meaningful context). A detailed discussion is presented of studies of brain-injured patients, in order to suggest, by contrast, that normal persons are "always determined by the situation . . . in hammering I am the 'hammerer'; to be a hammerer is the sense of my concrete being in this situation" (p. 77). Here Gurwitsch goes too far, however, since history is involved, both in the form of one's biography and in the social structures giving rise to any present situation.

For the specialist in phenomenological social theory, this book represents a significant period piece. On the other hand, for sociologists with general interests in this area, contemporary works in social phenomenology would probably prove more useful.

Social Exchange in Developing Relationships. Edited by Robert L. Burgess and Ted L. Huston. New York: Academic Press, 1979. Pp. xxii+422. \$24.00.

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This book contains 13 chapters covering various perspectives on the development of social relationships. The social exchange approach is given primacy and the authors are differentially successful at integrating exchange notions with other approaches like evolutionary theory, equity theory, personality theory, and certain perceptual and developmental theories. *Social Exchange in Developing Relationships*, edited by Robert L. Burgess and Ted L. Huston, consists of two main sections. Part 2, with five chapters, presents the basic ideas behind the exchange model on different aspects of relationship development. Part 3, with six chapters, moves beyond the exchange model to include discussions of other perspectives. The first chapter (pt. 1) provides an introduction to exchange theory and models of development. In the "epilogue" (pt. 4) James Wiggins discusses the methodological complexities of longitudinal research of the type required for tests of "de-

developmental" theories. In order not to "produce premature closure" no summary chapter is included. Because of the extent of overlap in many of these chapters, however, a summary would have provided the needed integration of the contributions, and closure of this type would not have been "premature."

The chapters in part 2 are ordered in terms of the temporal process of relationship development: from initiation (chap. 2 by E. Berscheid and W. Graziano) to the development of behavioral interdependence and the movement from exploration through expansion of the relationship to commitment (chap. 3 by J. Scanzoni). E. Hatfield, M. K. Utne, and J. Traupmann discuss equity theory as it applies to intimate relations (chap. 4) and examine, among other things, the link between inequity and relationship stability. H. B. Braiker and H. H. Kelley (chap. 5) present a detailed exploratory analysis of the role of conflict in close relationships and its potential consequences. Finally, G. Levinger (chap. 6) reviews his exchange-based model of relationship progression and focuses attention on factors which predict dissolution or termination at the various stages of a relationship. The chapters by Hatfield et al., Scanzoni, and Levinger serve primarily as reviews of work the authors have published elsewhere. The chapters by Berscheid and Graziano and by Braiker and Kelley present interesting new perspectives and material on relationship initiation and the role and functions of conflict in relationships. This section of the book is generally a nice summary of the exchange-based research on the course of relationship development.

Some of the chapters in part 3 are more successful than others in providing useful theoretical synthesis. The chapter by R. C. Carson, for example, relates personality theory to the exchange perspective. Personality, he argues, is expected to have the greatest impact on exchange processes in less structured, less formal relationships involving transactions primarily of love and status. He argues further that need complementarity is linked directly to resource exchange in such relationships. Chapter 10, by R. M. Lerner, integrates notions derived from developmental psychology with social exchange theory. Lerner stresses his "dynamic interactional" concept of development emphasizing the importance of behavioral interchange for personal development and functioning. As he argues, "developing attributes of the person need to be considered in attempting to understand the nature of exchanges in developing social relationships" (p. 301).

Richard Alexander links recent developments in biological evolutionary theory to exchange-based explanations of behavior. The most complex task is specification of the nature of the relationship between genes and behavior, he argues. Alexander begins work on this task by focusing on the genetic "determinants" of altruism and "social donorism." He assesses the overlap between evolutionary theory and exchange conceptions of reciprocity and altruism. Interestingly, this discussion brings back to the forefront exchange analyses of family and kinship relations. Chapter 8, by C. A. Ridley and A. W. Avery, specifies how social networks influence

dyadic relationships, an important contribution to exchange theory. This chapter could have been strengthened, however, by a more complete consideration of recent developments in social network analysis within sociology.

Less informative for exchange theorists is the chapter by D. C. Kimmel describing a life-span developmental approach. The "developmental vectors" of those involved in social relationships, it is argued, have seldom been considered. While this criticism is valid, the author fails to indicate how inclusion of such vectors in an exchange perspective could broaden the theory. Instead, the exchange framework is eschewed (for reasons not altogether convincing) in favor of a symbolic-interactionist approach defined by Kimmel as more consonant with the life-span developmental perspective. The earlier chapters by Carson and Lerner present a clear contrast to this argument. The chapter by J. F. D'Augelli and A. R. D'Augelli presents a cognitive developmental approach to sexual involvement. Although it is acknowledged that sexual behaviors can be viewed in exchange terms, the chapter includes few references to exchange notions. However, the dispositional concept—levels or types of relationship reasoning, which ranges from egoistic to dyadic to the highest level, interactive—clearly links to similar conceptions introduced by Scanzoni and by Hatfield et al. earlier in the volume. It is fitting that in a book on intimate relations a chapter on sexual behavior be included.

This volume is a major contribution to the field of interpersonal relationships even though its focus is limited to relations both intimate, like dating and marriage, and typically heterosexual. The strength of the book lies in its interdisciplinary character, its primary weakness in the failure of some chapters to provide real synthesis. But this may be primarily a comment on the "state of the art." Finally, despite numerous discussions throughout the text of the applicability of "crass economic reward-cost terms" to the analysis of intimate relations, dyed-in-the-wool romantics and role theorists are apt to find this volume wanting.

Social Exchange: Advances in Theory and Research. Edited by Kenneth J. Gergen, Martin S. Greenberg, and Richard H. Willis. New York: Plenum Press, 1980. Pp. xviii+306. \$24.50.

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Social Exchange portrays itself as a forum for the "new voices" of exchange theory's third decade and as a catalyst for cross-disciplinary fertilization. The "voices," mostly psychologists, speak on an impressive array of topics: "theoretical extensions," including treatments of indebtedness (M. Greenberg), equity (G. Leventhal), uniqueness (H. Fromkin and C. R. Snyder), and resource classes (E. Foa and U. Foa); applications in such "specialized settings" as leadership (E. Hollander), organizational

analysis (W. Nord), sex roles (R. Willis and I. Frieze), and pair relationships (G. Levinger and L. R. Heusmann); "critical analysis" focusing on structural-motivational distinctions (H. Befu), balance (F. Pryor and N. Graburn), the generality of operant principles (B. Schwartz), and the universality of exchange principles (K. Gergen). In terms of its claims, the book is a mixed success; I begin by considering its weaknesses, then return to some brighter spots.

Consider first claims to novelty and cross-fertilization of this collection edited by Kenneth Gergen, Martin Greenberg, and Richard Willis. While some of the contributors are new voices (for me, at least), they appear to be singing old songs. For example, both Greenberg and Leventhal point out that behavioral measures of reactions to perceived inequity may tap other motives masquerading in the clothing of fairness (pp. 14-15, 52-53). Yet neither mentions the well-established "self-presentation" and "strategic accommodation" interpretations of reactions to overpayment (see, e.g., E. Van Avermaet, G. McClintock, and J. Moskowitz, "Alternative Approaches to Equity: Dissonance Reduction, Pro-Social Motivation and Strategic Accommodation," *European Journal of Social Psychology*, vol. 8 [1978]). Although well designed, Greenberg's weaving of attribution principles into a theory of indebtedness sidesteps parallel efforts in social psychology (see the chapters on attribution in R. L. Cohen and J. Greenberg, *Equity and Justice in Social Behavior* [New York: Academic Press, 1981], and in G. Mikula, *Justice and Social Interaction* [New York: Springer-Verlag, 1980]).

As a catalyst for cross-fertilization, the book does well in presenting anthropological perspectives (although exchange network analysis in anthropology and sociology is not represented). Only one chapter is coauthored by an economist (Pryor); no sociologists are among the new voices. Indeed, sociologists may find the stray comments about their discipline somewhat disconcerting. To take but one example, Nord suggests (p. 126) that the study of "irrational" elements such as norms in interorganizational exchange is the province of sociological analysis; but this flies in the face of A. Heath's discussion of the rational origins and effects of norms. Also, Marxists should be prepared for a strong dose of voluntarism and idealism, served up by, among others, Hollander, Greenberg, and Fromkin and Snyder; Marxist critiques of this aspect of exchange theory are not considered (see esp. W. P. Archibald, *Social Psychology as Political Economy* [Toronto: McGraw-Hill, 1978]). Echoing concerns expressed by Befu (chap. 8) and Schwartz (chap. 11), sociologists may question the generalizability of results based on laboratory experiments with undergraduate subjects, on which half of the book's chapters rely rather heavily.

Aside from mixed success in achieving its claims, the book suffers from other weaknesses. Perhaps foremost among these is unevenness in the quality of exposition. Contributors to edited volumes sometimes make the mistake of trying to abstract too much of their work into too little space. When this is done by leaving out evidence (Foa and Foa), the reader is

left high and dry; if too much is included without adequate exposition (Willis and Frieze), the reader is left gasping and groggy. Edited volumes also may suffer from a lack of integration, and this one is no exception. Section introductions provide useful summaries of various debates within exchange theory, yet fail convincingly to link these comments with the chapters that follow.

In the spirit of equity, I must say that much of the book is well written. And in spite of the problems of unevenness, virtually every chapter offers intriguing ideas. I particularly enjoyed the "critical" section. Pryor and Graburn's data on gift and visit exchanges in an Inuit community are fascinating, and the other chapters in the section are most stimulating. It is here that Gergen has the last word, expressing his doubts about the "transhistorical generalizability" of exchange theory. Historical explanation has proved a lively topic in the philosophy of science, and should be taken seriously by sociologists concerned with the limitations of their theories.

In short, this is an uneven book whose claims to novelty and disciplinary cross-fertilization can be questioned. A more representative airing of sociological voices would have helped to realize these claims.

A Critical Survey of Sociolinguistics: Theory and Application. By Norbert Dittmar. Translated by Peter Sand, Pieter A. M. Seuren, and Kevin Whiteley. New York: St. Martin's Press, 1977. Pp. x+307. \$19.95.

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In this book Norbert Dittmar presents a detailed review and critique of sociolinguistic theory and research which he feels has direct relevance to social problems (e.g., social and educational policies in the United States). In so doing, he offers a critical, Marxist reaction to both the work of B. Bernstein (which is referred to as a deficit approach) and that of W. Labov and others (which is termed the variability or difference approach). In fact, he argues that both approaches reflect bourgeois ideology and are destined to fail in "their avowed aim to eliminate social inequality" (p. 2). Before pursuing a discussion of Dittmar's Marxist critique, it is important to consider two general problems from which *A Critical Survey of Sociolinguistics* suffers.

First, the book is already out of date. This problem is not entirely the fault of the author, since the book was originally published in German in 1973. However, much happened in applied sociolinguistics and general theoretical work on discourse processes in the four years between the original and the translation. The problem is even more acute given the rapid growth of research in sociolinguistics over the past few years. Much that Dittmar reviews, especially in his critique of Bernstein and deficit theory, has been covered in detail in previously published articles and edited books. As a result, the utility of Dittmar's work as a source book is greatly reduced.

Perhaps the translation would have best consisted of a truncated version of chapters 1 through 6 of the original and a more developed presentation of the Marxist critique in chapter 7.

A second problem is the detail in which material is reviewed. Dittmar seemed to think it was necessary to discuss at length almost every published article in the area of applied sociolinguistics. For example, he spends a whole page discussing a 1955 publication by Shatzman and Strauss. Although this is one of the earliest articles on language and social class, it can hardly be considered a major contribution to sociolinguistic research. Furthermore, Dittmar seldom merely summarizes the main points of an article or monograph, but often lists all findings (sometimes with original tables and graphs) as well as interpretations and conclusions. Such detail is often distracting. In chapter 2 the review of Bernstein and deficit theory goes on for over 40 pages; the critique of the research in deficit theory (chap. 3) covers 20 more pages. Much of the discussion is repetitious and unnecessary. A similar pattern continues in the review of the variability approach in chapters 4 through 6.

Although often as repetitive as his review of Bernstein in chapters 2 and 3, Dittmar's discussion of Labov and the variability concept is valuable to sociologists, most of whom have only limited knowledge of linguistic theory. Dittmar is aware that Labov's development of variable rules must be understood in the context of linguistic theory and methodology. He notes that Labov's work is critical of Chomsky's competence-performance distinction in that Labov attempts to build social contextual factors into grammatical rules. As a result, Labov's work has been criticized by generative grammarians for a number of reasons, most especially his reliance on an inductive methodology and a quantitative (probabilistic) analysis of actual speech as opposed to the deductive and intuitionistic method of generative grammarians. Dittmar's discussion of these points is clear and shows that Labov's work is important for both its specific refutation of deficit theory and its more general contributions to sociolinguistic theory.

Dittmar's most original contribution in this book is his Marxist critique of much of American sociolinguistics in chapter 7. Unfortunately, as he himself notes, his ideas here are incomplete and need "a much deeper and fuller treatment" (p. v). He argues that the "sociolinguistic inquiries that have been made into the linguistic problems of Black and other ghetto children have the same fundamental aim as compensatory programmes: to adapt Blacks and other ethnic minorities to the social conditions that repress them with the bait of 'upward social mobility,' and to integrate them into capitalist society" (p. 248). As a result, sociolinguistic approaches, including both the deficit program Dittmar assigns to Bernstein and the variability position of Labov, "amount to a correction of the symptoms and do not address the basic fabric of social inequality and elitist conditions" (p. 248).

Although Dittmar's criticism of deficit theory is well taken, his placement of Bernstein squarely in the deficit camp is debatable. Furthermore, one

could argue that language programs suggested by Labov prepare lower-class black children to confront the dominant group more directly. In American society, political discourse depends upon use of the dominant language code. Minority group members can acquire the dominant code—while maintaining their own language style and values—as a means for entering political discourse and pushing for major social change. However, entering political discourse is no easy task regardless of prior training and education, and Dittmar's warnings of co-optation are well taken. On the other hand, the recent dismantling of bilingual programs while compensatory education remains intact clearly signals the present administration's preference of deficit theory to the variability approach of Labov and other sociolinguists.

Catharsis in Healing, Ritual, and Drama. By T. J. Scheff. Berkeley and Los Angeles: University of California Press, 1980. Pp. xvi+262. \$12.95 (cloth); \$3.95 (paper).

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In an important sense sociology came into its own with the rationalization of Western society and flourished more systematically in Protestant countries. One result of this embeddedness in the passionless world of the people who mistook civility for civilization and paraded affective neutrality as the hallmark of modernity was the neglect of the study of emotions. Indeed, that study was banished as an essential ingredient in any consideration of the emergence of act, self, and meaning.

In recent years, however, a number of scholars have tried to develop a sociology of emotions, and Thomas Scheff has been foremost among them. In *Catharsis in Healing, Ritual, and Drama*, he undertakes not only to present a theory of emotions but also to relate emotions to a theory of psychiatric "illness" and its cure. He begins with an overview of the sociology of emotions and the theory of catharsis, examines various scientific theories of emotion, and then applies his own view to certain selected areas such as ritual, mass entertainment, children's games, and drama.

His theory of emotions and catharsis seems founded on a hydraulic view of the human self and a literal interpretation of the word "catharsis." The curative process is said to lie in the release of accumulated emotions in prescribed ways. Catharsis itself is defined as "the discharge of the distressful emotions . . . as largely internal, involuntary processes with invariant external indicators, such as weeping, shivering, cold sweating and so on" (p. 50). The capacity of games and dramas to induce catharsis, Scheff argues, lies in their being constructed in order to elicit "aesthetic distance." The concept of "Distancing" is conceived as "the extent to which the person's *attention* is not taken up by the return of repressed emotion from past events" (p. 59; my emphasis). Distancing can be "under" or "over,"

both of which are said to be unsatisfactory for purposes of catharsis, or it can be "aesthetic," which is said to be suitable. "At aesthetic distance, the members of the audience became emotionally involved in the drama, but not to the point where they forget that they are also observers" (p. 59). Such a distancing is said to relieve emotional distress. "When the individual's attention is exactly divided between past distress and present safety . . . repression is lifted and catharsis can occur" (p. 60).

Scheff's description of the cathartic process itself is as follows: (1) the arrangement of the stimuli, which are optimally distant; (2) the participant's response; (3) catharsis; (4) decrease in tension. However, it appears that when Scheff's own view of catharsis as a process of discharge with definite physiological manifestations is applied to most of the activities which lead to catharsis, it faces some difficulty. "At the time of writing, there is no evidence that this is the case. Furthermore, the peek-a-boo game fits the theory even less well" (p. 133).

The temporal and social structures of the cathartic process as Scheff describes them can be applied to selves and circumstances that are found in therapeutic encounters as well as in everyday situations. In such a view, one must conceive emotions as not merely occasional processes, but standard and inextricable parts of all social acts, though by the nature of the case some acts are pronouncedly more expressive and emotional than others. It could therefore be concluded that those who are forced to go to a therapist in order to experience catharsis are those who did not or could not take advantage of available opportunities in everyday life. Extending the logic of Scheff's own discussion of children's games and drama, one can perhaps say that the use of obscenities and curses, swearing, and perhaps even singing can be cathartic in its effects—not to speak of writing. Scheff's own list of emotions—grief, intense fear, moderate fear, embarrassment, rage, anger, boredom, physical injury or tension—mixes both occasionally felt emotions (grief, intense fear) with those that are everyday occurrences. While admittedly grief and terror need extraordinary techniques for release and extreme forms of expression, it can be argued that the others are handled with mundane methods. Indeed, when one contemplates the social scene, one is overwhelmed with the ubiquity of the theater of catharsis.

Once Scheff comes this far, as he does in the variety of the cathartic media that he investigates and the varied nature of the responses they are able to mobilize from the participants, he must admit that catharsis, while often accompanied and perhaps even signaled and made effective by physiological manifestations, occurs primarily as a symbolic process. In fact, had Scheff begun his work with a discussion of catharsis in drama, games, poetry, and fiction and then proceeded to a consideration of its parallels in therapeutic encounters, he might well have discovered the essentially *dramatic* nature of the cathartic process. Weeping, laughing, etc., will then seem to be the final events in a series in which symbolic definitions and processes of identification are essential stages. Such events will then become performances rather than discharges. Once that is granted we can include

a number of other instances of the cathartic process: the contentment that results from seeing a play or entertainment in which the villain is punished and/or the hero is vindicated; the fulfillment that comes from having participated vicariously in the activities of a winning football team; the satisfaction that comes from having picked the right horse, etc.

My reservations notwithstanding, Scheff's work is an important, insightful, and pioneering contribution, rendered throughout with caution and careful attention to logic and evidence and a willingness to admit even the occasional weak spots in his theory. I trust this is merely the first stage of an argument that will eventually incorporate material from traditional cultures, where in fact therapy and theater are very often one and the same.

Manufacturing the News. By Mark Fishman. Austin: University of Texas Press, 1980. Pp. 180. \$11.95.

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In *Manufacturing the News*, Mark Fishman, a young sociologist at City University of New York, examines the ideological character of news, not in the trivial sense of looking for bias in the news and—eureka!—finding it, but more fundamentally, by tracing the origins of news content to the means and organization of news production.

His basic thesis follows a path marked out by others: that the social location of reporters—their beats—and their standard operating procedures for detecting newsworthy occurrences, assigning meaning and validity to them, and transcribing them into stories are crucial for explaining news content.

His findings replicate those of earlier studies: that newsmakers organize their beats around public bureaucracies and routinely disseminate information which officials formally transmit to them through documents, public hearings, and statements. News-manufacturing operations, in turn, rest on an economic logic. Short of staff, with a "newshole" to fill every day, newsmakers must gather and transcribe stories under pressure of deadlines. To do so, they must rely on the accounts of others, and public bureaucracies are organized to provide a number of such accounts in a form which is readily usable by reporters.

Fishman bases his conclusions on seven months' work as a courthouse reporter in a California town of 75,000 and a short stint observing the news desk on the town's daily, as well as its coverage of police court and county government.

He breaks new ground in showing not only that reporters' exposure to events is largely a function of their access to public officials, but also that their interpretive schema—what he calls their "phase structures"—are also

bureaucratic in origin. In choosing which events to report, which to ignore, and when in the course of a continuing story to file a news item, the reporters that Fishman observed had, in effect, adopted official categories of time and order. While crime and punishment were continuing phenomena, they surfaced as newsworthy epiphenomena only at discrete points in a bureaucratic process: arrest, arraignment, trial, and sentencing; at other times they were not deemed newsworthy. The criminal process inside the bureaucracy is itself a continuous one—trials take place over a period of weeks, so does incarceration, and perhaps plea bargaining—but newsmakers report only the “fact” of actual disposition, ignoring other facts of ongoing bureaucratic activity and recording only the formalities. Indeed, the crimes are themselves reported only as they are logged into police records.

Similarly, Fishman finds, bureaucratic accounts were self-validating, while accounts from unofficial sources were either regarded as doubtful and ignored, or treated as tips to be pursued further with more authoritative official sources.

Finally, the idealized picture of bureaucratic operations which reporters carried around in their heads became the basis for detecting inconsistencies and irregularities in bureaucratic accounts and for filling in missing information.

As a consequence, Fishman argues, “making a coherent news story out of bureaucratic proceedings in this way renders matters which violate or challenge bureaucratic idealizations invisible in newspapers” (p. 135). Yet it does not necessarily follow that, as Fishman claims, routine news “ends up legitimating institutions of social control by disseminating to the public institutional rationales as facts of the world” (p. 138).

No doubt that is the prevailing tendency, but there are counteracting factors which Fishman ignores. First, whether or not news content legitimates institutions of social control depends on the predispositions that the audience brings to the news and its exposure to alternative sources of information.

Second, looking only at the police court and county government beats, Fishman was likely to find bureaucratic phase structures in the ascendancy. Yet these typifications are not the only ones journalists use. They have a variety of standard story lines on which they draw in constructing news. Some story lines, far from bureaucratic in origin, are drawn from a muck-raking tradition quite antithetical to the dominant bureaucratic world view. Exposés of corrupt and incompetent officials, albeit not as commonplace as routine news emanating from the bureaucracy, are by no means invisible, and provide some corrective to the dominant run of the news.

Finally, Fishman sees the economic logic underlying news gathering: the effort to reduce labor costs. Pure profit maximization, however, would lead most newspapers to cut staff even further. That they do not do so suggests that a professional logic operates along with an economic one, counteracting it somewhat and pushing newsmakers to extend coverage beyond official sources.

Subculture: The Meaning of Style. By Dick Hebdige. New York: Methuen & Co., 1979. Pp. viii+195. \$13.50 (cloth); \$6.50 (paper).

Judith Adler

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In his general editor's preface Terence Hawkes introduces *Subculture: The Meaning of Style* as part of a series entitled "New Accents," whose purpose is to extend the boundaries of traditional literary studies by widening their focus and drawing upon work in other disciplines. The open spaces of border country promise freedom of intellectual movement, but they also present risks which this work, if evaluated as sociology, does not successfully surmount.

Despite its suggestively general title, the book focuses principally upon the dress and music of Britain's Mods, Rockers, Skinheads, Teddy Boys, Rastas, and Punks, and interprets their styles as ritualized expressions of subversive intent. In his eclectic use of concepts from Antonio Gramsci, Louis Althusser, Jacques Lacan, Roland Barthes, and Umberto Eco, Dick Hebdige joins current efforts of the British Left to refine Marxist analyses of culture through recourse to continental schools of structuralism and semiotics. The results of this wide reading are 45 pages of references, bibliography, and further suggested reading, a straightforward and promising 19-page account of the development of research on culture and society in Britain, and 131 pages of analysis of the various youth styles. Hebdige urges that each style be understood in relation to the social class experience of its creators, the culture of their parents, the black immigrant presence, and the mass media. Thus, contrasts in Mod and Rocker dress are interpreted as reflections of a different relationship to the labor market, the dandyism of the Bowie cult as a rejection of the working-class puritanism of the parent culture, Punk hair styles as signs of identification with West Indian Rastas, etc. The sensitivity and merit of this analysis lies in its recognition that, like other aesthetic movements, Skinheads, Punks, and similar groups are absorbed with their own stylistic history and the activity of passing commentary upon one another.

While Hebdige's readers will undoubtedly learn something about the history of Britain's youth cultures, they may also find some passages obscure, the focus somewhat vague, and, for those unfamiliar with the British scene, too much knowledge taken for granted. Though the author provides no account of his methodology, his interpretations appear to be based upon newspaper accounts, journalistic articles, and youth magazines, without the further support of participant observation, biographical studies, interviews, or demographic data. The extent, the sex and age composition, and even the geographical locale of the subcultures discussed are never given even speculative attention. "Are these styles as widespread as they are newsworthy?" we might ask. "Do they all originate in London? Are they limited to Britain? Is it not possible that the author is in reality looking at the occu-

pational culture of musicians when he assumes he is examining youth and class culture?" In the absence of more detailed inquiry, Hebdige too often is content with suggesting links between cultural style and vaguely intimated social conditions, such as alienation—a feature which has long characterized "sociological" literary criticism not based on social research.

Despite the contemporaneity of his textual references, the author appears at times to adopt a quaint perspective on cultural phenomena. By opening his discussion of Punk with a two-page account of the weather at the time of its debut, he appears to offer a meteorological explanation for the emergence of a social movement. Though sociology, like other humanistic arts, may involve storytelling, the conventions of folk storytelling traditions should not be allowed to intrude upon its analysis, and a good sociological story is unlikely to begin: "The British summer of 1976 was extraordinarily hot and dry: there were no recorded precedents" (p. 23).

The book's dominant theme—that youth styles represent "semiotic guerilla warfare" (p. 105)—is never argued against possible doubt, nor does the author try to explain why the impulse to subversion should take aesthetic rather than any other form. To persuade us to see refusal in novel alterations of the codes governing dress and demeanor, Hebdige opens his text with an illustration from Jean Genet's *The Thief's Journal* and closes it with a summary of Genet's introduction to *Soledad Brother: The Prison Letters of George Jackson*. But such support for his interpretation of British youth culture is rhetorical at best, and begs the stubborn question, Is the context which shapes a prisoner's communications literally comparable to the life situation of a British teenager? The prison metaphor, if shared by youth, might be pertinent to an interpretation of their styles; but as a personal association, it serves only to identify the author rather than his subjects with a traditional Romantic image. In fact, Hebdige concludes in true Romantic tradition with a dramatization of his own marginality and isolation as critic.

The call to treat the social world as a book and its exploration as a "reading" has a long history predating today's structuralism and semiotics. The past uses of this figure of speech were antischolastic, however, recommending empirical experience to counteract fanciful distortions of reality and overreliance on authoritative writers. The present work, on the other hand, while committed to a reading of nonliterary communications, exhibits a disproportionate attachment to written texts, both as sources of data and as keys to their interpretation. Intellectuals will always be tempted to use the cultural productions of others as allegories and illustrations of their own favored myths. The Song of Solomon can be read as a relation of Christ's love for the Church, or Punk dress as a challenge to the cultural hegemony of the dominant classes. Successful symbolic creation lends itself easily to such uses, for this, after all, is its purpose. But as practitioners of an interpretative discipline, sociologists must work to explicate other people's mythologies rather than reveal their own, and must be on guard against identifications which risk blurring the distinction.

Literary San Francisco. By Lawrence Ferlinghetti and Nancy J. Peters. San Francisco: City Lights Books and Harper & Row, 1980. Pp. xi+253. \$15.95 (cloth); \$9.95 (paper).

Albert Bergesen
University of Arizona

That strange mixture of radical politics, Eastern spirituality, and alternative life-styles makes San Francisco seem unique. Compared with the rest of the country it is; compared with its own history the present is not very different from the past. The history of Bay Area cultural life is one of eccentrics and mavericks, anarchist politics, concerns with nature and ecology, and a continuing engagement with Eastern thought. Muckraking, for instance, has a long Bay Area tradition, from Mark Twain, Upton Sinclair, Lincoln Steffens, Frank Norris, and Jack London through the exposés of *Ramparts Magazine* and other civil rights and antiwar publications of the 1960s. Nor did colorful bohemian poets first appear with the so-called Beat poets of the 1950s. Earlier there was Joaquin Miller, who, "like an outlandish dime-novel hero, dressed in sealskin cloak, sombrero, boots, and spurs. . . . He reputedly smoked three cigars at once, galloped on all fours, and bit the ankle of a Victorian lady in a Mayfair drawing room" (p. 54). He would feel comfortable in any period of San Francisco literary history.

The city's literati seem to be something of a mixture of home-grown talent and imported writers, which would include everyone from the early Spaniards through those who came with the gold fever, to the influx of Allen Ginsberg, Jack Kerouac, Gregory Corso, and Lawrence Ferlinghetti in the 1950s. Others were born here and went on to fame elsewhere, like Isadora Duncan, Alice B. Toklas, and Robert Frost, the latter living in the city until he was 11 and then moving east to his adopted New England.

Lawrence Ferlinghetti and Nancy J. Peters say there was no literary history of San Francisco prior to theirs, and their remedy is a set of one- or two-column sketches of the major figures from the earliest times through the 1970s. They also include a snapshot, when available, which is nice. *Literary San Francisco* is an enjoyable book to read, filled with interesting facts and trivia about literary life in the Bay Area.

The culture of San Francisco seems largely shaped by being at the end of the continent. Its boom-town atmosphere makes it something of a haven for those escaping more established areas. This certainly was true of the days of the gold rush and the 1950s poetry revival. By the 1960s, though, the position of the Bay Area, and California in general, was changing, reflecting the broad shift in population and power from the older Northeast to the expanding Sun Belt. By the 1970s California took on a new centrality in the national culture. If in the past the Bay Area was a minor eddy in the larger swirl of American cultural life, its importance was now growing, and interestingly enough becoming threatening to East Coast intellectuals.

No one wants to say it, but when you think about the dominant themes of East Coast intellectuals over the past 10 years they are largely negative and reactive; mostly against, not for; mostly defending old standards rather than reaching for new ones; and mostly reifying modernist experiments of the past rather than venturing into the unknown. In short, they are overreacting to the various political and countercultural experiments in California. In a very real sense the East Coast heirs to the once brash New York intellectuals have become the genteel Boston Brahmins of our time, worried about everything from the exhaustion of the modernist experiment in culture to the purported dangers the new culture of intimacy poses to the central values of Western civilization itself.

What Daniel Bell, Christopher Lasch, or Richard Sennett, to take some of the principals of this trend, share is a fear, even paranoia, about various California-instigated experiments in personal growth and consciousness expansion. Somehow the purported narcissism, hedonism, and "me decade" orientation of the "laid back" Californian is part of an *Awareness Trap*, according to Edwin Schur, or represents the "tyrannies of intimacy" responsible for the *Fall of Public Man*, according to Richard Sennett. In their imagination the danger of the awareness movement assumes gigantic proportions, constituting a *Culture of Narcissism*, according to Christopher Lasch, and even contributing to the *Cultural Contradictions of Capitalism*, according to Daniel Bell.

But what is this danger to the modernist sensibility in the arts, urban culture, and even capitalism itself? Hot tubs? Having a good cry in some therapy group? Talking about your feelings? Sharing some emotions? Think about it. If these are the real dangers to Western civilization, what do these people think of dangers like depression and war, let alone nuclear annihilation?

If we assume for a moment that explorations in personal growth are not subverting the core values of Western civilization, then just what is bothering these people? Why are such serious writers so defensive about modernism, urban culture, and the Western work ethic, and so threatened by such insignificant activity coming from California? Let me suggest that their defensiveness is tied to the transition in power from East to West coast, and the shift in the center of American cultural life from New York to California. What we have been witnessing for the past decade is a repetition of Richard Hofstadter's process of status protest, except that this time the Northeast is being economically and culturally displaced by the Sun Belt. As a result of this transition, East Coast intellectuals are defensively upholding yesterday's avant-garde as today's middle-brow standards, and being fastidiously preoccupied with the inflated dangers of this or that California experiment in personal relations.

The analogy with Boston aesthetes is a good one, for the process here is quite similar to what happened a century ago, when we changed from an agrarian and rural to an urban and industrial nation. Then old cultural centers gave way to new ones, Boston to New York. With this transition

the New England mind became increasingly genteel, losing its vitality and becoming sterile compared with the vigor and creativity of New York. Today we are witnessing the same process. New York is giving way to California. The New York mind is in decline—exhausted, and hiding behind past glories as a protective shield to cover the lack of new ideas, as it substitutes criticism for creativity and genteel modernism for avant-garde vigor. If there is a fate worse than the “exhaustion of modernism” it is its transformation into a snobbish gentility. The defensive reaction of Boston and New England did not stop the rise of New York, and it seems doubtful that all the huffing and puffing about what is going on in California, or clinging to the modernist advances of the past, will prevent the rise of California or stop the declining cultural significance of the Northeast.

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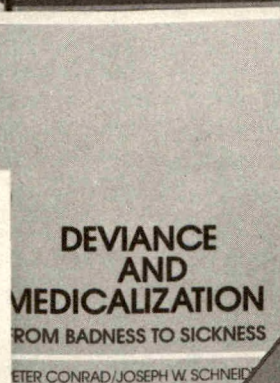
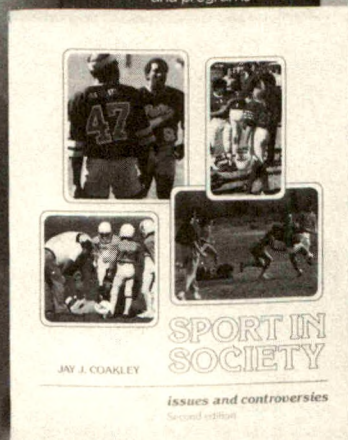
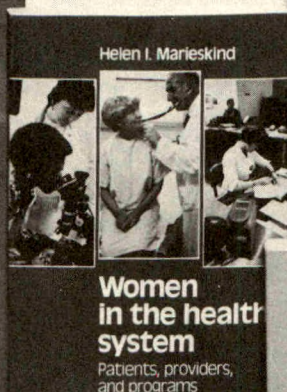
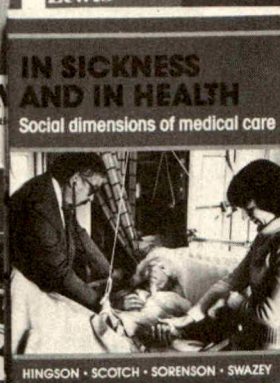
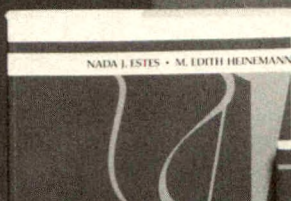
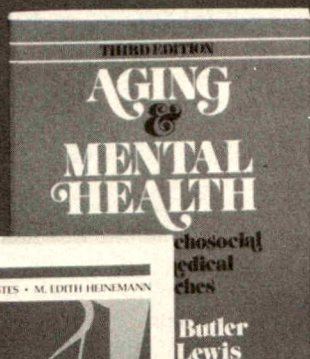
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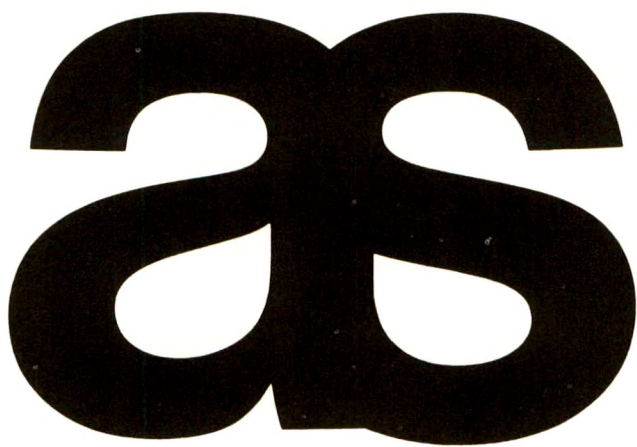
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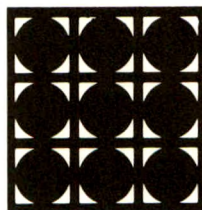
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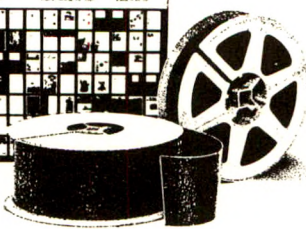
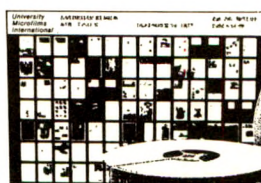
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